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Number 61 of a series of photographs of past presidents of the ~~the~~ Association.



Arthur J. Jones



The American Economic Review

VOLUME L

MARCH 1960

NUMBER ONE

PROGRESS TOWARDS ECONOMIC STABILITY*

By ARTHUR F. BURNS

The American people have of late been more conscious of the business cycle, more sensitive to every wrinkle of economic curves, more alert to the possible need for contracyclical action on the part of government, than ever before in our history. Minor changes of employment or of productivity or of the price level, which in an earlier generation would have gone unnoticed, are nowadays followed closely by laymen as well as experts. This sensitivity to the phenomena of recession and inflation is a symptom of an increased public awareness of both the need for and the attainability of economic progress. It is precisely because so much of current industrial and governmental practice can be better in the future that our meetings this year are focused on the broad problem of improving the performance of the American economy. However, as we go about the task of appraisal and criticism, it will be well to discipline our impatience for reform. In the measure that we avoid exaggerating our nation's failures or understanding its successes, we shall make it easier for ourselves as well as for economists in other countries to see current needs and developments in a just perspective.

It is a fact of the highest importance, I think, that although our economy continues to be swayed by the business cycle, its impact on the lives and fortunes of individuals has been substantially reduced in our generation. More than twenty-five years have elapsed since we last experienced a financial panic or a deep depression of production and employment. Over twenty years have elapsed since we last had a severe business recession. Between the end of the second world war and the present, we have experienced four recessions, but each was a relatively

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mild setback. Since 1937 we have had five recessions, the longest of which lasted only 13 months. There is no parallel for such a sequence of mild—or such a sequence of brief—contractions, at least during the past hundred years in our own country.

Nor is this all. The character of the business cycle itself appears to have changed, apart from the intensity of its over-all movement. We usually think of the business cycle as a sustained advance of production, employment, incomes, consumption, and prices, followed by a sustained contraction, which in time gives way to a renewed advance of aggregate activity beyond the highest levels previously reached. We realize that changes in the price level occasionally outrun changes in production, that employment is apt to fluctuate less than production, and that consumption will fluctuate still less; but we nevertheless think of their movements as being roughly parallel. This concept of the business cycle has always been something of a simplification. For example, during the early decades of the nineteenth century, when agriculture dominated our national economy, occasional declines in the physical volume of production, whether large or small, had little effect on the number of jobs and sometimes had slight influence even on the flow of money incomes. As agriculture diminished in importance, the nation's production, employment, personal income, consumption, and price level fell more closely into step with one another and thus justified our thinking of them as moving in a rough parallelism. In recent years, however, and especially since the second world war, the relations among these movements have become much looser.

The structure of an economy inevitably leaves its stamp on the character of its fluctuations. In our generation the structure of the American economy has changed profoundly, partly as a result of deliberate economic policies, partly as a result of unplanned developments. In considering problems of the future, we can proceed more surely by recognizing the changes in economic organization which already appear to have done much to blunt the impact of business cycles.

I

In the early decades of the nineteenth century the typical American worker operated his own farm or found scope for his energy on the family farm. Governmental activities were very limited. What there was of industry and commerce was largely conducted through small firms run by capitalist-employers. Corporations were rare and virtually confined to banking and transportation. As the population grew and capital became more abundant, individual enterprise expanded vigorously but corporate enterprise expanded still more. An increasing part of the nation's business therefore came under the rule of corporations.

By 1929, the output of corporate businesses was already almost twice as large as the output of individual proprietorship and partnerships. The gap has widened appreciably since then. Corporate profits have therefore tended to increase faster than the incomes earned by proprietors who still remain very numerous in farming, retail trade, and the professions. Fifty years ago the total income of proprietors was perhaps two and a half times as large as the combined sum of corporate profits and the compensation of corporate officers. By 1957 this corporate aggregate exceeded by a fourth the income of all proprietors and by two-thirds the income of proprietors outside of farming.

The great growth of corporations in recent decades has occurred preponderantly in industries where the firm must operate on a large scale to be efficient and therefore must assemble capital from many sources. But a corporation whose stock is held publicly and widely has a life of its own, apart from that of its owners, and will rarely distribute profits at the same rate as they are being earned. While profits normally respond quickly and sharply to a change in sales and production, the behavior of dividends is tempered by business judgment. In practice, dividends tend to move sluggishly and over a much narrower range than profits. Corporations have therefore come to function increasingly as a buffer between the fluctuations of production and the flow of income to individuals. In earlier times the lag of dividends was largely a result of the time-consuming character of corporate procedures. More recently, the advantages of a stable dividend—especially its bearing on a firm's financial reputation—have gained increasing recognition from business managers. Meanwhile, modern trends of taxation have stimulated corporations to rely more heavily on retained profits and less on new stock issues for their equity funds, and this development in turn has facilitated the pursuit of stable dividend policies. Thus the evolution of corporate practice, as well as the growth of corporate enterprise itself, has served to reduce the influence of a cyclical decline of production and profits on the flow of income to individuals.

The expansion and the means of financing of governmental enterprise, especially since the 1930's, have had a similar effect. The increasing complexity of modern life, a larger concept of the proper function of government, and the mounting requirements of national defense have resulted in sharp increases of governmental spending. Fifty years ago the combined expenditure of federal, state, and local governments was about 7 per cent of the dollar volume of the nation's total output. Governmental expenditures rose to 10 per cent of total output in 1929 and to 26 per cent in 1957. This huge expansion of governmental enterprise naturally led to increases in tax rates and to an energetic search for new sources of revenue. In time, taxes came to be imposed on estates.

gifts, employment, sales, and—most important of all—on the incomes of both corporations and individuals. Fifty years ago customs duties still yielded about half of the total revenue of the federal government, and none of our governmental units as yet collected any tax on incomes. Twenty years later, personal and corporate income taxes were already the mainstay of federal finance. Subsequently, the activities of the federal government increased much faster than local activities and taxes followed suit. By 1957 the income tax accounted for nearly 70 per cent of federal revenue, 8 per cent of state and local revenue, and a little over half of the combined revenue of our various governmental units.

This dominance of the income tax in current governmental finance, together with the recent shift of tax collection toward a pay-as-you-go basis, has measurably enlarged the government's participation in the shifting fortunes of the private economy. During the nineteenth century, taxes were not only a much smaller factor in the economy, but such short-run elasticity as there was in tax revenues derived almost entirely from customs duties. Hence, when production fell off and private incomes diminished, the accompanying change in governmental revenues was usually small. In recent years, however, governmental revenues have become very sensitive to fluctuations of business conditions. When corporate profits decline by, say, a billion dollars, the federal government will collect under existing law about a half billion less from corporations. When individual incomes decline by a billion, the federal government may be expected to collect about \$150 million less from individuals. State income taxes accentuate these effects. In short, when a recession occurs, our current tax system requires the government to reduce rather promptly and substantially the amount of money that it withdraws from the private economy for its own use. The result is that the income from production which corporations and individuals have at their disposal declines much less than does the national income.

Moreover, the operations of government are now so organized that the flow of personal income from production is bolstered during a recession by increased payments of unemployment insurance benefits. Unemployment insurance was established on a national basis in 1935, and the protection of workers against the hazards of unemployment has increased since then. Not all employees are as yet covered by unemployment insurance and the benefits, besides, are often inadequate to provide for essentials. Nevertheless, there has been a gradual improvement in the ability of families to get along decently even when the main breadwinner is temporarily unemployed. At present, over 80 per cent of those who work for a wage or salary are covered by unemployment insurance, in contrast to 70 per cent in 1940. The period over

which benefits can be paid to an unemployed worker has become longer and the typical weekly benefit has risen in greater proportion than the cost of living. Furthermore, arrangements have recently been concluded in several major industries whereby benefits to the unemployed are supplemented from private sources.

Other parts of the vast system of social security that we have devised since the 1930's have also served to support the flow of personal income at times when business activity is declining. Payments made to retired workers kept increasing during each recession of the postwar period. The reason is partly that workers handicapped by old age or physical disability experience greater difficulty at such times in keeping their jobs or finding new ones and therefore apply for pensions in somewhat larger numbers. Another factor has been the intermittent liberalization of statutory benefits. But the most important reason for the steady increase of old-age pensions is the maturing of the social security system. In 1940, only 7 per cent of people of age 65 and over were eligible for benefits from the old-age insurance trust fund, in contrast to 23 per cent in 1948 and 69 per cent in 1958. The trend of other public pension programs and the various public assistance programs has also been upward. Between 1929 and 1957 the social security and related benefits paid out by our various governmental units rose from 1 per cent of total personal income to 6 per cent. In 1933, with the economy at a catastrophically low level, these benefit payments were merely \$548 million larger than in 1929. On the other hand, in 1958—when business activity was only slightly depressed—they were \$4.4 billion above the level of 1957. Even these figures understate the difference between current conditions and those of a quarter century ago, for they leave out of account the private pensions which are beginning to supplement public pensions on a significant scale.

As a result of these several major developments in our national life, the movement of aggregate personal income is no longer closely linked to the movement of aggregate production. During the postwar period we have had several brief but sizable setbacks in production. For example, in the course of the recession of 1957-58, the physical output of factories and mines fell 14 per cent, the physical output of commodities and services in the aggregate fell 5.4 per cent, and the dollar volume of total output fell 4.3 per cent. In earlier times personal incomes would have responded decisively to such a decline in production. This time the government absorbed a substantial part of the drop in the dollar volume of production by putting up with a sharp decline of its revenues despite the need to raise expenditures. Corporations absorbed another part of the decline by maintaining dividends while their un-

distributed profits slumped. In the end, the aggregate of personal incomes, after taxes, declined less than 1 per cent and the decline was over before the recession ended.

Although the details have varied from one case to the next, a marked divergence between the movements of personal income and production has occurred in each of the postwar recessions. Indeed, during 1953-54 the total income at the disposal of individuals defied the recession by continuing to increase. This unique achievement was due to the tax reduction that became effective soon after the onset of recession as well as to the structural changes that have reduced the dependence of personal income on the short-run movements of production.

II

When we turn from personal income to employment, we find that the imprint of the business cycle is still strong. During each recession since 1948, unemployment reached a level which, while decidedly low in comparison with the experience of the 'thirties, was sufficient to cause serious concern. But although the fluctuations of employment have continued to synchronize closely with the movements of production, the relation between the two has been changing in ways which favor greater stability of employment in the future.

As the industrialization of our economy proceeded during the nineteenth century, an increasing part of the population became exposed to the hazards of the business cycle. Manufacturing, mining, construction, freight transportation—these are the strategic industries of a developing economy and they are also the industries in which both production and jobs have been notoriously unstable. Shortly after the Civil War, the employees attached to this cyclical group of industries already constituted 23 per cent of the labor force. Employees of industries that have remained relatively free from cyclical unemployment—that is, agriculture, merchandising, public utilities, financial enterprises, the personal service trades, and the government—accounted for another 32 per cent. The self-employed in farming, business, and the professions, whose jobs are especially steady, made up the rest or 45 per cent of the work force. This was the situation in 1869. Fifty years later, the proportion of workers engaged in farming, whether as operators or hired hands, had shrunk drastically, and this shrinkage was offset only in part by the relative gain of other stable sources of employment. Consequently, the proportion of employees in the cyclical industries kept rising, decade after decade, and reached 36 per cent in 1919.

Clearly, the broad effect of economic evolution until about 1920 was to increase the concentration of jobs in the cyclically volatile industries, and this was a major force tending to intensify declines of employment

during business contractions. Since then, the continued progress of technology, the very factor which originally was mainly responsible for the concentration in the cyclical industries, has served to arrest this tendency. The upward trend of production in manufacturing and the other highly cyclical industries has remained rapid in recent decades. However, advances of technology have come so swiftly in these industries as well as in agriculture that an increasing part of the nation's labor could turn to the multitude of tasks in which the effectiveness of human effort improves only slowly, where it improves at all. Thus the employees of "service" industries constituted 24 per cent of the labor force in 1919, but as much as 44 per cent in 1957. The proportion of self-employed workers in business and the professions, which was 9.4 per cent in the earlier year, became 10.6 per cent in the later year. True, these gains in types of employment that are relatively stable during business cycles were largely canceled by the countervailing trend in agriculture. Nevertheless, the proportion of employees attached to the cyclically volatile industries has not risen since 1919. Or to express this entire development in another way, the proportion of workers having rather steady jobs, either because they work for themselves or because they are employed in industries that are relatively free from the influence of business cycles, kept declining from the beginning of our industrial revolution until about 1920, and since then has moved slightly but irregularly upward.

Thus, the changing structure of industry, which previously had exercised a powerful destabilizing influence on employment and output, particularly the former, has ceased to do so. The new stabilizing tendency is as yet weak, but it is being gradually reinforced by the spread of "white-collar" occupations throughout the range of industry. For many years now, the proportion of people who work as managers, engineers, scientists, draftsmen, accountants, clerks, secretaries, salesmen, or in kindred occupations has been increasing. The white-collar group, which constituted only 28 per cent of the labor force outside of agriculture in 1900, rose to 38 per cent in 1940 and to 44 per cent in 1957. Workers of this category are commonly said to hold a "position" rather than a "job" and to be paid a "salary" rather than a "wage." Hence, they are often sheltered by a professional code which frowns upon frequent firing and hiring. Moreover, much of this type of employment is by its nature of an overhead character and therefore less responsive to the business cycle than are the jobs of machine operators, craftsmen, assembly-line workers, truck drivers, laborers, and others in the "blue-collar" category. For example, during the recession of 1957-58, the number of "production workers" employed in manufacturing, who approximate the blue-collar group, declined 12 per cent, while the employ-

ment of "nonproduction workers," who approximate the white-collar group, declined only 3 per cent. This sort of difference has been characteristic of recessions generally, not only the most recent episode, and on a smaller scale it has also been characteristic of industry generally, not only of manufacturing.

It appears, therefore, that changes in the occupational structure of the labor force, if not also in the industrial structure, have been tending of late to loosen the links which, over a considerable part of our economic history, tied the short-run movement of total employment rather firmly to the cyclical movement of total production, and especially to the cyclical movement of its most unstable parts—that is, the activities of manufacturing, mining, construction, and freight transportation. This stabilizing tendency promises well for the future, although up to the present it has not left a mark on records of aggregate employment that is comparable with the imprint that the stabilizing influences we discussed previously have left on personal income. In the postwar period, as over a longer past, the number of men and women at work, and even more the aggregate of hours worked by them, has continued to move in fairly close sympathy with the fluctuations of production.

We can no longer justifiably suppose, however, when employment falls 2 million during a recession, as it did between July 1957 and July 1958, that the number of people who receive an income has declined by any such figure. In fact, the number of workers drawing unemployment insurance under the several regular plans rose about 1.3 million during these twelve months, while the number of retired workers on public pensions rose another million. Hence, it may be conservatively estimated that the number of income recipients increased over 300 thousand despite the recession. In the other postwar recessions our experience was fairly similar. In other words, as a result of some of the structural changes on which I dwelt earlier, the size of the income-receiving population has grown steadily and escaped cyclical fluctuations entirely.¹

III

Turning next to consumer spending, we must try once again to see recent developments in historical perspective. The fact that stands out is that the impact of business cycles on consumption has recently diminished, while the effects of consumption on the business cycle have become more decisive.

In the classical business cycle, as we came to know it in this country,

¹ This upward trend would appear steeper than I have suggested if recipients of property income and of public assistance were included in the count. In the present context, however, it has seemed best to restrict the income-receiving population to the working class, or more precisely, to members of the labor force or those recently in the labor force who receive an income as a matter of right and on some regular basis.

once business investment began declining appreciably, a reduction of consumer spending soon followed. Sometimes the expansion of investment culminated because the firms of one or more key industries, finding that their markets were growing less rapidly than had been anticipated, made an effort to bring their productive capacity or inventories into better adjustment with sales. Sometimes the expansion culminated because the belief grew that construction and financing costs had been pushed to unduly high levels by the advance of prosperity. Sometimes it culminated for all these or still other reasons. But whatever the cause or causes of the decline in investment, it made its influence felt over an increasing area of the economy. For a while consumer spending was maintained at a peak level or even kept rising. But since businessmen were now buying on a smaller scale from one another, more and more workers lost their jobs or their overtime pay, financial embarrassments and business failures became more frequent, and uncertainty about the business outlook spread to parts of the economy in which sales and profits were still flourishing. If some consumers reacted to these developments by curtailing their spending in the interest of caution, others did so as a matter of necessity. Before long, these curtailments proved sufficient to bring on some decline in the aggregate spending of consumers. The impulses for reducing business investments therefore quickened and the entire round of events was repeated, with both investment and consumption declining in a cumulative process.

As the contraction continued, it tried men's patience, yet in time worked its own cure. Driven by hard necessity, business firms moved with energy to reduce costs and increase efficiency. Consumers whose incomes were declining often saved less or dissaved in order not to disrupt their customary living standards. Hence, even if sales and prices were still falling, profit margins improved here and there. In the meantime, bank credit became more readily available, costs of building and terms of borrowing became more favorable, the bond market revived, business failures diminished, and the investment plans of innovators and others began expanding again. When recovery finally came, it was likely to be led by a reduced rate of disinvestment in inventories or by a new rush to make investments in fixed capital. At this stage of the business cycle, consumer spending was at its very lowest level, if not still declining.

Many of these features of earlier business cycles have carried over to the present. However, the behavior of consumers in the postwar recessions has departed from the traditional pattern in two respects. In the first place, consumers maintained their spending at a high level even after business activity had been declining for some months, so that the tendency of recessions to cumulate was severely checked. During the

recession of 1945 consumer spending actually kept increasing. In each of the later recessions it fell somewhat; but the decline at no time exceeded one per cent and lasted only a quarter or two. In the second place, instead of lagging at the recovery stage of the business cycle, as it had in earlier times, consumer spending turned upward before production or employment resumed its expansion. This shift in cyclical behavior appears clearly in department store sales, which have been recorded on a substantially uniform basis for several decades and are widely accepted as a tolerably good indicator of consumer spending. In the recoveries of 1921, 1924, 1927, and 1938, these sales lagged by intervals ranging from two to four months. In 1933 their upturn came at the same time as in production and employment. It thus appears that, during the 1920's and 1930's, consumer spending in no instance led the economy out of a slump. In the postwar period, on the other hand, department store sales have led successive recoveries by intervals stretching from two to five months. Of course, department store sales cover only a small fraction of consumer expenditure, and correction for price changes would alter their historical record somewhat. But the main features of the cyclical behavior of dollar sales by department stores are broadly confirmed by other evidence on consumer spending, which is extensive for recent years. We may therefore conclude with considerable assurance that consumer spending has played a more dynamic role in recent times. Not only have consumers managed their spending during recessions so that the cumulative process of deflation has been curbed, but consumer spending has emerged as one of the active factors in arresting recession and hastening recovery.

This new role of the consumer in the business cycle reflects some of the developments of the postwar period that we considered earlier, particularly the greatly enhanced stability in the flow of personal income, the steady expansion in the number of income recipients, and the relative increase in the number of steady jobs. It reflects also the improvements of financial organization and other structural changes which have strengthened the confidence of people, whether acting as consumers or investors, in their own and the nation's economic future. Whatever may have been true of the past, it can no longer be held that consumers are passive creatures who lack the power or the habit of initiating changes in economic activities. There is no harm in thinking of consumer spending as being largely "determined" by past and current incomes, provided we also recognize that the level of current incomes is itself shaped to a significant degree by the willingness of people to work hard to earn what they need to live as they feel they should. The evidence of rising expectations and increased initiative on the part of consumers is all around us. It appears directly in the rapidly

rising proportion of women in the labor force, in the sizable and increasing proportion of men who hold down more than one job, in the slackening of the long-term decline of the average work week in manufacturing despite the increased power of trade unions, as well as indirectly in the improvement of living standards and the great upsurge of population. Indeed, the expansive forces on the side of consumption have been so powerful that we must not be misled by the cyclical responses of consumer spending, small though they were, to which I referred earlier. There are no continuous records of inventories in the hands of consumers; but if such statistics were available, we would almost certainly find that consumption proper, in contrast to consumer spending, did not decline at all during any of the postwar recessions.

In view of these developments in the realm of the consumer, it is evident that the force of any cyclical decline of production has in recent years been reduced or broken as its influence spread through the economy. Production has remained unstable, but the structure of our economy has changed in ways which have limited the effects of recessions on the lives of individuals—on the numbers who receive an income, the aggregate of personal incomes, consumer spending, actual consumption, and to some degree even the numbers employed. It is, therefore, hardly an exaggeration to assert that a good part of the personal security which in an earlier age derived from living on farms and in closely knit family units, after having been disrupted by the onrush of industrialization and urbanization, has of late been restored through the new institutions that have developed in both the private and public branches of our economy.

IV

In concentrating, as I have thus far, on the changes of economic organization which have lately served to reduce the impact of business cycles on the lives of individuals, I have provisionally taken the cyclical movement of production for granted. Of course, if the fluctuations of production had been larger, the impact on people would have been greater. On the other hand, the stabilized tendency of personal income and consumption has itself been a major reason why recent recessions of production have been brief and of only moderate intensity. Many other factors have contributed to this development. Among them are the deliberate efforts made in our generation to control the business cycle, of which I have as yet said little.

In earlier generations there was a tendency for the focus of business thinking to shift from the pursuit of profits to the maintenance of financial solvency whenever confidence in the continuance of prosperity began to wane. At such times experienced businessmen were prone to

reason that it would shortly become more difficult to collect from their customers or to raise funds by borrowing, while they in turn were being pressed by their creditors. Under the circumstances it seemed only prudent to conserve cash on hand, if not also to reduce inventories or accounts receivable. Such efforts by some led to similar efforts by others, in a widening circle. As pressure on commodity markets, security markets, and on the banking system mounted, the decline of business activity was speeded and the readjustment of interest rates, particularly on the longer maturities, was delayed. More often than not the scramble for liquidity ran its course without reaching crisis proportions. Sometimes, however, as in 1873, 1893, and 1907, events took a sinister turn. Financial pressures then became so acute that doubts arose about the ability of banks to meet their outstanding obligations and, as people rushed to convert their deposits into currency, even the soundest banks were forced to restrict the outflow of cash. With the nation's system for making monetary payments disrupted, panic ruled for a time over the economy and production inevitably slumped badly.

It was this dramatic phase of the business cycle that first attracted wide notice and stimulated students of public affairs to seek ways and means of improving our financial organization. The Federal Reserve Act, which became law under the shadow of the crisis of 1907, required the pooling of bank reserves and established facilities for temporary borrowing by banks. The hope that this financial reform would ease the transition from the expanding to the contracting phase of business cycles has been amply justified by experience. But the Federal Reserve System could not prevent the cumulation of financial trouble during business expansions. Nor could it prevent runs on banks or massive bank failures, as the Great Depression demonstrated. The need to overhaul and strengthen the financial system became increasingly clear during the 'thirties and led to numerous reforms, among them the insurance of mortgages, the creation of a secondary market for mortgages, the insurance of savings and loan accounts, and—most important of all—the insurance of bank deposits. These financial reforms have served powerfully to limit the propagation of fear, which in the past had been a major factor in intensifying slumps of production.

But more basic than the financial innovations or any other specific measures of policy has been the change in economic and political attitudes which took root during the 'thirties. The economic theory that depressions promote industrial efficiency and economic progress lost adherents as evidence accumulated of the wreckage caused by unemployment and business failures. The political belief that it was best to leave business storms to blow themselves out lost its grip on men's

minds as the depression stretched out. In increasing numbers citizens in all walks of life came around to the view that mass unemployment was intolerable under modern conditions and that the federal government has a continuing responsibility to foster competitive enterprise, to prevent or moderate general economic declines, and to promote a high and rising level of employment and production. This new philosophy of intervention was articulated by the Congress in the Employment Act of 1946, which solemnly expressed what had by then become a national consensus.

In recent times, therefore, the business cycle has no longer run a free course and this fact has figured prominently in the plans of businessmen as well as consumers. During the 1930's, when the objectives of social reform and economic recovery were sometimes badly confused, many investors suspected that contracyclical policies would result in narrowing the scope of private enterprise and reducing the profitability of investment. These fears diminished after the war as the government showed more understanding of the need to foster a mood of confidence so that enterprise, innovation, and investment may flourish. In investing circles, as elsewhere, the general expectation of the postwar period has been that the government would move with some vigor to check any recession that developed, that its actions would by and large contribute to this objective, and that they would do so in a manner that is broadly consistent with our national traditions. This expectation gradually became stronger and it has played a significant role in extending the horizons of business thinking about the markets and opportunities of the future. The upsurge of population, the eagerness of consumers to live better, the resurgence of Western Europe, the revolutionary discoveries of science, and the steady flow of new products, new materials, and new processes have added impetus to the willingness of investors to expend huge sums of capital on research and on the improvement and expansion of industrial plant and equipment. Some of these influences have also been effective in augmenting public investment. The fundamental trend of investment has therefore been decidedly upward. The private part of investment has continued to move cyclically; but it is now a smaller fraction of total national output and it has displayed a capacity to rebound energetically from the setbacks that come during recessions.

The specific measures adopted by the government in dealing with the recessions of the postwar period have varied from one case to the next. In all of them, monetary, fiscal, and housekeeping policies played some part, with agricultural price-support programs assuming special prominence in one recession, tax reductions in another, and increases of public expenditure in still another. Taking a long view, the most nearly consistent part of contracyclical policy has been in the monetary

sphere. Since the early 1920's, when the Federal Reserve authorities first learned how to influence credit conditions through open-market operations, long-term interest rates have tended to move down as soon as the cyclical peak of economic activity was reached, in contrast to the long lags that were characteristic of earlier times. Since 1948 the decline of long-term interest rates in the early stages of a recession has also become more rapid. This change in the cyclical behavior of capital markets reflects the increased vigor and effectiveness of recent monetary policies. Inasmuch as optimism, as a rule, is still widespread during the initial stages of an economic decline, a substantial easing of credit, provided it comes early enough, can appreciably hasten economic recovery. This influence is exerted only in part through lower interest rates. Of greater consequence is the fact that credit becomes more readily available, that the money supply is increased or kept from falling, that the liquidity of financial assets is improved, and that financial markets are generally stimulated. The effects of easier credit are apt to be felt most promptly by smaller businesses and the home-building industry, but they tend to work their way through the entire economy. There can be little doubt that the rather prompt easing of credit conditions, which occurred during recent setbacks of production, was of some significance in keeping their duration so short.

Business firms have also been paying closer attention to the business cycle, and not a few of them have even tried to do something about it. These efforts have been expressed in a variety of ways—through the adoption of long-range capital budgets, closer control of inventories, and more energetic selling or some relaxation of credit standards in times of recession. I do not know enough to assess either the extent or the success of some of these business policies. Surely, business investment in fixed capital has remained a highly volatile activity—a fact that is sometimes overlooked by concentrating attention on years instead of months and on actual expenditures instead of new commitments. There is, however, strong evidence that the businessmen of our generation manage inventories better than did their predecessors. The inventory-sales ratio of manufacturing firms has lately averaged about a fourth less than during the 1920's, despite the increased importance of the durable goods sector where inventories are especially heavy. The trend of the inventory-sales ratio has also moved down substantially in the case of distributive firms. This success in economizing on inventories has tended to reduce the fluctuations of inventory investment relative to the scale of business operations and this in turn has helped to moderate the cyclical swings in production. Not only that, but it appears that the cyclical downturns of both inventories and inventory investment have tended to come at an earlier stage of the business cycle in

the postwar period than they did previously, so that any imbalance between inventories and sales could be corrected sooner. Since consumer outlays—and often also other expenditures—were well maintained during the recent recessions of production, the rising phase of inventory disinvestment ceased rather early and this naturally favored a fairly prompt recovery of production.

Thus, numerous changes in the structure of our economy have combined to stimulate over-all expansion during the postwar period and to keep within moderate limits the cyclical declines that occurred in production. Indeed, there are cogent grounds for believing that these declines were even more moderate than our familiar statistical records suggest. The line of division between production for sale and production for direct use does not stand still in a dynamic economy. In the early decades of the industrial revolution an increasing part of our production was, in effect, transferred from the home to the shop and factory. This trend has continued in the preparation of foods, but in other activities it appears on balance to have been reversed. The great expansion of home ownership, the invention of all sorts of mechanical contrivances for the home, longer vacations, the general eagerness for improvement, if not also the income tax, have stimulated many people to do more and more things for themselves. Consumers have become equipped to an increasing degree with the capital goods they need for transportation, for the refrigeration of food, for the laundering of clothes, as well as for entertainment and instruction. They have also been doing, on an increasing scale, much of the carpentry, painting, plumbing, and landscaping around their homes. Such activities of production are less subject to the business cycle than the commercial activities which enter statistical reports. Yet these domestic activities have undoubtedly been expanding rapidly, and perhaps expanding even more during the declining than during the rising phase of the business cycle. Hence, it is entirely probable that the cyclical swings of production have of late been smaller, while the average rate of growth of production has been higher, than is commonly supposed.

V

It is in the nature of an economic vocabulary to change slowly, when it changes at all. We keep speaking of the price system, the business cycle, capitalism, socialism, communism, and sometimes we even refer to the "inherent instability" of capitalism or of communism; but the reality that these terms and phrases are intended to denote or sum up does not remain fixed. I have tried to show how a conjuncture of structural changes in our economy has served to modify the business cycle of our times. Some of these changes were planned while others were

unplanned. Some resulted from efforts to control the business cycle while others originated in policies aimed at different ends. Some arose from private and others from public activities. Some are of very recent origin and others of long standing. The net result has been that the intensity of cyclical swings of production has become smaller. The links that previously tied together the cyclical movements of production, employment, personal income, and consumption have become looser. And, as everyone knows, the once familiar parallelism of the short-term movements in the physical volume of total production, on the one hand, and the average level of wholesale or consumer prices, on the other, has become somewhat elusive.

To be sure, special factors of an episodic character played their part in recent business cycles, as they always have. For example, a pent-up demand for civilian goods was highly significant in checking the recession of 1945. The tax reduction legislated in April 1948 helped to moderate the recession which began towards the end of that year. The tax cuts announced soon after business activity began receding in 1953 merely required executive acquiescence in legislation that had been passed before any recession was in sight. Again, the sputniks spurred the government's response to the recession of 1957-58. Special circumstances such as these undoubtedly weakened the forces of economic contraction at certain times; but they also strengthened them at other times. In particular, governmental purchases from private firms have not infrequently been an unsettling influence rather than a stabilizing force. We need only recall the drop of federal expenditure on commodities and services from an annual rate of \$91 billion in the early months of 1945 to \$16 billion two years later, or the fall from \$59 billion to \$44 billion soon after the Korean hostilities came to a close. The ability of our economy to adjust to such major disturbances without experiencing a severe or protracted slump testifies not only to our good luck; it testifies also to the stabilizing power of the structural changes that I have emphasized.

It seems reasonable to expect that the structural changes in our economy, which have recently served to moderate and humanize the business cycle, will continue to do so. The growth of corporations is not likely to be checked, nor is the tendency to pay fairly stable dividends likely to be modified. The scale of governmental activities will remain very extensive, and so it would be even if the communist threat to our national security were somehow banished. Our methods of taxation might change materially, but the income tax will remain a major source of governmental revenue. Governmental expenditures might fluctuate sharply, but they are not likely to decline during a recession merely

because governmental revenues are then declining. The social security system is more likely to grow than to remain stationary or contract. Private pension arrangements will multiply and so also may private supplements to unemployment insurance. Our population will continue to grow. The restlessness and eagerness of consumers to live better is likely to remain a dynamic force. Research and development activities will continue to enlarge opportunities for investment. Governmental efforts to promote a high and expanding level of economic activity are not likely to weaken. Private businesses will continue to seek ways to economize on inventories and otherwise minimize the risk of cyclical fluctuations in their operations. Employment in agriculture is already so low that its further decline can no longer offset future gains of the service industries on the scale experienced in the past. The spread of white-collar occupations throughout the range of industry will continue and may even accelerate. For all these reasons, the business cycle is unlikely to be as disturbing or troublesome to our children as it once was to us or our fathers.

This is surely a reasonable expectation as we look to the future. Yet, it is well to remember that projections of human experience remain descriptions of a limited past no matter how alluringly they are expressed in language of the future. A lesson of history, which keeps resounding through the ages, is that the most reasonable of expectations sometimes lead nations astray. If my analysis is sound, it supports the judgment that the recessions or depressions of the future are likely to be appreciably milder on the average than they were before the 1940's. It supports no more than this. In view of the inherent variability of business cycles and our still somewhat haphazard ways of dealing with them, there can be no assurance that episodic factors will not make a future recession both longer and deeper than any we experienced in the postwar period.

Nor can there be any assurance that the conjuncture of structural changes on which I have dwelt will not be succeeded by another which will prove less favorable to economic stability. For example, although the stabilizing influence of the rising trend of white-collar employment in manufacturing has been more than sufficient to offset the cyclically intensifying influence of a greater concentration of employment in the durable goods sector, the balance of forces might be tipped the other way in the future. This could happen all the more readily if, as white-collar work continues to grow, the need to cut costs during a recession should make this type of employment less stable than it has been. Again, our exports in recent decades have tended to intensify the business cycle somewhat, and this factor may become of larger significance.

Also, it still remains to be seen whether the rising trend of prices—to say nothing of the rapidly growing consumer and mortgage debt—may not serve to complicate future recessions.

A generation ago many economists, having become persuaded that our economy had reached maturity, spoke grimly of a future of secular stagnation. Parts of their analysis were faulty and their predictions have proved wrong; yet their warning helped to mobilize thought and energy to avert the danger of chronic unemployment. Of late, many economists have been speaking just as persuasively, though not always as grimly, of a future of secular inflation. The warning is timely. During the postwar recessions the average level of prices in wholesale and consumer markets has declined little or not at all. The advances in prices that customarily occur during periods of business expansion have therefore become cumulative. It is true that in the last few years the federal government has made some progress in dealing with inflation. Nevertheless, wages and prices rose appreciably even during the recent recession, the general public has been speculating on a larger scale in common stocks, long-term interest rates have risen very sharply since mid-1958, and the yield on stocks relative to bonds has become abnormally low. All these appear to be symptoms of a continuation of inflationary expectations or pressures.

Such developments have often led to economic trouble. They could do so again even if our balance of payments on international account remained favorable. That, however, has not been the case for some time. The "dollar shortage" which influenced much of our economic thinking and practice during the past generation seems to have ended. The economies of many areas of the Free World, especially of Western Europe and Japan, have lately been rebuilt and their competitive power has been restored. This re-establishment of competitive and monetary links between our country and others may cause us some inconvenience, but it is basically a promising development for the future. It should stimulate our economic growth as well as contribute to the economic progress and political stability of other nations of the Free World. Our financial policies, however, will gradually need to be adjusted to the changed international environment. Although our gold stocks are still abundant and the dollar is still the strongest currency in the world, we can no longer conduct our economic affairs without being mindful of gold, or of the short-term balances that foreign governments and citizens have accumulated here, or of the levels of labor costs, interest rates, and prices in our country relative to those in other nations. Unless the deficit in our balance of payments is soon brought under better control, our nation's ability to pursue contracyclical policies during a business recession may be seriously hampered.

We are living in extraordinarily creative but also deeply troubled times. One of the triumphs of this generation is the progress that our nation has made in reducing economic instability. In the years ahead, no matter what we do as a people, our economy will continue to undergo changes, many of which were neither planned nor anticipated. However, the course of events, both domestic and international, will also depend—and to a large degree—on our resourcefulness and courage in deliberately modifying the structure of our economy so as to strengthen the forces of growth and yet restrain instability.

Great opportunities as well as difficult problems face our nation. Monopoly power, which is still being freely exercised despite all the exhortation of recent years, can be curbed by moving toward price and wage controls or, as many economists still hope, by regenerating competition. Higher protective tariffs, import quotas, and "Buy American" schemes can be embraced or, as many economists hope, avoided. A tax structure that inhibits private investment and directs people's energy into activities that contribute little to the nation's economic strength can be retained or reformed. Costly farm surpluses can be further encouraged by government or discontinued. The problems posed by the slums and the inefficient transportation of many of our cities can be neglected or attacked with some zeal. The inadequacy of our unemployment insurance system can be ignored until the next recession or corrected while there is opportunity for a judicious overhauling. In general, our governmental authorities can deal with recessions by trusting to improvisations of public spending, which often will not become effective until economic recovery is already under way, or by providing in advance of any recession for fairly prompt and automatic adjustment of income tax rates to a temporarily lower level of economic activity. The coordination of governmental policies, which may make the difference between success and failure in promoting our national objectives, can be left largely to accidents of personal force and ingenuity or it can be made systematic through an economic policy board under the chairmanship of the President. These and other choices will have to be made by the people of the United States; and economists—far more than any other group—will in the end help to make them.

INFLATION: COST-PUSH AND DEMAND-PULL

By FRANKLYN D. HOLZMAN*

The primary purpose of this paper is to draw clearly, at the macroeconomic level, the distinction between cost-push and demand-pull inflations and to show their interrelationships. Criteria for distinguishing between them at the macroeconomic level will be discussed in Section I. Section II presents a model which expresses the interrelationships between these cost and demand forces. In Section III, the conditions under which it *is* or *is not* possible to distinguish between cost and demand inflations are established. In Section IV the U.S. inflation of 1955-58 is briefly examined in terms of the results derived from the model.

The concern here is with creeping inflations such as the United States has been experiencing in the postwar period. This justifies neglect of expectations. We also make the assumption that household consumption is subject to the money illusion. The implications for the model of "real-income consciousness" will be indicated, however. The supply of money is not included in the model. For simplicity, it is assumed that the monetary authority provides sufficient funds to meet the requirements of the economy at a fixed interest rate. This does not appear to be a serious simplification in terms of the objectives of this paper. The term cost-push will refer, unless otherwise specified, to a wage-push. This is not meant to deny the possibility of an autonomous "profit-push" but indicates a belief that, in recent years at least, the wage-push has probably assumed greater importance. Finally, the model is conceived to deal only with the short-run impacts of cost and demand forces, and not with questions pertaining to secular inflation.

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¹ The Swedish approach, initiated by Bent Hansen [5] and followed by Hans Brems and Ralph Turvey [8] and others, of distinguishing between factor and final output markets is not equivalent to distinguishing between cost-push and demand-pull. Actually, these writers simply separate demand-pull effects in factor-market and final-output market components and do not consider increases in wages where demand is not permissive.

I. *Some Criteria of Cost and Demand Inflation*

At first glance it would not appear difficult to distinguish between cases of inflation which are due respectively to the push of wage increases and to the pull of demand. It seems reasonable to infer that if an inflationary situation is characterized by a buyers' market, prices are being *pushed* up faster than demand will permit.² On the other hand, if a sellers' market exists in the course of an inflationary movement, one can infer that the inflation is demand-inspired, that demand is out-running supply, and that prices are being *pulled* up. In practice, the criteria just stated would have to be formulated differently to be operationally useful. Thus, a buyers' market can be said to exist when, at given prices, inventories begin to accumulate³ and factors of production become unemployed. The opposite would be true of a sellers' market. ✓

Probably the simplest indicator of softening markets is a rise in the level of unemployment. This is certainly an easier statistic to come by in most countries than the degree of capital-capacity utilization, and probably both are easier to come by and more reliable than other indicators such as changes in inventories, manufacturers' advance orders, etc. One defect of unemployment as an indicator is that it may be sticky. That is, there might be cost-push with no increase in unemployment because workers are not usually laid off immediately as markets soften, although underemployment may develop and show up in the form of shorter hours. Thus, if wage rates, prices, and unemployment (including an allowance for shorter hours⁴) are all rising simultaneously, wages are being pushed up faster than demand; the cost-push effects of the wage-rate increase predominate over whatever demand-pull forces are operating in the economy. On the other hand, if wage rates, prices, and employment are rising simultaneously, we say, as a first approximation, that demand-pull predominates. This method seems the best for making the distinction.

A second possible method would involve a comparison of the rates of increase of prices and wages. If prices are rising faster than wage rates, one could argue that demand is at work. The opposite would not be true however; wage rates rising more rapidly than prices would not be an unambiguous indicator of cost-push. One would expect wage rates to rise faster than prices as long as (1) wages do not comprise the total cost of commodities and (2) increases in productivity occur. If however wage rates, adjusted both for productivity increases and for the percentage of total cost which they comprise, still increase more rapidly

² We assume imperfect markets since cost-push cannot occur in a perfect market.

³ Accumulating inventories might also signify the speculative anticipation of rising prices of materials such as often prevails in a sellers' market.

⁴ An adjustment may also have to be made for the annual increment to the labor force.

than final output prices, a case could be made for wage-push. Under these circumstances, the profit per unit of output would be expected to decline. The adjusted price-wage gap or profit criterion loses some of its usefulness because the price-wage gap and profit per unit of output are both so difficult to measure accurately for the purpose at hand. In addition, if there should be an autonomous profit-push either with or without the wage-push then the price-wage gap would not close as indicated above but would remain constant or would widen. For these reasons, the price-wage gap criterion is dropped.⁵

A third possible method suggests itself. Since the demand for labor is derived from the demand for finished products, one would expect to find, under demand-pull, final output prices leading wage rates temporally though not necessarily in magnitude. Under cost-push, wage rates would be expected to lead prices. However, since wage rates and prices usually rise simultaneously, it is a chicken-egg type problem to say which was first. This problem poses insuperable difficulties and the lead-lag criterion is therefore not employed.⁶

The unemployment criterion seems to be operationally the most satisfactory. Final judgment should be withheld, however, because so far the cost and demand inflationary forces have been presented in an oversimplified manner. More realistically, we find it appropriate to treat cost inflation as constituting the total impact of the following classes of forces: (1) cost-push—the change in prices and employment as a direct result of the cost effect of higher wage rates; (2) direct cost-pull—the change in prices and employment due to the change in spending which results from increasing wage rates at the expense of other factor incomes⁷; and (3) indirect cost-pull—the change in prices and employment which results from changes in investment, exports, and government expenditures induced by the wage-rate increases. Demand-pull, then, is defined as the change in prices and employment due to changes in investment, exports, and government expenditures autonomous to the wage-rate increase.

⁵ It can be shown that, in the absence of profit-push, the price-wage gap criterion corresponds, in theory, with the unemployment criterion in distinguishing cost and demand inflations.

⁶ The other criteria are also affected by this difficulty but not so crucially. Thus demand may pull prices and profits ahead of wages in one period only to have them "catch up" the following period. It is difficult to say in period II whether wages were pushed or pulled up. In the same sequence, demand may initially raise employment, but not wages. When wages finally rise in period II, and employment declines, the decline in employment can be attributed to either cost-push in period II or to the demand-pull of period I. Clearly, if there is no preponderance of either cost-push or demand-pull, it is probably not very meaningful to try to distinguish between them as the economy shifts back and forth from one to the other, nor may such a distinction be possible.

⁷ This is not a precise definition. See equations (8)-(10) below.

II. *The Model: Employment and Price Effects of Wage and Demand Increases*

A. *Components of the Model*

The symbols used to represent the variables and parameters of the model are:

Y = national income = national expenditures.

W = national wage bill or Nw .

w = wage rate per worker per year.

V = total profits.

G = expenditures by government for domestic goods and services:

$G(w)$ = same, induced by wage change; $G(a)$ = same, autonomous to changes in income and wages.

I = expenditures for domestically produced investment; $I(w)$ and $I(a)$ as in G ; induced investment as $v'V_{t-1}$.

X = expenditures for domestically produced exports; $X(w)$ and $X(a)$ as in G .

N = employment: $\Delta N = N_{t-1} - N_{t-2}$.

P = price level.

Q = level of output.

m = workers' marginal propensity to consume (spend).

v = profit-receivers' marginal propensity to consume.

v' = profit-receivers' marginal propensity to invest.

\bar{v} = profit-receivers' marginal propensity to spend ($v + v'$).

e = impact elasticity of demand for labor [defined in equation (1) below].

k = percentage of wage increase which comes out of profit; $(1 - k)$ = percentage of wage increase passed on in higher prices.

Δr = change in output per worker since previous wage increase (say 1 year).

d and d' = the percentage of the increase in demand (ΔD) which raises prices and which is spent on unemployed labor, respectively.

ΔD = increment to inflationary gap ($\Delta Y - \Delta S$) calculated as difference between new level of expenditures and new supply price.

ΔS = increase in value of goods sold due to cost-push.

$z = (m - \bar{v}k) - (1 - k)$.

t = subscript—time.

Before proceeding to the model, we will discuss briefly e , k , d and d' , m , \bar{v} , and Δr :

The letter e represents the impact-elasticity of demand for labor by employers, i.e., the elasticity of demand of employers for labor in the short run (say 6 months to a year), when faced with a wage-rate

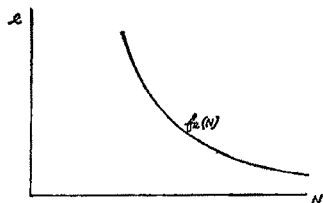
increase, on the assumption that the demand schedule remains unchanged.⁸ Write:

$$(1) \quad e = - \frac{\frac{\Delta N}{N_{t-2}}}{\frac{\Delta w}{w_{t-1}}}.$$

Neither e nor some of the other variables and parameters introduced in this section are meant to be operational but are designed to serve an heuristic end—to help isolate, conceptually, phenomena which cannot be isolated in the real world. (The purpose of e , for example, is to isolate the empirically nonisolable *direct* employment effects of a cost-push.) Nevertheless, the connection between these variables and parameters and the real economy should be obvious.

The value of e is assumed to reflect not only overt unemployment but underemployment in the form of shorter hours as well. The value of e for labor as a whole is probably fairly low in general reflecting the low marginal rate of substitution between labor and other factors in the short run. However, it seems likely that the value of e will depend on the level of employment and on whether the trend is up or down. For, in fact, the demand curve for labor will depend largely on the expectations of entrepreneurs as to how much they can sell at higher prices and this will depend on the state of the market and on whether the level of profits, V , is adequate to absorb all or part of a wage increase if necessary. In periods of expansion, profits will probably be high enough to absorb part of a wage-rate increase if necessary, but even more important, entrepreneurs will be willing to raise prices and maintain output on the basis of optimistic expectations. In periods of low and declining employment the reverse is likely to be true. The very-short-run demand curve for labor, as the entrepreneur sees it, then, will be very inelastic at high and rising levels of N , but then will become fairly elastic at low and declining levels of N (and V). For these reasons write:⁹

⁸ ΔN refers to $N_{t-2} - N_{t-1}$ and Δw to $w_{t-2} - w_{t-1}$. The dating of N and w in equation (1) requires explanation. The usual method of expressing an arc elasticity in terms consistent with the definition of point elasticity is to use average values of N and w respectively. However, such an expression would have substantially complicated the model presented below. A simpler (and more convenient in terms of our model) method of achieving the same result is to date N and w differently as in equation (1). Equation (1) reduces to the traditional point elasticity formula when ΔN and Δw are infinitesimals rather than finite changes.



⁹ The relationship between e and N may take the form indicated in the chart: as N increases, e approaches zero as a lower limit. The practical upper limit of e would be much lower than infinity since the wage-push would stop probably before N had fallen very far.

$$(2) \quad e = f_e(N, V); \quad \frac{\partial f_e}{\partial N} \quad \text{and} \quad \frac{\partial f_e}{\partial V} < 0.$$

In the case of $f_e(V)$, it should be noted, successive wage-rate increases over a period of years which successively reduce profits will cause the value of e to rise.

The letter k is used to represent the part of a payroll increase which is absorbed by profits. The term $(1-k)$, therefore, is the part of the wage increase which is marked-up into price.¹⁰ The value of k will be determined very largely by the state of the market. If employment is at a high level (and/or rising) because of high levels of autonomous expenditures, entrepreneurs will be able to pass on most if not all of a wage increase into higher prices, i.e., k will be small.¹¹ On the other hand, at low or declining levels of employment, k will be large as wage increases must be absorbed in profits. The value of k will also be affected by the customary pricing practices of entrepreneurs which to some extent are rigid over short periods and may not react immediately to changes in market conditions. Ignoring this last fact, we write:¹²

$$(3) \quad k = f_k(N); \quad f'_k < 0.$$

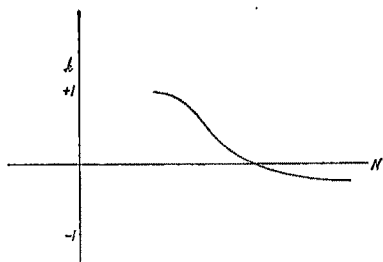
It should be noted that e and k tend to move in the same direction becoming smaller in periods of expansion and larger in periods of contraction.

¹⁰ The value of k does not represent the conscious mark-up decision of the entrepreneur but rather the ex post percentage of the payroll increase which comes out of profits after price, employment, and output have been adjusted for the wage increase. In this respect it is analogous to the usual measurement of the incidence of a sales tax on profits. Barring extremely large wage increases and very highly elastic demand curves for final output, there will not be much difference between the entrepreneur's decision as to how much of the wage-increase to absorb and the value of k .

¹¹ If there is profit-push, k will be negative; demand-pull implies $k \geq 0$.

¹² k would tend to vary inversely with N as in the chart: When N is at very low levels, k will rise but approach a limit in the neighborhood of 1 (higher than 1 means that prices are reduced by even more than the amount of the wage rate increase). At high levels of N , k

becomes 0, and under very inflationary conditions, probably, approaches some negative limit a little below 0 (meaning that prices are raised by more than the wage-rate increase). Note that while we posited that k depended on both the level and direction of change of employment, for simplicity of notation, k was made a function of N alone. In all functional relationships of N , it should be understood that rising N has the same effect as a high level of N , and declining N , the same effect as a low level of N . Note also that when we speak of high levels of N , we usually mean relative to full employment; and when we speak of low levels of N , the implication is a high level of involuntary unemployment.



The letters d and d' represent those fractions of a change in expenditures (demand) which raise prices and employment respectively. We assume that an increase in demand has both price and output effects, these depending in the usual way on the slopes and intercepts of the supply and demand curves for final output [1, p. 4]. The coefficient d is applied directly to the change in demand, along with a quantity deflator, to give us the resulting change in price. The change in employment of labor as a result of a change in expenditures for final output depends on the supply and demand curves for labor, the latter being derived in a complex manner from the supply and demand curves for final output. For simplicity, we multiply any change in expenditures by d' (and divide by the wage rate) to represent the whole complex of parameters which gives us the change in employment which results. The d 's do not add up to 1.

At low levels of employment, d' will approach a value equal to the percentage of wage income in total national income, or W/Y . That is to say, increases in demand will increase employment but not raise wage rates much. As employment increases and approaches full utilization of the labor force, the value of d' will decline and approach zero: since by definition, no more labor remains to be employed, any increase in demand will involve the use of other unemployed resources or serve to pull prices and wage rates up still further. The form of d' as a function of N will be similar to that of e with the exception that the upper limit, as N declines, is probably less than W/Y .¹³ Write:

$$(4) \quad d' = f_{d'}(N); \quad f'_{d'} < 0.$$

The value of d will move in the opposite direction from d' . At low levels of employment, changes in demand will have primarily output effects and d will approach zero. At full employment, price effects will predominate and d will approach unity. Write:

$$(5) \quad d = f_d(N); \quad f'_d > 0.$$

We view m as the straightforward parameter in a simple consumption function relating worker spending and wage income. As noted, we assume a money illusion. We also assume for simplicity, no autonomous changes in consumption and that the marginal propensity to invest of workers is zero. The value of \bar{v} , particularly v' , will vary with the level and direction of changes in employment and the level of profits, but the exact nature of the functional relationships is hard to specify. Thus, if employment and income are high and rising, entrepreneurial spend-

¹³ If wage-rates are being pushed up faster than other factor returns, other factors will be substituted for labor.

ing, and particularly investment expenditures, will tend to increase. On the other hand, as k declines and the level of profits rises, while the total amount of investment out of retained profits will tend to increase and the total amount of owner spending on consumers' goods will also increase, the *proportion* of spending to profits, \bar{v} , *may* decline in both instances. The opposite is true in periods of low and declining levels of employment. Since profits tend to rise in good times and fall in bad times, these forces always tend to offset each other and we cannot say here in which direction the net effect is likely to be. We write:

$$(6) \quad \bar{v} = f_v(N, V); \quad \frac{\partial f_v}{\partial N} > 0, \quad \frac{\partial f_v}{\partial V} \leq 0.$$

Since we do not know the net effect of these forces on \bar{v} , we assume throughout this paper that the value of \bar{v} remains constant.

5. The term Δr stands for the change in labor productivity since the previous wage-rate change, or change in output per worker per period (say one year). We assume that the change in productivity, like the wage change, takes place all at once. Its impact on the absorption of wages into profits is treated explicitly in our equations (and k is therefore here left unchanged by changes in productivity). An increase in productivity also reduces the value of e by increasing the level of profits [see (2)]; its impact on e is treated implicitly—a large increase in productivity is assumed to reduce the unemployment which results from a wage-push.

Before proceeding to the equations, it should be noted that we assume a one-period lag of expenditure behind income. To simplify the model, we assume that consumption and savings, imports, and taxes are all functions of income and that the values of m and \bar{v} reflect this fact. The equations presented below are derived in a national income framework in the appendix.

B. *Employment Effects*¹⁴

1. *Cost-push*: Assuming no change in demand, a wage-rate increase will act to reduce employment (and output). Following equation (1), the reduction in employment is defined as:

$$(7) \quad \Delta N^1 = - \frac{N_{t-2} \Delta w e}{w_{t-1}}.$$

¹⁴ The starting point of the model is Bronfenbrenner's brilliant paper, "A Contribution to the Aggregate Theory of Wages" [2]. Rothschild has shown in a comment [6] that Bronfenbrenner's analysis and results are conditioned by the fact that he assumes a complete money illusion on the part of his consumers rather than real-income consciousness. Bronfenbrenner's reply points out that the effects of real-income consciousness tend to be offset by real-balance (Pigou) effects. In a later section, an attempt is made to show the effects on our results of assuming real-income consciousness.

2. *Direct cost-pull*: We assume that wages and profits are the only factor returns. Then the increase in wage rates will (in the absence of profit-push) result in a reduction in profits. If the propensity to spend of wage-earners, m , exceeds that of profit-receivers, \bar{v} , expenditures will rise and employment will tend to increase, offsetting the cost effects noted above.¹⁵

$$(8) \quad \Delta Y = mN_{t-2}\Delta w(1-e) - \bar{v}kN_{t-2}(\Delta w - \Delta r)(1-e).$$

The first term on the right represents the change in expenditure out of wages. The change in wages, $N_{t-2}\Delta w$ ($i-e$), is multiplied by m to get the change in spending by wage-earners.¹⁶ The second term represents the change in expenditures out of profits. This is obtained by estimating the extent to which the wage-rate increase diminishes profits. For this purpose the wage-rate increase is reduced by Δr , the increase in output per worker since the previous wage increase. The wage increase adjusted for Δr is multiplied by k , the percentage of wage increase absorbed in profits, and by \bar{v} , the marginal propensity to spend of profit-receivers.

The expenditure effect of equation (8) does not represent the net inflationary (deflationary) gap generated by the wage-push. The gap is reduced to the extent that the wage-push has raised wages and prices. In other words, labor and final output must now be purchased at higher prices. The increase in supply price (i.e., value) of goods sold due to cost-push and adjusted for Δr is represented as follows:

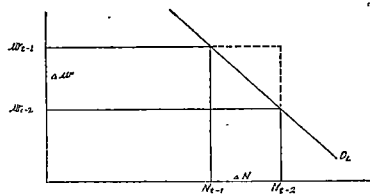
$$(9) \quad \Delta S = N_{t-2}(\Delta w - \Delta r)(1-e)(1-k).$$

In equation (9), the increase in wage rates is adjusted for an increase in productivity; the resulting amount is multiplied by the price mark-up ($1-k$). The inflationary (deflationary) gap or net direct expenditure effect of the wage-push is equal to (8) - (9):

$$(10) \quad \Delta D = mN_{t-2}\Delta w(1-e) - N_{t-2}(\Delta w - \Delta r)(1-e)[\bar{v}k + (1-k)].$$

The change in employment which results from the expenditure effect of direct cost-pull is obtained by multiplying (10) by d'/w_{t-1} where d' is the fraction of expenditures which goes to hire labor.

¹⁵ This is only true given certain values of the other parameters in equation (8). The cost-pull effects will be analyzed in more detail in Section III. Equation (8) is derived in a national income framework in the appendix.



¹⁶ The change in wages is equal to $N_{t-2} \Delta w - \Delta N w_{t-1}$. Substituting in (7) gives us, $N_{t-2} \Delta w - N_{t-2} \Delta w e$, which reduces to $N_{t-2} \Delta w (1-e)$.

$$(11) \quad \Delta N^2 = \frac{d'}{w_{t-1}} \Delta D.$$

3. *Indirect cost-pull*: Wage-rate increases have an indirect cost effect on expenditures and employment by affecting, with some lag: the level of autonomous (to income) investment, the level of exports, and the level of government expenditures.¹⁷

The level of, and changes in the level of, investment are a function of the rate of profit, rate of interest, availability of credit, expectations about prices, and many other variables.¹⁸ The rate and level of profit depend importantly, of course, on both Δw and k as indicated in equations (8) and, in the appendix, (a.6). Following these equations we assume that an increase in wage rates tends to reduce investment via the cost effect.

Wages are an important part of the selling price of export commodities. An increase in the wage rate would tend, in the absence of comparable changes in wages abroad or of offsetting changes in productivity and exchange rate, to reduce a nation's level of exports.

While increases in wage rates have a depressing effect on spending and employment through investment and exports, the opposite may be true in the case of government expenditures. There is a tendency for the government to try to maintain its level of real expenditures despite increases in costs. Also, as a matter of general fiscal policy, the government tends to validate wage-rate increases by spending to maintain employment when wage increases (or anything else) lead to unemployment. It appears strategic, however, to abstract here from the effects of fiscal policy designed to maintain full employment, in attempting to assess the impact of a cost-push, since fiscal policy is a tool used to deal with the unemployment or inflation which results from a wage-rate increase.

The change in employment, then, which results from indirect cost-pull is likely to be negative through the impact of a wage increase on exports and investment, but could be positive as a result of positive government reaction:

$$(12) \quad \Delta N^3 = \frac{d'}{w_{t-1}} [\Delta I(w) + \Delta X(w) + \Delta G(w)].$$

4. *Demand-pull*: Finally, the level of demand and of employment

¹⁷ Recall that we assume savings, taxes and imports are induced.

¹⁸ We divide investment, here, into that induced by changes in wages $I(w)$, that induced by changes in profits or income (represented by $v'V_{t-1}$ in equation (a.4) of the appendix, and that autonomous to both wage and income changes, $I(a)$. It should be noted that an increase in wages could have a positive effect on investment over the longer run by encouraging expenditure on labor-saving machines and equipment. Also, part of the impact of a wage increase on investment will be reflected in the marginal propensity to invest, v' .

will be affected very importantly by autonomous¹⁹ changes in: investment, $I(a)$; exports, $X(a)$; and government expenditures, $G(a)$. The change in employment which results may be represented by:²⁰

$$(13) \quad \Delta N^4 = \frac{d'}{w} [\Delta I(a) + \Delta X(a) + \Delta G(a)].$$

It should be noted that changes in autonomous investment, exports, and government expenditures can affect the level of employment indirectly by affecting the value of e and k , as indicated above. Also, given equivalent values of new autonomous expenditures, on the one hand, and increases in payroll, $N_{t-2}\Delta w(1-e)$, on the other, the employment (and price) impact of the former will be much greater than that of the latter. This is because the full amount of autonomous expenditures is spent in the first round on goods and services, whereas the increase in payroll: is partly saved ($1-m$); is partly at the expense of entrepreneurial spending (k); and is offset in part by the cost-push impact on employment.

The total change in employment, ΔN , is represented as the sum of the ΔN 's where ΔN^1 will always be negative except under the most inflationary circumstances, but the remaining ΔN 's can take on positive, negative or zero values. From this it becomes quite clear that one cannot necessarily infer either that wage-rate increases cause unemployment because an increase in wage rates is observed to be associated with a rise in unemployment or that wage-rate increases increase employment because a rise in wage rates is observed to be associated with an increase in employment. One must also take into consideration the fact that autonomous changes in investment, exports, and government expenditures unrelated to the wage change may have been responsible for the observed relationship.

C. Price Effects of Wage and Demand Increases

The price effects of wage-rate and demand increases are quite closely related to the employment effects and can therefore be set forth very briefly. The cost-push effect on prices can be represented by simply dividing the increase in value of goods sold due to cost-push, of equation (9), by the quantity of goods:

$$(14) \quad \Delta P^1 = \frac{\Delta S}{Q_{t-1}}.$$

The direct cost-pull effect on prices is represented by multiplying the change in expenditures of equation (10) by d , that fraction of the in-

¹⁹ With respect to income and wage changes.

²⁰ w takes on the date at which autonomous changes occur.

crease in expenditures which raises prices, and dividing through by the total quantity of goods:

$$(15) \quad \Delta P^2 = \frac{d}{Q_{t-1}} \Delta D.$$

The indirect cost-pull and demand-pull effects on prices (ΔP^3 and ΔP^4) are written analogously to the employment effects in (12) and (13). The total price rise in the period is the sum of the ΔP 's.

III. *Cost Inflation or Demand Inflation?*

A. *The Problem*

We define a cost inflation as comprising all those effects which result from an autonomous increase in wage rates, *viz.*, the sum of the effects of cost-push, direct cost-pull and indirect cost-pull. A demand inflation is that inflation which results from autonomous (to wages and income)

TABLE 1—COST AND DEMAND EFFECTS ON PRICES AND EMPLOYMENT

	Cost-Push	Direct and Indirect Cost-Pull		Demand-Pull	
		Positive	Negative	Positive	Negative
Price	upward	upward	downward	upward	downward
Unemployment	upward	downward	upward	downward	upward

changes in investment, exports and government expenditures. The criterion for distinguishing between cost and demand inflation presented earlier in the paper was that a cost-push raised prices but increased unemployment while a demand-pull raised prices but decreased unemployment.²¹

It is quite obvious, intuitively, that an increase in demand will have the effect just noted. From equations (7) and (14), we see that the cost-push effect of a wage-rate increase will be to increase unemployment and raise prices.²² So far, then, the inflations are distinct.

What about the effects of direct and indirect cost-pulls? Leaving aside, for the moment, whether they are likely to be positive or negative, the essential point is that they work through demand. Thus, if positive, they reduce unemployment and prices; if negative, they increase unemployment and prices. The sum total of effects may be more easily visualized in Table 1.

²¹ Assuming no increase in the labor force.

²² There are other conditions as well but these are almost sure to be fulfilled. For an increase in unemployment, e must be greater than 0. This is sure to be the case except under very inflationary circumstances. And for prices to increase, e and k must be less than 1. k will always be less than 1, and e will be less than 1 except in a moderately deep recession.

The difficulty in distinguishing between cost and demand inflations is apparent from the table. Positive cost-pull while re-enforcing the price effect of cost-push tends to reverse its impact with respect to unemployment (and has the same effect as positive demand-pull). Negative cost-pull re-enforces the unemployment indicator but reverses the price effect and, if strong enough, could cause deflation rather than inflation. In this respect it has the same impact as negative demand-pull.

It is clear that a necessary condition for the validity of our unemployment criterion is that cost-push effects must dominate cost-pull effects where they operate in different directions. This condition has two parts: (1) if cost-pull is positive it must not reduce unemployment by as much as cost-push increases it; (2) if cost-pull is negative, it must not reduce the price level by as much as cost-push raises it (or the result would be deflation). Because the indirect cost-pull effects are so long-run in incidence and their magnitude and direction impossible to define with any precision, we shall have to restrict ourselves here to cost-push and direct cost-pull.

B. *Does Cost Inflation Increase Prices and Unemployment?*

Condition (1), stated in the previous paragraph, will be fulfilled if:

$$(16) \quad \Delta N^1 + \Delta N^2 < 0.$$

Assuming for the moment that there have been no changes in productivity, ($\Delta r = 0$) this can be expressed in terms of our variables and parameters as [from (7) and (11)]:

$$(17) \quad \frac{d'(1-e)[(m-\bar{v}k) - (1-k)]}{e} < 1.$$

Let us look first at the bracketed term $[(m-\bar{v}k) - (1-k)]$ which we designate by the letter z . Recall that k is the percentage of a wage increase absorbed in profits, and m and \bar{v} are the marginal propensities to spend of wage-earners and profit-receivers, respectively. The range of possible values of z can be estimated by assuming all possible values of k and examining the implications for m and \bar{v} . At the extremes, k is likely to approach 0 at full employment and approach 1 at high levels of unemployment. This gives us the following equations:

$$\text{Full employment: } k = 0, m = \bar{z} + 1$$

$$\text{Much unemployment: } k = 1, m - \bar{v} = \bar{z}$$

Where $k = 0$, \bar{z} must be negative since it is unlikely that the marginal propensity to consume of workers will be unity or greater. Where $k = 1$, \bar{z} will undoubtedly be less than, say, .5 as a maximum since this value

represents the difference between workers' and entrepreneurs' propensities to spend. Thus, we see that z may vary from a small negative fraction at full employment to, say, .5, at low levels of employment.

Returning to inequality (17), Table 2 summarizes probable hypothetical value-ranges of its parameters at different levels of unemployment. Extreme values are taken purposely to establish the condition unambiguously.

TABLE 2—HYPOTHETICAL VALUES OF VARIABLES AT DIFFERENT UNEMPLOYMENT LEVELS

	Little Unemployment	Much Unemployment	Intermediate Unemployment
	(e.g., <3%)	(e.g., >8%)	(3-8%)
z	-.1 to -.3	.1 to .3	.3 to -.2
d'	.1 to .3	.7 to .9	.4 to .6
e	.1 to .3	.9 to 1.1	.4 to .6
$1-e$.7 to .9	-.1 to .1	.6 to .4
Maximum $\frac{d'z(1-e)}{e}$	-.09	+.03	+.28

From Table 2 we may infer that inequality (17) and therefore condition (1) are fulfilled. Thus a wage-push leads to unemployment because the positive employment effects of direct cost-pull are not likely to be sufficiently strong to offset the negative employment effects of cost-push.

Condition (2) will be fulfilled if:

$$(18) \quad P^1 + P^2 > 0.$$

Assuming again no changes in productivity, this condition can be expressed as follows [from (14) and (15)]:

$$(19) \quad (1-e)[(1-d)(1-k) + d(m - \bar{v}k)] > 0.$$

Since we are not primarily interested in situations of such a high level of unemployment that $e > 1$, we can rewrite (19) as follows:

$$(20) \quad e < 1, \quad k < \frac{1-d+dm}{1-d+\bar{v}d}.$$

Since $m > \bar{v}$ the fraction will be greater than 1. And k , as we have seen, will be less than 1. Thus (20) and therefore condition (2) are fulfilled. This means the negative price effects of negative direct cost-pull, *if any*, will not offset the positive price effects of cost-push—a wage increase will lead to a price rise.

Under our assumed conditions, then, we have demonstrated that the cost-push and direct cost-pull effects of an autonomous wage-rate in-

crease lead to unemployment and a price rise and the necessary conditions for using our criterion as an indicator of cost inflation are fulfilled.

C. Qualifications: Changes in Productivity and Real-Income Consciousness

We have abstracted, so far, from indirect cost-pull and from increases in productivity, and have also assumed complete money-illusion on the part of consumers, depriving them of any real-income consciousness. Indirect cost-pull will not be elaborated further here, but attention will now be given to the possibility of an increase in productivity and of the existence of real-income consciousness.

Examination of the cost-push and cost-pull equations indicates that the assumption of no productivity change, particularly where the average wage-rate increase is small (say under 6 per cent), reduces the validity of our unemployment criterion for the following reasons: An increase in productivity (a) reduces the initial cost-push drop in employment. This is because an increase in productivity raises profits, and the value of e falls since it varies inversely with the level of profits [cf. equation (2)]. (b) Since less labor is unemployed with a smaller e , the positive cost-pull of expenditure out of wages is increased. (c) The negative cost-pull of expenditures out of smaller profits is reduced because the rise in productivity tends to maintain profits in the face of a wage increase. This is offset, but probably only in part, by the decline in the value of e . (d) The negative cost-pull of the rise-in-supply price (which reduces the inflationary gap) is also reduced for the same reasons as in (c). These effects are more easily visualized through equations (8) — (10). The assumption of an increase in productivity does not appear to be serious in the case of our price condition because it has offsetting effects.²³

²³ An increase in productivity (a) tends to reduce the increase in prices due to rising costs (14), but also tends to accentuate the price increase by (b) maintaining entrepreneurial income and spending in the face of a wage increase (15); and (c) by increasing the inflationary gap due to the failure of costs and prices to rise as much as mentioned in (a).

Note that if productivity increases are as large as wage-rate increases, the entire cost-push effect on employment and prices is eliminated, as can be seen from (7) and (14). This does not mean that the wage-rate increase has no inflationary impact—it has; but the impact is through positive cost-pull which, as we have seen, is increased by increases in productivity. From a policy point of view, this has considerable significance. This is because inflationary forces which operate through demand can be offset through fiscal and monetary measures and price stability can be achieved without a reduction in employment. Cost-push forces, on the other hand, can only be stopped through fiscal and monetary measures in so far as these measures reduce the level of employment and weaken the bargaining strength of the unions.

Finally, we must consider whether a price rise which occurs concomitantly with wage-rate and productivity increases (the latter two of equal magnitude), can be legitimately considered cost-push even though it doesn't meet the criterion of causing unemployment. I would argue that it should be so considered where the wage-rate and productivity increases are large

If we remove the assumption of money illusion and give our consumers real-income consciousness, two new effects are introduced, one with respect to increases in productivity and the other with respect to rising wage rates and prices. The positive effect on employment of productivity increases is reduced or negated by such a change of assumptions. An increase in productivity means that the same number of workers can produce a larger output and at no greater cost *or* the same output can be produced by fewer workers and at a lower cost. The money-illusion assumption is equivalent to postulating that the entire increment in output will be taken off the market and workers will not be unemployed as a result of the productivity increase. On the other hand, if there is real-income consciousness, the output effect of the productivity increase will lead to some unemployment. This will be offset either partially or completely by the positive impact on employment which results from the cost-pull effect of the productivity increase.

While real-income consciousness implies more unemployment in the case of a productivity increase, it means just the opposite in connection with rising wage rates and prices. Faced with rising prices, a family with money illusion spends the same percentage of its money income thereby ending up with relatively smaller real consumption. On the other hand, a family with real-income consciousness will spend a larger percentage of money income than previously in an attempt to maintain its real level of consumption. In terms of our model, this would mean that the positive cost-pull effects on employment of a wage-push would be larger than we have indicated.²⁴

relative to the price increase. Suppose, for example, there have been wage-rate and productivity increases of 6 per cent each along with a price increase of 2 per cent. Clearly, demand forces also must have been present or there would not have been a price rise. But suppose there had been no wage increase! Under these circumstances prices would have fallen by 4 per cent instead of rising by 2 per cent (ignoring here the possibility that part of the productivity increase would have served to raise profits rather than lower prices). One could say, then, that the 6 per cent wage increase, not the increase in demand, was *primarily* responsible for the price increase *if* the wage increase can be considered to have been autonomous to demand forces. Since autonomous demand forces are assumed to pull up prices by 2 per cent, it can be assumed that these forces had a similar impact on wages. This leaves two-thirds of the wage increase to be explained either as an autonomous "push" or as the result of the productivity increase. While the productivity increase may cause an increase in demand for goods because of its real-income effects on spending, it causes no increase in demand for labor because the same number of laborers can produce the larger output—in fact it might even reduce the demand for labor (see next paragraph in text). Therefore, we conclude that the wage-rate increase would have been largely autonomous and, under the circumstances outlined, largely responsible for the price increase which occurred. Difficulty in visualizing this may stem from the fact that, for analytical convenience, productivity and wage-rate changes were paired together in the model, though in fact they are largely mutually autonomous. In conclusion, it should be noted that where there has been, say, a price increase of 6 per cent and wage and productivity increases of 3 per cent, demand (not cost) forces obviously would have been the cause of inflation.

²⁴ Rothschild [6], in his manipulation of Bronfenbrenner's model, deduces that real-income

To summarize: Our criterion does not provide a completely unambiguous indicator of a cost inflation. Cost inflation will not necessarily give rise to unemployment: (a) If productivity increases are large relative to wage increases, cost-pull may be strong enough that rising wage rates and prices will be accompanied by rising employment. (b) If real-income consciousness is assumed, however, the output effects of a large productivity increase will cause unemployment which will offset in whole or in part the cost-pull effect in (a). (c) Real-income consciousness also implies, however, larger cost-pull effects from rising wage rates and prices, which will, in turn, reduce unemployment and tend to offset in whole or in part the output effects of (b).

The distinction between cost and demand inflation can be made unambiguously only where prices, wage rates and unemployment are rising simultaneously. In this situation, cost inflation is primarily responsible because (a) demand inflationary forces, if dominant, would more than offset the cost-push reduction in employment and (b) deflationary demand forces, if dominant, would prevent prices from rising. Cost inflation will be more likely to exhibit these characteristics the smaller the increase in productivity and the more complete the money illusion.

If price and wage rates are rising, and unemployment declining, the situation is ambiguous. The declining unemployment could be due either to dominance of demand-pull forces, on the one hand, or to (a) strong cost-pull effects of a small gap between the increase in wage rates and productivity,²⁵ or to (b) strong cost-pull effects of rising wage rates and prices if consumers are real-income conscious.²⁶

Under certain conditions, even in this case, it could be argued that demand-pull is responsible. First, a rapid rise in employment almost certainly indicates demand-pull since it is unlikely that the positive employment effects of cost-pull could so overwhelm the negative effects of cost-push. Second, rising employment from a very high level of unemployment to begin with also indicates demand-pull because at high levels of unemployment wage pressures are weak or nonexistent and by themselves could not start a recovery.²⁷

consciousness implies no decline in employment from a wage increase so long as the marginal propensity to spend of workers is greater than that of profit-receivers. Aside from pointing out that complete real-income consciousness is as unrealistic as complete money-illusion, it should also be noted that this result also depends on the assumption that all of the price effects of the wage increase precede the spending effects. In actual fact, of course, part of the income increase will be spent at the old prices or at some intermediate level of prices short of the final price adjustment due to the wage increase.

²⁵ With money illusion and therefore no output effects of the productivity increase.

²⁶ The output and cost-pull effects of a large increase in productivity tending to offset each other.

²⁷ Some final qualifications: Our analysis has indicated the *direction* but not the magnitude of the impact on employment of wage and demand changes. It may often be the case that the

Finally, if cost-push leads to rising unemployment, as we believe it usually does, then it is bound to be a very short-run phenomenon. This is because the rising level of unemployment rapidly robs labor of its power to "push" up wages and prices. If substantial cost-push is generated and largely offsets the unemployment effects of the cost-push, then of course, the "push" may sustain itself for a much longer period of time. It seems unlikely, however, that a cost inflation could, in fact, continue uninterrupted for more than a few years (in the United States); over any longer period, cost-push forces are almost certain to be supplanted by stronger negative or positive cyclical demand forces.

IV. *The U.S. Economy, 1955-58*

We will briefly examine the U.S. inflation in this period in order to show the usefulness of the general conceptual approach developed above²⁸ and to evaluate alternative approaches. We will begin with the last 2 years of the period since they are the easiest to analyze.

The inflation from March 1957 to August 1958 seems to have been caused, unambiguously, by cost-push. Unemployment (adjusted) rose from 3.9 per cent in March 1957 to 7.6 per cent in August 1958 with only a brief decline in May-June 1958. Unemployment was accentuated from March 1957 until April 1958 by a 4 per cent decline in average weekly hours of work in manufacturing. Concomitantly, average hourly earnings in manufacturing and the CPI (consumer price index) each rose by almost 4 per cent and the WPI (wholesale price index) by about 2 per cent. An important contributing factor to the rising unemployment was the drop in demand.²⁹ Both private domestic investment and net foreign investment fell sharply and were not at all compensated for by the gradual rise in government purchases. While declining demand contributed (along with cost-push) to rising unemployment, it must also have constituted a drag on prices and wage rates. That prices and wage rates rose at all during this period, therefore, must be attributed to the dominance of cost-push.

L. E. Gallaway [4] rejects cost-push or what he calls the "unqualified wage-push inflation thesis" for 1950-1957. His reasoning as applied to 1957 at least, is based primarily on the mistaken premise that no

employment effects of either force operating in isolation may be too small to allow one to say with any confidence what type of inflation is being experienced. Difficulties are compounded when both forces are operating simultaneously on the economy; likewise when, as indicated earlier (fn. 6), the two forces take turns so that the impact of neither has time to build up a measurable impact on the level of employment.

²⁸ While the detailed model cannot, of course, be readily applied to real situations for lack of data, the deduced results and general framework are readily applicable.

²⁹ I am using quarterly changes in real GNP minus changes in business inventories as well as changes in the individual components of GNP as a crude indicator of changes in demand. These data are taken from [9].

change in the number of unemployed occurred. In fact, he assumes that the whole period, 1950-1957, was one of "relatively full employment" implying by this that no significant variations occurred. He arrives at this conclusion from observation of annual average rates of unemployment.³⁰ Certainly, annual average rates are inadequate for the analysis of this problem.

From his judgment of "relatively full employment," Gallaway argues that the unqualified wage-push hypothesis requires either that (1) the income effects of the wage increase or (2) some exogenous increase in demand just offset the unemployment effects of the wage-push. He rejects (1), though it seems a reasonable possibility at "relatively full employment," primarily on the grounds that government expenditures and government debt had increased from 1950 to 1957 and these constituted, in his opinion, the exogenous demand forces that give us "relatively full employment." However almost all of the increase in government expenditures over 1950-57 took place in 1951 and 1952; expenditures in 1957 were \$1 billion more than in 1952 and \$1 billion less than in 1953.³¹ Furthermore, the budget ran surpluses in 1956 and 1957. Had Gallaway examined the fiscal data for the period 1950-57 on a quarterly or annual basis and the unemployment data on a monthly or quarterly basis, I feel certain that he would have been led to a different conclusion at least for 1957.³²

R. T. Selden [7] supports, in part, the cost-push explanation for 1957 though for a somewhat different reason.³³ He disaggregates the increase in wholesale prices into its various subgroups finding that the largest price increases were experienced in the farm and processed food subgroups. A major factor in the rise of meat prices, he finds, was the voluntary withholding of livestock from the market which, he argues

³⁰ I would even disagree with this conclusion on the basis of his annual average rates. Certainly his 2.51 per cent for 1953 is of a different order of magnitude from the 5.01 per cent for 1954 [4, p. 968 n.].

³¹ Expenditures fell sharply in 1954 and rose again from 1954 to 1957.

³² Gallaway may have been thinking of the long-run impact of cost-push forces. It seems clear, however, that the long-run impact of cost-push cannot be detected easily in terms of its impact on the level of unemployment for two reasons. First, since the strength of cost-push is largely a function of the level and direction of change of unemployment, the greater the unemployment effect of the cost-push, the quicker the process peters out as labor loses the power to push up wages. The unemployment criterion is only useful in the short run. Second, if substantial cost-pull develops, the impact of the wage increase on unemployment will be negligible or unemployment may even be reduced. It would seem, incidentally, that the larger the cost-pull, the greater the impact of wage-pushes on the long-run level of wages and prices and the lower the price stability of the economic system.

³³ Selden's arguments are very involved because he is concerned with demonstrating the relative effects of cost and demand changes on the quantity and especially the velocity of money. His general line of reasoning and his arguments are much too long and detailed to be reproduced here.

and I think correctly, is analogous to an autonomous cost change such as a wage-push. I would accept this as an additional factor in favor of the cost-push for 1957.

I find it hard to characterize the year 1955 as "inflationary."³⁴ The CPI showed no rise at all and the WPI rose by only a fraction of one per cent. With respect to demand, gross private investment and personal consumption rose very sharply while net foreign investment and government purchases remained fairly stable. At first glance, one would be inclined to attribute the less than one per cent rise in prices, 5 per cent rise in wages, decline in unemployment from 4.9 to 4.1 per cent, and $2\frac{1}{2}$ per cent increase in hours worked per week entirely to the sharp increase in autonomous demand as evidenced primarily by the substantial increase in private domestic investment and in personal consumption. Certainly, these were the major inflationary forces. The anomalously small increase in prices may be attributed to the enormous increase in output per man-hour in manufacturing which occurred in 1955—an increase of 5.8 per cent [3, p. 366]. This productivity increase completely offset the wage-rate increase in manufacturing.

But consider the following additional factors and interpretation. The large rise in productivity tended to blunt the cost-push negative effects and to accentuate the cost-pull positive effects of the wage increase on employment. The large cost-pull which resulted thereby contributed to the increase in personal consumption which occurred in 1955 and which on casual observation appeared to have been entirely demand-pull. To put it another way, had productivity not increased by such a large percentage in 1955, prices would have risen more rapidly due to the wage increase and unemployment would not have declined by as much—might even have risen. This would have been a picture much closer to that of a cost-push inflation. In other words, there may well have been some cost-push but its impact was disguised by the rapid increase in productivity which reduced the price rise and contributed through cost-pull to the large increase in spending. While I agree that demand factors predominated in the 1955 inflation, I do feel that the very strong cost-pull forces cannot be ruled out as an important contributing factor.

The year 1956 was definitely inflationary with a 6 per cent increase in wage rates, 3 per cent in CPI and 4 per cent in WPI. Real expenditures increased by a small amount, money expenditures rather sharply over the year. The level of unemployment remained stable, at about 4 per cent fluctuating by only fractional amounts as did average weekly hours worked. In terms of the crude indicator developed in this paper,

³⁴ Selden also considers, in detail, the 1955-56 period. He feels that whatever inflation characterized this period was demand inflation. I agree, on the whole, but think that cost factors were also partly responsible in 1955 though not in 1956.

it would be impossible to choose on the evidence presented so far, between cost and demand forces. Two additional factors must be introduced: (a) the productivity increase in 1956 of .4 per cent was the smallest of the postwar period; (b) the increase of 1.9 million in the nonagricultural labor force was the largest since 1947. If there had been any cost-push, the small increment to productivity would imply relatively little positive cost-pull, and therefore some unemployment effects. That unemployment did not decline despite this and the more than 3 per cent rise in the nonagricultural labor force implies that strong demand forces were at work and were predominantly responsible for the wage and price rises which occurred.

Selden argues that if a cost change is responsible for a price increase, output should fall. On the other hand, if demand is responsible, output should rise. He then proceeds to correlate changes in industrial output with changes in wholesale prices on an industry-by-industry basis for 1955-56. He finds a positive relationship between the two variables, with a correlation coefficient of $+ .4$. Since output typically increases in industries which experienced price increases, he concludes that demand-pull is responsible for the inflation.

While Selden showed considerable ingenuity in devising his test (which is only one part of his argument) I have one reservation regarding its general validity. Selden's method takes no account and shows no awareness of the possibility of cost-pull effects associated with a cost-push, and these may have been unusually strong, as we have already noted, in 1955 because of the large increase in productivity in that year. To take an extreme example, if there were no increase in autonomous demand, and if wage increases were just offset by productivity increases, the resulting cost-pull would still cause increases in prices and output and these would be positively correlated.

There is another type of argument from disaggregated data which is even more fraught with pitfalls. Many persons have argued that the recent inflation must have resulted from demand-pull because the largest price increases have occurred in industries (e.g. the service industries) least dominated by strong unions and oligopolistic producers. But the amount by which price will rise and output will change in a particular enterprise due to rising wages will depend not only on the size of the wage increase in that enterprise but also on (1) the original level of profits in each industry, (2) the increment in output per man-hour in each industry since the previous wage increase, (3) the percentage of wage cost to total cost by industry, (4) the change in aggregate demand due to the national wage increase (cost-pull), and associated with this (5) the income-elasticity for the product of the particular enterprise.

APPENDIX

1. Write national income, Y_t , as the sum of wages and profits:

$$(a.1) \quad Y_t = W_t + V_t.$$

On the expenditure side (income = expenditure) write:

$$(a.2) \quad Y_t = C_t + I_t + X_t + G_t.$$

where all components of Y_t are for domestically produced goods.

Write consumption and investment equations:

$$(a.3) \quad C_t = mW_{t-1} + vV_{t-1}.$$

$$(a.4) \quad I_t = v'V_{t-1} + I(a)_t.$$

Let the wage rate rise, increasing total wages paid and changing the level of profits:

$$(a.5) \quad W_{t-1} = W_{t-2} + N_{t-2}\Delta w(1 - e).$$

$$(a.6) \quad V_{t-1} = V_{t-2} - kN_{t-2}(\Delta w - \Delta r)(1 - e).$$

Substituting (a.5) and (a.6) in (a.3) and (a.4):

$$(a.7) \quad C_t = m[W_{t-2} + N_{t-2}\Delta w(1 - e)] \\ + v[V_{t-2} - kN_{t-2}(\Delta w - \Delta r)(1 - e)].$$

$$(a.8) \quad I_t = v'[V_{t-2} - kN_{t-2}(\Delta w - \Delta r)(1 - e)] + I(a)_t.$$

Substituting in (a.2) and consolidating:

$$(a.9) \quad Y_t = mW_{t-2} + \bar{v}V_{t-2} + mN_{t-2}\Delta w(1 - e) \\ - \bar{v}kN_{t-2}(\Delta w - \Delta r)(1 - e) + I(a)_t + X_t + G_t.$$

The change in national expenditure due to a wage increase and assuming no change in autonomous expenditures is:

$$(a.10) \quad Y_t - Y_{t-1} = mN_{t-2}\Delta w(1 - e) - \bar{v}kN_{t-1}(\Delta w - \Delta r)(1 - e).$$

Equation (a.10) is the same as (8) in the text.

2. The model presented can be viewed as consisting of the following:

Unknowns: $Y, C, N, V, W, I, e, k, d, d', v', w$.

Exogenously given parameters and variables: $X, G, I(a), m, v$.

Equations: The 12 equations which determine the unknowns are (1)–(5); (a.1)–(a.4);

$$(a.11) \quad W = Nw$$

$$(a.12) \quad v' = f_{v'}(N, V)$$

$$(a.13) \quad \Delta w = f_{\Delta w}[N, \Delta X, \Delta G, \Delta I(a)].$$

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THE MEANING AND VALIDITY OF THE INFLATION-INDUCED LAG OF WAGES BEHIND PRICES

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Many economists write as if the proposition that inflation causes prices to rise faster than wages were well established. From this proposition at least two important classes of inferences have been derived.

1. A lag of wages behind prices as a result of inflation produces extraordinarily large business profits. These swollen profits generate a high rate of capital formation. In this role, the wage-lag axiom constitutes the foundation of a theory of industrial development.

2. The lag of wages behind prices caused by inflation accentuates oscillations in the general level of economic activity. The failure of wages to keep pace with prices reinforces disequilibrating movements in the general level of economic activity. In this capacity, the wage-lag axiom functions as an integral part of both overinvestment and underconsumption business cycle theories [14, p. 137 and ff.].

The contention that inflation causes real wages to fall appears frequently in the literature of economics. Those who make this contention argue in effect that inflation produces a negative correlation between real wages on the one hand and money wages and prices on the other. As a practical matter, it is extremely difficult to employ this idea as a tool of analysis for understanding observed movements of time series of wages and prices. This difficulty stems from the fact that, as almost everyone would agree, the level of real wages can be affected by such real forces as the relative supplies of labor and capital, the quality of the labor force, the pattern of final demands in the economy, and the state of the arts. Furthermore, increases in the general price level can be produced by changes in the real stock of goods, e.g., by droughts, plagues, wars, etc., even with a fixed money stock. For any time series of real wages, there exists a fantastically difficult problem of imputing changes in the level of real wages to one or the other of two classes of

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variables, i.e., real or monetary forces. Only if one is able to abstract from the effects of real forces can one determine the effect of inflation upon an observed time series of real wages.

To illustrate this problem, consider the data showing real wages, money wages, and prices in the United States since 1889 [39, pp. 15-16]. These data indicate a high positive correlation between real wages on the one hand and money wages and prices on the other. Are these positive correlations to be interpreted as evidence against the proposition that inflation causes real wages to fall? Surely not. Real wages rose during this time, according to most observers, because of the per capita increase in capital, improvements in technology, and improvements in the skills of the labor force. Those who believe that inflation causes real wages to fall would not deny this. Their position would be that real wages rose despite inflation and that if the effects of real forces upon real wages were properly abstracted, one could observe a fall in real wages attributable to inflation.¹

I. Some Alternative Wage-Lag Hypotheses

What, then, is the wage-lag hypothesis? To answer this question, we have turned to the works of those economists who have used this idea. The most important "explanation," importance being measured by either the extent to which it has been used or its deviation from the way economists explain behavior in nonlabor markets, is the belief that wages have more "inertia" or "sluggishness" than other prices because of custom, weak bargaining power of labor, or lack of foresight of workers. For example, Hamilton states: "The chief factor in the failure of wages to keep pace with soaring prices in the second half of the eighteenth century was the 'natural' inertia of wage movements in both directions. History records few instances of wage movements in unison with rapidly changing commodity prices" [18, p. 259]. And:

There have been no such offsets to the strong tendency during most of the last four hundred years for wages to lag behind prices whenever they were rising. This lag has benefited capitalists as a class at the expense of laborers as a class and awarded gains that dwarf into insignificance the profits from inventory appreciation and from declines in the real value of debts. A tendency for wages to lag behind falling prices has inflicted losses on businessmen, discouraged saving and investment, and aggravated commercial crises [20, p. 327].

Mitchell also contended that an imperfection exists in the labor market. He wrote:

¹ Or for a less recent inflation, consider the Black Death period. During this time prices rose and real wages rose. Clearly what explains this phenomenon is the decrease in the stock of labor which also produced a fall in rents. See Lipson [30, pp. 93 and ff.].

In the '60's and, though in somewhat less degree, in the '70's, the labor market of the United States was one in which individual bargaining prevailed. Now the individual laborer is a poor bargainer. He is ignorant of the possibilities of his situation, exposed to the competition of others with the same disabilities, more anxious to sell than the employer to buy. Moreover, custom in the form of rooted ideas about what is a "fair wage" has a peculiarly tenacious hold upon the minds of both parties in the labor market, weakening the wage-earner's aggression and strengthening the employer's resistance [35, pp. 275-76].

- In his study of the Civil War, Mitchell concluded: "All of the statistical evidence that has been presented in the preceding pages supports unequivocally the common theory that persons whose incomes are derived from wages suffer seriously from a depreciation of the currency" [34, p. 347]. Basically, the rationale for this position is that there exists a flaw in the labor market which, during times of inflation, lowers the wage rate below the marginal product of workers. In effect Mitchell and Hamilton are saying that the same principles economists use in explaining what happens in other markets are invalid for explaining what happens in labor markets during inflation.²

Bresciani-Turroni enunciated, in his famous study of German inflation, a hypothesis that could explain declines in real wages during inflation and be consistent with a perfectly functioning labor market.³ This hypothesis rests on the postulate that employees, as a condition of employment, are almost invariably creditors of their employers. And as creditors, employees lose to their employers for the same reason that creditors generally lose to debtors as a result of inflation. Therefore, even if wage rates correctly represented the marginal product of workers, the fact that wages accrue, i.e., that wages are paid after they are earned, implies that workers extend credit to their employers and incur a loss on this account.

There exists strong *prima facie* evidence for accepting the wage-accrual hypothesis of Bresciani-Turroni. This explanation rests upon a debtor-creditor relationship that is essentially similar to debtor-creditor relationships between, say, department stores and their charge customers, finance companies and the credit purchasers of automobiles and other appliances, corporations and their bond holders, etc. Since there already exists evidence that supports the belief that interest rates are biased downward during inflation, because of the public's lack of

² Explanations of this type may be found in [31, p. 7 and ff.] [41, p. 213] [6, p. 380] [29, p. 222] and [32, p. 88].

³ "In fact, wages were fixed on the basis of an index number of prices which, at the time of payment, no longer represented actual conditions" [7, p. 310]. It failed to represent actual conditions because of the bias in interest rates. Also he argued that wage earners lost because they held cash during inflation [7, p. 302]. Both of these are of course special cases of the proposition that creditors lose during inflation.

knowledge of the course of future prices, there appears to be a reasonable basis for accepting the proposition that wealth is transferred from employees to employers when inflation occurs.⁴

As a practical matter, it does not appear that this relationship between employees and employers, at least in modern times, has the potential for transferring a great deal of wealth from employee to employer. Consider a case that is most favorable for sustaining the proposition that accrued wages constitute an important source of business profits during inflation. Assume that cash is acquired for wage payments at the very instant these payments are made by a business firm. Therefore this firm may be regarded as a consistent net debtor with respect to its employees.

What can be said about the magnitude of such profits under these assumptions? Of all industrial firms listed on the New York Stock Exchange in 1952, approximately 200 reported the size of their aggregate wage bills, or more properly the size of their aggregate wage and salary bills, for at least one year between 1939 and 1952. Among these 200 firms, the ratio of total annual wages to equity (equity being measured by the market value of outstanding shares) ranged from a low of .1 to a high of about 4, depending upon firm and industry. If it is assumed that wages are paid biweekly, then the average amount of wages and salaries accrued is $1/52$ of the annual wage bill. Consequently it follows that accrued wages range from a low of about .2 per cent to 8 per cent of equity. This analysis implies that if the price level doubled in any given year, the real value of stock prices would rise from a minimum of .1 per cent to a maximum of 4 per cent.⁵

Using this same debtor-creditor relationship, Fisher had earlier set forth still another explanation of why real wages would fall during inflation. Like the Bresciani-Turroni explanation, Fisher's was consistent with a perfectly functioning labor market. Fisher contended that relations between employer and employee can be viewed as being contractual, just as are economic relations between, say, bondholders and those who incur bonded debt [10, p. 185 and *ff.*]. The same lack of foresight that would lead to too low an interest rate to permit debtor-creditor relations to be unaffected by inflation would lead to an effective

⁴ This of course does not imply that business firms gain through inflation. Such a statement would be correct only if an examination of all of their debtor-creditor relations, of which relations with employees are only a part, revealed that business firms are on balance debtors. On this point, as well as for evidence that interest rates are biased during inflation, see Kessel [23, p. 128]. Nor does the "bias" of interest rates imply any defect in the capital market; instead it reflects people's inability to predict future prices.

⁵ The Bresciani-Turroni hypothesis also appears in Meyer [33, p. 17]. "Creditors lost in inflation. Wage-earners and salary-earners normally work before they are paid. They lend their labour until pay day; their work is work given on credit." Meyer also asserts that wages lag because of contractual arrangements between employers and employees.

wage below the marginal product of labor when prices are rising. Only at the time wage contracts are signed would wages be equal to the marginal product of labor. Between contract negotiations, real wages would fall as a result of rising prices.

Prima facie evidence does not support this hypothesis. Wage contracts are typically nonenforceable when broken by employees. Consequently, the legal reasons for arguing that contracts between employers and employees are on a par with contracts between creditors and debtors are of dubious validity. As far as employees are concerned, wage contracts have generally been continuously renegotiable, at least until relatively modern times. Employees can almost always leave their current jobs in favor of alternative employment possibilities in complete freedom from legal sanction by employers. Consequently, in the absence of other evidence there is very little basis for accepting Fisher's hypothesis.

However, there is more to a substantive hypothesis than its logical structure. In its broader aspects, the Fisher hypothesis implies that during inflation there exists a differential in the movements of wage rates of workers under contract as compared with workers employed without contract. It also implies that the longer the life of a contract, the greater the differential in the movements of real wages during inflation. No evidence is contained in this paper for evaluating these two implications.

If one abandons a legalistic frame of reference and argues, as Fisher has, that custom plays a great role independent of contractual arrangements, then this hypothesis becomes indistinguishable from the argument of Hamilton and Mitchell, namely, that a flaw exists in the labor market which manifests itself during times of inflation by a fall in real wages.

The use of inflation as a means of taxation appears to have created a belief that inflation causes real wages to fall. Inflation is a means of taxation, and has been used by those who control the stock of money as an alternative to explicit forms of taxation, such as income taxes, excises, tariffs, etc. Using their power to create money, governments have exchanged money for real resources. Such an exchange reduces the volume of real resources available to the private sector of the economy. The mere existence of an exchange of this character has led many observers to conclude that a fall in real wages is necessarily implied [e.g. 25, pp. 171-74]. Yet it can and has been shown that taxation through inflation is consistent with no reduction in real wage rates.⁶

⁶ A discussion of the mechanism by which the government acquires resources from the rest of the community through inflation has been presented in [1]. For the first published analysis of this mechanism that the authors have encountered, see the revised portion of Friedman [11, p. 263]. See also Cagan [8].

Inflation constitutes a tax upon monetary wealth and not upon wages or other factor incomes. This tax affects the real functional returns of the cooperating agents of production if inflation is anticipated, i.e., when the increased cost of holding money caused by rising prices is recognized and enters into the calculations of the community. Under these circumstances, both velocity and the nominal or money rate of interest rise. These higher costs of using money are ultimately reflected in a rise in product prices relative to the sum of the returns to the cooperating agents of production. Whether or not real wages fall depends upon the cross elasticity between the price or cost of holding money and the quantity of labor demanded. If one is prepared to argue that capital is a better substitute for money than labor, and to assume that the alternative to inflation as a means of taxation is no tax or a wealth or income tax, then the argument that anticipatory inflation can cause real wages to fall can be sustained.

However if inflation is not anticipated, then the losses of the money holders are on a par with an *ex post facto* penalty or Knightian profits and do not affect resource allocation. In general, it appears that the inflations associated with our Civil War in the North and our two world wars were unanticipated. If excise taxes or turnover taxes are regarded as the alternative to taxation through an unanticipated inflation, then inflation implies a higher level of real wages than would otherwise be true.

II. *The Empirical Evidence*

The remainder of this paper falls into two parts: (1) a review of the statistical evidence that has been used to support the Mitchell-Hamilton hypothesis and (2) a new test of this hypothesis based on differences in the labor intensiveness of business firms and the performances of their stock prices during inflation.⁷

⁷ The field of income and employment theory contains still another hypothesis that implies the existence of a lag of wages behind prices when prices are rising. It stems from the observation that less than full employment, where full employment is defined as a labor market in which everyone who wants a job at the prevailing wage rate can find one, implies nonprice rationing of employment opportunities. This is consistent with an infinitely elastic supply function of labor that relates the quantity of labor offered with money wages if rising prices will restore full employment. Under these assumptions, increases in prices at times of less than full employment imply a fall in real wages.

This hypothesis is clearly relevant to the present discussion, if it is relevant at all, only for inflations or portions of inflations associated with less than full employment. Since the authors cited believe that inflation causes wages to lag behind prices independently of whether full or less-than-full employment exists, this is not a hypothesis they considered extensively although it appears in the work of Mitchell and Bresciani-Turroni.

This model leads to difficult questions. One is: Shouldn't the wages of unemployed workers be considered in the wage index? If they are included in the wage index, then it is not clear that real wages decline under these circumstances. Another difficulty is that we do not know enough about how an economy returns to full employment to impute to inflation a fall in real wages of those continuously employed. Possibly real supply conditions have not

What is the empirical evidence used to support the hypothesis that inflation, independently of real forces, causes real wages to fall when prices are rising? Major data used to support this hypothesis have been collected for six inflationary episodes: (1) the period from 1350 to 1800 in Spain, (2) the early days of the industrial revolution in England, (3) the U.S. Civil War in the North, (4) the U.S. Civil War in the South, (5) the German inflation following the first world war, and (6) the inflation in the United States associated with the first world war.

A. *Spanish Data*

E. J. Hamilton probably has contributed more to the acceptance of the hypothesis that inflation causes real wages to fall than has any other single economist.⁸ His evidence consists almost entirely of time series of wages and prices. In order to use such data as evidence of a wage lag, the impact of real forces must be distinguished from that of inflation. Hamilton is not unaware of this difficult problem of imputation. Throughout his monumental three-volume work on Spanish wages and prices, which covers the interval from 1350 to 1800, are references to real forces and their impact upon the price level and real income [17, pp. 100-4]. Yet, as far as we can discover, he consistently forgets about real forces when using his time series to test the hypothesis; any fall in real wages when prices are rising he interprets as evidence supporting the wage-lag hypothesis.

Yet even with this implicit assumption that real forces are constant during inflation and consequently any change in real wages is attributable to inflation, Hamilton's data in his study of Spanish wages and prices fail in large part to support his thesis.⁹ Of the three areas studied in the first episode from 1350-1500, Valencia, Aragon, and Navarre, only Navarre incurred inflation during this time. He concludes: "The greatest anomaly disclosed by the present study is the complete failure of wages to lag behind prices in any of the kingdoms during a single period of upheaval. In fact, Navarrese wages advanced much faster than prices in the last decade of the fourteenth century" [17, p. 203].

For the second period, 1501 to 1650, he concludes: "With few interruptions, the trend [in real wages] was downward from 1520 to 1600" [16, p. 280]. And, "The calamitous depreciation of the inflated Castilian vellon and debased Valencian silver coinage in 1623-1650 impaired the economic welfare of workers no less catastrophically than had the

changed but demand conditions have changed. Real aggregate demand could increase, through an increase in the nominal monetary stock, and with an infinitely elastic aggregate supply function, full employment would be restored with no fall in real wages.

⁸ This view runs through most of his works. See particularly [18, p. 256] [20, pp. 335-36].

⁹ In the ensuing examination of his statistical results, the reported data will be taken at face value. However, the statistical procedures employed merit more extended critical examination than is possible here.

TABLE 1—COMPOSITE INDEX OF REAL WAGES*
Base 1571–1580, Period 1501–1650

Year	Year	Year	Year	Year	Year
1501 112.78	1526 105.66	1551 100.27	1576 103.47	1601 100.88	1626 101.15
1502 115.55	1527 102.26	1552 98.64	1577 106.52	1602 108.68	1627 97.82
1503 118.96	1528 106.62	1553 102.76	1578 102.95	1603 112.80	1628 102.44
1504 111.56	1529 100.15	1554 108.40	1579 97.81	1604 111.94	1629 104.22
1505 108.62	1530 91.35	1555 110.41	1580 102.86	1605 112.10	1630 109.31
1506 92.47	1531 94.39	1556 109.60	1581 104.43	1606 116.80	1631 110.89
1507 99.68	1532 99.40	1557 100.66	1582 101.12	1607 119.60	1632 107.79
1508 102.75	1533 106.25	1558 101.75	1583 100.09	1608 121.35	1633 111.11
1509 117.06	1534 102.43	1559 111.05	1584 102.48	1609 127.83	1634 113.47
1510 127.84	1535 114.03	1560 110.75	1585 102.22	1610 125.49	1635 114.60
1511 120.80	1536 104.49	1561 102.02	1586 106.01	1611 130.56	1636 111.63
1512 126.85	1537 108.19	1562 96.50	1587 103.14	1612 127.96	1637 105.83
1513 125.48	1538 99.82	1563 100.96	1588 111.63	1613 128.09	1638 105.86
1514 122.04	1539 104.06	1564 102.12	1589 107.31	1614 122.85	1639 110.81
1515 118.56	1540 102.30	1565 101.27	1590 105.85	1615 126.57	1640 111.59
1516 120.62	1541 103.73	1566 99.22	1591 107.70	1616 121.45	1641 106.13
1517 123.87	1542 98.23	1567 103.37	1592 104.12	1617 119.81	1642 98.07
1518 118.36	1543 97.24	1568 105.80	1593 107.07	1618 122.90	1643 101.30
1519 119.77	1544 101.45	1569 108.14	1594 106.47	1619 127.08	1644 102.45
1520 125.56	1545 105.14	1570 105.56	1595 106.29	1620 121.61	1645 105.91
1521 112.61	1546 98.36	1571 99.58	1596 103.84	1621 122.11	1646 102.07
1522 104.81	1547 99.28	1572 100.02	1597 99.00	1622 121.85	1647 103.10
1523 109.89	1548 95.54	1573 97.40	1598 93.02	1623 120.16	1648 98.20
1524 109.36	1549 93.61	1574 100.11	1599 91.40	1624 114.64	1649 97.53
1525 106.87	1550 97.61	1575 94.18	1600 91.31	1625 113.82	1650 93.30

* Reproduced from Hamilton [16, p. 278], with permission of Harvard University Press.

influx of American gold and silver in the last eight decades of the sixteenth century" [16, p. 282]. However again, and once more holding real forces constant, Hamilton's conclusion is not supported by his data. While it is strictly true that real wages as reported by Hamilton were lower in 1600 than they were in 1520, the trend he reports is absent from his data. The reason he gets the results that he does is that 1520 is a year when real wages were exceptionally high when compared with the years immediately preceding and succeeding 1520. On the other hand, 1600 appears to be a year when real wages were exceptionally low when compared with the years immediately preceding and succeeding 1600. If real wages in 1522 are compared with real wages in 1602, then one can conclude that real wages rose. The results Hamilton obtained can be obtained from random series. There is no downward trend in real wages nor any coincidence of wages lagging with inflation.¹⁰ Hamilton's data for the episode are reproduced in Table 1.

¹⁰ Alternatively one might say that the base year for Hamilton's observations had a strong plus random factor and the final year a strong minus random factor, and what he attributes to inflation can be attributed very easily to sampling error. In statistical jargon, he commits the regression fallacy.

In his third volume, Hamilton covers the time interval from 1651-1800 and he finds that real wages declined in the urban areas, Madrid and Valencia, in the second half of the eighteenth century. What happened to real wages for the country as a whole is unclear since real wages rose in some rural areas and presumably the country as a whole was predominantly rural [19, p. 210]. The second half of the eighteenth century was characterized by rising prices. However it was also a time when the Spanish population was increasing sharply; it doubled during this century, and was associated with migration from rural to urban areas [19, p. 216]. Consequently one would expect, in the absence of any imperfections in the labor market, that such a population increase would lower real wages. Yet Hamilton did not disentangle the effects of this population increase from the effects of inflation upon real wages, and he concluded in the final sentence of his last volume:

By involuntarily sacrificing real income through the price-wage squeeze, the laboring class bore the burden that implemented material progress, just as laborers and peasants in Soviet Russia, sacrificing through governmental directives, have largely financed the mechanization of industry that was instrumental in the recent expulsion of German invaders [19, p. 225].

B. English and French Data

Hamilton buttresses his conclusions about the effect of inflation upon industrial development by citing similar effects for England and France during inflations that occurred in these countries. Specifically, in his third volume he says:

The concurrence of profit inflation and of rapid economic development in England and France tends to confirm the thesis that the lag of wages behind prices was an important factor in the great material progress in Spain during the second half of the eighteenth century [19, p. 224].

Again, even if the potential impact of real forces upon wage-price relationships is ignored, can it be said that wages fell during the inflation in England?

Hamilton's study of the movement of prices and wages in London between 1729 and 1800 indicates that real wages fell.¹¹ Mrs. Gilboy, however, who also studied prices and wages in England at this time, supports Hamilton's findings of fact but not his conclusions [12, pp.

¹¹ [18, p. 259]. One of the relevant problems for analyzing Hamilton's data, which he fails to discuss, is the fact that he has more observations, typically, in his price than in his wage index. Consequently, if the price and wage observations change with the same degree of frequency, say once a year, it will appear, falsely, as if wages were lagging behind prices. This error accounts for much of the intuitive appeal of the wage-lag hypothesis. If during inflation one sees prices moving up day by day whereas one's own wage rate changes once a year, the conclusion that wages lag behind prices during inflation is difficult to resist.

TABLE 2—INDEX NUMBERS OF PRICES AND WAGES IN ENGLAND, 1500-1702*
(Index for 1451-1500=100)

Period	Prices	Wages
1501-1510	95	95
1511-1520	101	93
1521-1530	113	93
1531-1540	105	90
1541-1550	79	57
1551-1560	132	88
1561-1570	155	109
1571-1582	171	113
1583-1592	198	125
1593-1602	243	124
1603-1612	251	124.5
1613-1622	257	134
1623-1632	282	138.5
1633-1642	291	152.5
1643-1652	331	175
1653-1662	308	187
1663-1672	324	190
1673-1682	348	205.5
1683-1692	319	216
1693-1702	339	233

* Reproduced from Hamilton [15, p. 352], with permission of London School of Economics and the author.

191-215]. She found that real wages fell in London and rose in the north of England [12, pp. 191-215]. Therefore she concluded: "Generalizations as to what happened to English wages as a whole must at present meet no little skepticism."¹² Her findings were particularly damaging to Hamilton's interpretation of the implications of a fall in real wages during inflation. Capital formation in the north of England was especially high, whereas Hamilton's hypothesis implies that capital formation ought to have been particularly low in this area.¹³

Hamilton has also examined data for an earlier period of English history, 1500 to 1702 [15, p. 351, Chart 1]. Will these data support the hypothesis that inflation causes wages to lag behind prices if one abstracts from the effects of real forces? (See Table 2.) Taking the period as a whole, Hamilton is right. Real wages declined. However, virtually all of the decline occurred during the first 50 years of this period, and it is unclear whether this shorter time interval ought to be regarded as being on net balance inflationary or deflationary. Prices

¹² [12, p. 227]. In a paper dealing with this same issue, Mrs. Gilboy puts the case even more forcefully. "Sufficient data are not at present available to make any statements concerning the movement of real wages in England as a whole for this period" [13, p. 141].

¹³ For a partially overlapping time period, 1790-1830, Ashton does not believe that real wages declined [4, p. 158].

TABLE 3—INDEX NUMBERS OF PRICES AND WAGES IN FRANCE, 1500-1700*
(Index for 1451-1500=100)

Period	Prices	Wages
1501-1525	113	92
1526-1550	136	104
1551-1575	174	103
1576-1600	248	113
1601-1625	189	113
1626-1650	243	127
1651-1675	227	127
1676-1700	229	125

* Reproduced from Hamilton [15, p. 353], with permission of London School of Economics and the author.

were about 17 per cent lower at the end of these 50 years than they were for the base observation. The first 40 years were inflationary, and real wages fell. However, the next 10 were deflationary, and real wages fell even more. Again these data will not support even this very simple conception of the wage-lag hypothesis.¹⁴

Tucker studied real wages in London during the latter half of the eighteenth century but has no data for the country as a whole [40]. In view of Gilboy's findings, his data are not of great relevance for England as a whole. Tucker, for reasons quite different from Hamilton's, was interested in testing the hypothesis that real wages fall as a result of rising prices. However, every time he observes a fall in real wages, he is able to explain this fall by real factors such as poor crops, resources consumed by wars, etc. [40, p. 82, for example]. Yet he ignores these explanations when drawing his conclusions.

For France, Hamilton does have data that unambiguously show that real wages fell [15, p. 353]. (See Table 3.) However his explanation of why they fell is not supported by related evidence. His hypothesis implies that the larger the fall in real wages, the greater the rate of industrial development. Differences in the rates of capital formation between England and France ought, therefore, to be related to differences in

¹⁴ Using time series of wages and prices as Hamilton does involves the vexing question of how to choose one's starting point or base observation. Presumably one wants to start observations when prices start to rise. But the trough of a price series is usually determined by random components. This produces a transitory peak in the real wage series; the subsequent decrease, if interpreted as a lag, provides an example of the regression fallacy. Only by averaging out transitory or random variations about some turning point can one avoid part of this problem.

Only after acceptance of this paper for publication did we discover the following corroboratory conclusion, "It follows that Keynes was misled when he argued in the *Treatise* that the general rise in prices had stimulated industrial growth by widening profit margins," in E. H. Phelps Brown and S. V. Hopkins, "Wage-rates and Prices: Evidence for Population Pressure in the Sixteenth Century," *Economica*, Nov. 1957, N.S. 24, 299.

either the observed fall in real wages or the rates of change of prices. Nef was unable to explain differences between the rates of capital formation in France and England with Hamilton's hypothesis.¹⁵ Similarly, the failure to find "correlation between inflation, or its absence, and variations in the rate of economic growth" has led another student of industrial development, Felix, to reject Hamilton's theory of development.¹⁶

C. *The Civil War in the North*

Mitchell's basic time series of wages and prices for the North during the Civil War [34] are substantially better than the data for the early days of the industrial revolution. And there is little doubt that real wages truly fell during the Civil War; most of Mitchell's results cannot be rationalized as an artifact resulting from the choice of the time period said to be inflationary. Moreover, these data [34, p. 343] indicate that a substantial fall in real wages occurred.

One might quarrel with Mitchell's use of a wholesale price index as a deflator of real wages. This index was in large part composed of commodities like opium, mercury, zinc, soda ash, tin plate, blue vitriol, etc. A mere count of such items indicates that an unweighted index overrepresents their effect on the cost-of-living index. Rent, as is typically the case for wholesale price indexes, was absent. But it is easy to make too much of this point. Mitchell also computed a cost-of-living index for this period, and when either this index or one computed by Ethel Hoover, who used the same source material, is used as a deflator, the results still indicate a substantial fall in real wages, although smaller than when wholesale prices are used.¹⁷ (These data are reproduced in [24, p. 102].)

These results led Mitchell to conclude that: "All of the statistical evidence that has been presented in the preceding pages supports unequivocally the common theory that persons whose incomes are derived from wages suffer seriously from a depreciation of the currency" [34, p. 347]. They also led Mitchell to embrace the hypothesis that the labor market in the 1860's and 1870's was imperfect and that this imperfection was of a kind that virtually no serious student of industrial organization asserts exists in any other factor or product market [35, p. 276]. However, there is an alternative explanation of the fall in real wages in the North during the Civil War that is con-

¹⁵ J. Nef has collected evidence that fails to show a relationship between the magnitude of the lag and the rate of industrial development. He also has evidence that Hamilton's data exaggerate the magnitude of the fall in real wages [37].

¹⁶ See also Felix's discussion, "Hamilton's *Tour d'Horizon*" [9, pp. 457-59].

¹⁷ Ethel Hoover's index [22, p. 40, Table 1] is better than Mitchell's CPI because it uses more of the available data and better techniques for accounting for gaps in the data.

sistent with the way economists explain changes in price relationships in markets other than labor and it explains more of the relative price movements that occurred. Indeed, this explanation is consistent with the postulate that the labor market was operating perfectly during the inflation associated with the Civil War. Because it has none of the *ad hoc* character of the explanation employed by Mitchell and Hamilton, it is to be preferred.¹⁸

The outbreak of the Civil War substantially destroyed a triangular trading relationship among the North, the South, and England. The South earned foreign exchange through its exports of cotton, which accounted for roughly two-thirds of all U. S. exports. It, in effect, traded these foreign exchange earnings for Northern goods and services, and the North in turn used this foreign exchange to purchase imports. The outbreak of hostilities, in addition to destroying a mutually profitable trading relationship between the North and the South, presented the North with what would be regarded today as an extremely difficult balance-of-payments problem. This problem was aggravated by a capital flight of foreign investments during the early years of the war.

That this important problem confronting the North has been largely unrecognized is in large part to be explained by the fact that it was solved unobtrusively and successfully by a measure designed for a largely unrelated function. During the war, the North engaged in the printing of greenbacks; and the resulting inflation and the maintenance of convertibility at the prewar exchange rate were incompatible. In consequence, the North abandoned the gold standard in favor of an inconvertible paper standard and a freely fluctuating exchange rate which inadvertently solved the balance-of-payments problem.

The rise in the prices of imports relative to the rise in domestic prices and wages inevitably produced a fall in real factor incomes of all types. In so far as money wages are deflated by a price index that includes international goods, particularly imports, real wages decline. Since Mitchell's wholesale price index was more heavily weighted by imports than his consumer price index, the use of the former as a deflator produces a greater fall in real wages than does the latter. And of course if imports are excluded from his consumer price index and what remains is used as a deflator of money wages, a still smaller fall in real wages is measured.

However, this is only part of the explanation of the fall in real wages that Mitchell observed. The North, in addition to taxing through inflation, also employed turnover taxes and tariffs as means of war finance. The severity of these taxes increased during the course of the

¹⁸ The analysis which follows is more fully developed in a paper which appears elsewhere. See [24].

war. These taxes produced a divergence between the sum of the payments to agents of production and final product prices, because unlike retail sales taxes today, they became a part of final product prices. One would also expect for this reason to find that real wages, as measured by Mitchell, declined during the course of the Civil War.

Both the balance-of-payments problem and the turnover taxes would have produced a fall in real wages whether or not inflation had occurred. If the government's increased expenditures had not been financed by inflationary methods, some other means of taxation would have been required. Had tariffs or turnover taxes in any part replaced the inflation tax, an even greater fall in real wages would have occurred. The inflation tax implies that real wages were higher than they otherwise would have been.

D. The Civil War in the South

In a number of respects, Eugene Lerner's study of the Confederacy [27] [28] is parallel to Mitchell's work. In particular, both found that real wages declined. In neither case can most of the decline be attributed to the special characteristics of the base or terminal years for the time period defined as inflationary. Like Mitchell, Lerner attributes the fall in real wages to the lag of wages behind prices and accepts the extraordinary profitability implication of the wage-lag argument. "Prices rose much faster than wages in the Confederacy, and southern businessmen made large profits" [28, p. 31]. His paper contains virtually no evidence on profits.

The acceptance by Lerner of the wage-lag explanation of the fall in real wages is inconsistent with another interpretation of the events of the time that may be found in his own papers. He indicates that much of Southern capital was highly specialized to the production of cotton for an international market and that the Northern blockade sharply reduced the productivity of this capital. Lerner also reports that excises, either in the form of taxes or payments in kind, constituted an important means of war finance. In fact, Lerner implicitly presents a hypothesis that explains the fall in real wages by nonmonetary phenomena, but he explicitly accepts the thesis that the fall in real wages is attributable to inflation.

E. The First World War

Hansen's study is concerned with real wages and price changes in the United States from 1820 to 1923 and thus includes the inflation associated with the first world war. His position is much like that of Mitchell and Hamilton. "Rising prices cause a gap between the marginal productivity of the various factors employed by the entrepreneur

TABLE 4—HANSEN'S SERIES OF MONEY WAGES, COST OF LIVING AND REAL WAGES*
(1913 = 100)

Year	Index of Money Wages	Index of Cost-of-Living	Index of Real Wages
1910	94	94	100
1911	95	92	103
1912	98	96	102
1913	100	100	100
1914	102	102	100
1915	104	104	100
1916	118	111	106
1917	134	131	102
1918	168	159	106
1919	193	183	105
1920	232	208	112
1921	207	182	114
1922	201	168	120
1923	220	171	129

* Reproduced from Hansen [21, p. 32].

and the return that each receives. Indeed in such periods it is literally true that 'labor does not receive the full value of its product' " [21, p. 40].

However, even if real forces are assumed to be constant, as Hansen presumably assumed, the data do not support the wage-lag hypothesis. Indeed, they can be just as easily construed as undermining the hypothesis. Only if one chooses the year 1916 as a base and compares it with 1919 or 1917, can one show that real wages fell.¹⁹ (See Table 4.) If one uses 1913 as a base, and every succeeding year through 1920 as a terminal point, there is nothing to indicate a fall in real wages. In fact, Hansen's data show that real wages were almost 10 per cent greater in 1920 than in 1917.

These data of Hansen's contain an unfortunate bias in favor of the wage-lag hypothesis for the entire time interval with which he was concerned. Starting with 1890, Hansen uses weekly earnings rather than hourly earnings. If leisure is a superior good, and if real hourly earnings per capita rise, then weekly earnings understate real wages because of the substitution of leisure for income from work. Consequently, evidence collected to reveal a fall in real wages can be explained, at least in part, by the hypothesis that they were in fact rising. This bias is particularly unfortunate in a study of secular inflations

¹⁹ Hamilton in a parenthetical remark [15, p. 355] selects 1916 as a base year and observes that "... American profiteers reaped [income] from a similar divergence between prices and wages from 1916 to 1919." Hansen's data, reproduced as Table 5, show a less than one per cent fall in real wages for this period.

because the longer the time period considered, the greater the error it introduces into the calculations.

F. *The German Inflation*

Bresciani-Turroni contends that real wages declined as a *result* of the inflation in Germany following the first world war [7].²⁰ For the entire inflationary episode, he concluded: "But it may be said that on the whole the inflation generally favoured the entrepreneurs and the owners of material means of production, especially strengthening the positions of industrial capitalists; that it caused a lowering of the real wages of workmen . . ." [7, p. 286]. However, leaving aside questions of the impact of real forces upon real wages, Bresciani-Turroni's wage data, which consist almost exclusively of miners' wages, show that real wages sometimes declined and sometimes rose during the course of the inflation. Over the period as a whole, real wages did not fall [7, pp. 307, 309].

During the later stages of the inflation when the real value of the nominal stock of money declined sharply, or during the time that velocity increased at a rate more rapid than the rate of increase of the monetary stock, Bresciani-Turroni found that real wages fell. This rise in velocity was attributable to the recognition by the community of the increased cost of holding cash balances caused by rising prices. In this respect the German hyperinflation was unlike the inflations examined by Mitchell, Hansen, Hamilton, Gilboy, and Tucker, and it led to a marked reduction in the effective stock of capital in money form. Under these circumstances, the higher marginal cost of using money is an additional cost of doing business, and this implies that the share of the final output of the economy going to the other cooperating agents of production has decreased. Consequently, a fall in real wages during an inflation that is generally anticipated is consistent with a perfectly functioning labor market and does not imply an increase in business profits. In fact this analysis is consistent with Bresciani-Turroni's data on share prices, which do not support the thesis that business firms are extraordinarily profitable as a consequence of inflation [7, p. 253].

In general, it appears that a highly selective sampling from the population of all inflations has produced two important unambiguous cases of a fall in real wages for individual economies, those of the North and the South during the Civil War. For these cases, the wage-lag hypothesis has to compete with price theory. For the one case that has been studied in great detail, that of the North during the Civil War, price theory offers a more satisfactory explanation.

²⁰ "The increase in nominal wage rates was slower than the increase in prices caused by monetary inflation. In other words, real wages fell" [7, p. 305; also pp. 186-88]. This fall in real wages, according to Bresciani-Turroni, continued until the summer of 1922.

Whether or not available data indicate that real wages fell during inflation for some particular economy does not in itself establish or disprove the existence of an inflation-induced wage lag unless one assumes real forces to be inoperative. A time series of wages and prices can be made relevant evidence for testing the wage-lag hypothesis only after the effects of real forces are controlled. Unfortunately, the wage-lag theorists have generally ignored real forces. In the case of the North during the Civil War, the real forces ignored are substantial in magnitude and capable of producing the effects upon real wages imputed to the wage-lag hypothesis. When one considers the implications of this hypothesis, as the wage-lag theorists have not, the differences between industrial development in the North and South of England during the early days of the industrial revolutions, along with the Nef findings, must be regarded as still more evidence against this hypothesis.

III. *New Evidence*

In an effort to bring some new evidence to bear on the validity of wage-lag hypothesis, the annual wage bills for 56 industrial corporations listed on the New York Stock Exchange during the time interval 1940 to 1952 have been collected. These were all the industrial firms listed that reported their wage bills during this entire period.

The proposition tested was that the firms with large annual wage bills would experience an increase in profits (and wealth) relative to firms with smaller annual wage bills. That is, for any given rise in prices, sales and costs other than wages rise by the same proportion, whereas total wages [W] rise by less, e.g., by only some fraction, α , of the general price rise. Thus, $W(1 - \alpha)$ constitutes the size of the gain in profits for any firm. The relative magnitude of the gain is a function of the size of a firm's wage bill relative to its equity, as measured by its market value. In other words, the ratio of wages to equity is an indicator of the relative rise in stock prices attributable to a lag of wages behind prices.²¹

The ratio of wages to equity was obtained for each of the years from 1940 to 1952 through the use of the annual wage bill and the market value of stock outstanding at the end of the year. Unfortunately, testing for a relationship between the relative change in market value and the wage-to-equity ratio produces a bias in favor of finding a positive correlation because ratios with the same denominator are being corre-

²¹ Hamilton evidently regards the ratio of wages to total costs as the correct indicator of the size of the gain attributable to the lag of wages behind prices [18, p. 262]. However, two firms with identical equity values and identical ratios of wages to total costs might have different mark-ups and consequently different aggregate wage bills. (For example, consider a jewelry store and a supermarket grocery.) What is relevant is the size of the wage bill. And for interfirm comparisons, the relationship of the wage bill to total equity is the appropriate one.

lated. To reduce this bias, the annual wage-to-equity ratios, one for each year in the 1940 to 1952 period, were averaged for each corporation and then used as a predictor of relative changes in equity values.²²

The use of this average seemed reasonable because the differences between firms with respect to this average were significantly greater than the variations of any given year from the average for any firm. (The wage-to-equity ratios exhibited no trend over time.) The standard deviation of the ratio of wages to equity for any given year was about 20 per cent of the average for any firm. On the other hand, the average ratio varied, from firm to firm, from a low of 1 to a high of 7.²³ And because the interfirm variation was so much greater than the intrafirm variation, it seemed sensible to enlarge the size of the sample by using data for firms that reported annual wage bills for as little as two years of the time-span studied. This brought the sample to 113 firms. (A listing of the firms and other relevant data may be obtained through personal communication with the authors. Unfortunately, space constraints do not permit us to publish them here.)

By trying to detect a correlation between wage-to-equity ratios and changes in stock prices, the effects of a lag of wages behind prices caused by inflation can be disentangled from the effects of real forces upon real wages. After all, if one believes that real and monetary forces can operate independently and concurrently, the wage lag should be operative regardless of whether time series of wages and prices during inflation show that real wages fell, rose, or were constant. Given independence between real forces and the wage-to-equity ratio of a firm, this test ought to reveal the presence of the effects of inflation upon real wages.

According to the wage-lag hypothesis, the greater the wage-to-equity ratio, the larger should be the rise in equity values as a result of inflation.²⁴ To test whether or not this implication is in fact correct, firms were ranked according to their average ratio of wages to equity. The percentage increase in equity for firms with an average ratio of annual wages to equity below .5 were compared with those above 1. The results of this comparison are presented in summary form in Table 5. The

²² This also buys some insurance against committing the regression fallacy. If the wage-to-equity ratio at the beginning of the time period were used, firms with large wage-to-equity ratios might be those with transitorily small equity valuations and conversely.

²³ This ratio is affected by the financial structure. A firm with large debts and small equity financing will have a high wage-to-equity ratio and conversely.

²⁴ For example, if a firm's stock sold for \$4 at the end of 1939 and \$40 at the end of 1952, the equity increase is shown as a ratio, 10. Dividends paid are assumed to be reinvested into more shares of the same firm, and thus their growth was compounded. In this way, differences in dividend payout policy were held constant.

average equity rise was greater the lower the wages-to-equity ratio. Such a difference in the wrong direction clearly does not support the wage-lag hypothesis. Dividing the sample into two equal parts, one consisting of firms with the larger wage-to-equity ratios and the other of firms with the smaller wage-to-equity ratios, yields similar results.

In any attempt to impute the absence of causality to the absence of correlation between two variables, there always exists the danger that still another variable is so correlated with what is regarded as the independent variable that the effects of the independent variable upon the dependent variable are concealed. Relevant to this problem is the fact that a relationship is known to exist between the net monetary status

TABLE 5—MEAN EQUITY INCREASES OF FIRMS CLASSIFIED BY
WAGE-TO-EQUITY RATIO*

Ratio of Wages-to-Equity	Average Increase in Equity (1939-52)	Number of Firms	Variance
Under .5	8.41	34	48.4
.5 to .99	7.40	30	39.1
1. and over	6.19	49	26.5
"t" for $8.41 - 6.19 = +1.58$ $P(t \geq 1.26) = .12$			

* Sources: Moody's *Industrials* [36], *Annual Reports* [3], and *New York Times* [38].

of a firm and the relative change in its stock prices during inflation [23, p. 128]. The increase in the equity of the 43 firms in the sample that were net monetary debtors at least two-thirds of the time from 1940 to 1952 was greater than that experienced by the 29 firms in the sample that were net monetary creditors at least two-thirds of the time.²⁵ These results are consistent with known effects of debtor-creditor status upon stock-price changes during inflation.²⁶ Consequently, if firms that were large net debtors were also firms that had

²⁵ There were 43 debtor and 29 creditor firms. The mean rise for the debtor firms was 8.25 with a variance of 39.67; for the creditor firms, the mean was 5.94 and the variance 19.20. $\bar{x}_d - \bar{x}_c = 2.31$, $t = 1.82$, $P(t \geq +1.82) \approx .04$ Sources: [3] [36] [38].

²⁶ Possibly this is too strong a statement. Bach and Ando [5] report that they were unable to detect the debtor-creditor effect. There seem to be two reasons for the apparent difference between the outcome of Kessel's early work and the results reported here on the one hand, and the results reported by Bach and Ando on the other. Bach and Ando used several different criteria for determining whether or not the debtor-creditor effect existed. Only one of these criteria was implied by the hypothesis being tested. On that one pertinent criterion their results do verify the debtor-creditor wealth transfer. But they relied on the rule of the majority rather than the rule of a decisive test. This error was compounded by their erroneous use of a "two-tailed" probability calculation instead of a one-tailed calculation. For additional evidence, published subsequent to the Bach and Ando paper, see [2, p. 537].

large wage-to-equity ratios, debtor status would counteract the effect of the wage lag upon stock prices, and the consequences of inflation-induced lags of wages behind prices would go undetected.

In order to determine whether or not debtor-creditor effects were masking the effects of inflation upon business profits, the relationships among (1) changes in equity values, (2) annual wage-to-equity ratios, (3) debtor-creditor status, and (4) annual sales-to-equity ratio (for those who think that sales are correlated with wage-to-equity ratios) were explored by means of a multiple correlation analysis. As a measure of a firm's net monetary creditor or debtor status over the interval 1940 to 1952, the average of debtor-creditor status in each year was

TABLE 6—MATRIX OF SIMPLE AND RANK CORRELATION COEFFICIENTS AMONG EQUITY RISE, WAGE-TO-EQUITY RATIO, NET MONETARY STATUS, AND SALES-TO-EQUITY RATIO*

	(1)	(2)	(3)	(4)
Equity Rise, 1952/1939 (1)	1.	.04 (— .09)	.01 (.24)	.10 (.02)
Ratio of Wages-to-Equity (2)		1.	.33 (.15)	.51 (.83)
Net Monetary Status (3)			1.	.10 (.36)
Ratio of Sales-to-Equity (4)				1.
Partial Correlation Coefficients	$r_{12.34} = -.09 (-.11)$			
	$r_{13.24} = .04 (.36)$			
	$r_{14.23} = .16 (.08)$			

* The rank correlation coefficients are in parentheses. For the ranks, the one-tailed 5 per cent probability value is .16, the two-tailed probability value is .22, $P(r > .36) < .001$.

weighted by the price rise for the year as measured by the change in the consumer price index of the Bureau of Labor Statistics.²⁷ For each of 113 firms there are observations with respect to four variables. The simple correlation coefficients among these four variables are presented in Table 6 along with the partial correlation coefficients of each predictive variable with the other two predictive variables held statistically constant. Results of this partial correlation analysis do not support the wage lag.²⁸ However, these correlation coefficients are difficult to interpret because the necessary conditions for computing their sampling distribution are not satisfied. In particular, the predicted or dependent variable is not normally distributed.²⁹ Therefore, no reliable probability tests of significance can be applied.

²⁷ Subsequent examination indicates that an unweighted average, which is cheaper to compute, would have given essentially similar, but not quite as effective, results.

²⁸ Since these are the same data used in the previous test, these results cannot be construed as new independent evidence against the wage-lag hypothesis.

²⁹ One objection to this procedure that does not seem warranted is the objection that correlations among ratios, such as these are, must be invalid because they are subject to biases. But it is the ratios themselves that are interesting in an economic sense. Secondly,

To obtain a probability test, the values associated with each of the variables were converted to ranks, and rank correlation coefficients were computed. These are reported in Table 6. These calculations indicate a positive partial correlation between net monetary status and increases in equity values. And there is only one chance in 1,000 that such a result could be obtained by randomly sampling from a population characterized by an absence of this relationship. The negative partial correlation of wage-to-equity ratios and changes in stock prices still persists; however, this correlation can be easily rationalized as the result of random sampling from a population characterized by an absence of this relationship. Again the wage-lag hypothesis is not supported after the potential masking effects of two variables are specifically eliminated. The absence of a relationship between sales and changes in equity values is probably the result of using the level of sales rather than the rate of change of sales as an independent variable.

If neither a regression phenomenon nor a masking effect from monetary status is operating, can the results obtained be attributed to a correlation among specific industries? Relative price changes have possibly favored industries consisting of low wage-to-equity firms. Eight of the 34 firms in the low wage-to-equity class are oil firms. The removal of these firms from the sample failed to alter significantly the results obtained. The average equity rise, with the oil firms removed, of the low wage-to-equity firms was still greater than for the other class by a 6.76 to 6.19 margin. Needless to say, there exists an indefinitely large number of variables that might be so correlated with wage-to-equity ratios that the effects of the wage lag upon changes in equity values would be concealed. All any investigator can do is to eliminate only the most promising candidates in the light of his knowledge of the economics of the problem.

IV. *Conclusions*

✓ One of the important advances in economic analysis in the postwar period has been the formal incorporation into theory of the effects of wealth upon consumption expenditures. Previously it seemed reasonable to argue that wages must lag behind prices during inflation if the government acquired resources through inflation. The logic of this argument has been shown to be false.

Another independent line of argument for the proposition that inflation causes real wages to fall is based on sluggishness or flaws in the labor market whereby wage-earners receive less than their marginal

even if one thinks in absolute terms, the weighting of observations by the inverse of their standard deviation eliminates the bias. Moreover, the bias of ratios, if present, would work in favor of the wage-lag hypothesis, not against it.

product when prices are rising. But much of the data which investigators have collected to show a fall in real wages during the course of selected inflations simply fail to support the hypothesis. By one selection of beginning and terminal points for an inflation it can be shown real wages fell; by another selection it can be shown that real wages rose. The fall in real wages reported by these observers is a product of the arbitrary way the time period during which inflation occurred was defined.

However, data do exist, particularly in Mitchell's work, that unambiguously indicate a fall in real wages. But before such data can be seriously considered as supporting the wage-lag hypothesis, one must first show that even after price theory has done all it can to explain the altered price-wage relationship, there is still something left to explain. The advocates or investigators of the wage-lag hypothesis have never shown this. As for the time period studied by Mitchell, it appears that known and measurable real forces can and do explain the fall in real wages that he has observed.

Efforts to detect the existence of the wage lag during inflation through the examination of stock prices of firms that differed with respect to the volume of labor hired per dollar of invested capital by owners have also failed. This evidence contradicts the wage-lag hypothesis. Still, it is easy to make too much of this evidence since it was based on a nonrandom sample and was obtained for only one inflation.

In general, it appears that unwarranted validity has been assigned to the wage-lag hypothesis, given the character of the evidence that has been used to support it. A rereading of this evidence suggests that the wage-lag hypothesis ought to be regarded as essentially untested.

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THE PURE THEORY OF INTERNATIONAL TRADE

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The English classical model of foreign trade is the source of many propositions which form the body of international trade theory today. Despite attacks on other branches of classical theory it still survives as a basic tool of analysis. Its survival can be attributed to its applicability to leading policy issues in the country in which it originated, and to the power of its methodology: it was logically immune to the criticisms of general equilibrium and macroeconomic analysis.

The classical economists were content to establish the direction in which the terms of trade move as a result of such disturbances as disbanding, tariff adjustments, devaluation, income transfers and productivity changes. Nowadays more refined methods make it possible to derive more implications from the model, implicit in their analysis, and to ascertain the quantitative extent of the change in the terms of trade. The purpose of this paper is to derive and summarize these results.

Specifically, I shall construct an international trade model owing its origin to the classical school, and apply it to determine the exact effects on international equilibrium of unilateral transfers, productivity changes, export and import taxes, and production and consumption taxes. Many of the conclusions are already known, but it is believed that the methods employed will help to simplify the techniques used in this branch of international trade theory, and that the results established will provide a convenient survey of the subject. The first part of the analysis will be concerned with the implications of the two-country two-commodity model usually employed by the classical economists. In the final section an attempt is made to determine the validity of the results when there are many countries.

I. *The Free Trade Model*

Assume that there are two countries, A and B, in full employment, producing two commodities— X , which is exported by A, and Y , which is exported by B. Let capital letters denote production, small letters consumption, and subscripts, countries. Let T represent the capital

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exports (lending) of country A expressed in terms of X ; let P denote the terms of trade, the price of Y in terms of X ; and let D represent domestic expenditure.

The system can then be described by the following equations:

$$(1) \quad D_a \equiv x_a + Py_a = X_a + PY_a - T.$$

Domestic expenditure in A (in terms of X) equals national income minus net capital exports.

$$(2) \quad D_b \equiv x_b/P + y_b = X_b/P + Y_b + T/P.$$

Domestic expenditure in B (in terms of Y) equals national income plus net capital imports.

$$(3) \quad y_a = y_a(D_a, P).$$

The demand for Y in A depends on domestic expenditure and the terms of trade.

$$(4) \quad x_b = x_b(D_b, 1/P).$$

The demand for X in B depends on domestic expenditure and the terms of trade.

$$(5), (6) \quad X_a = X_a(1/P); \quad Y_a = Y_a(P).$$

The production of X and Y in A depends on the terms of trade.

$$(7), (8) \quad X_b = X_b(1/P); \quad Y_b = Y_b(P).$$

The production of X and Y in B depends on the terms of trade.

$$(9) \quad T = x_b - X_b - P(y_a - Y_a).$$

The net capital exports of country A equal the balance of trade of country A.

Variations in domestic expenditure in each country are assumed to depend on changes in policy. In the free-trade case the system is completed by the following equations:

$$D_a = D_a(T); \text{ and } D_b = D_b(T/P).$$

We then have eleven independent equations in the twelve unknowns: x_a , x_b , y_a , y_b , X_a , X_b , Y_a , Y_b , D_a , D_b , P and T , so there is one degree of freedom. Knowing the rate at which A is lending to B (i. e., T), we can solve for the equilibrium terms of trade (P); or, assuming that the terms of trade are fixed, we can find the rate of lending which will establish equilibrium.¹

There are other, equivalent ways of expressing the same system.

¹ The system can be represented in one diagram by the Edgeworth-Bowley box diagram if production is fixed, and by the technique introduced by Meade [13] if production is variable.

The diagrams in the text provide an alternative proof of some of the propositions; but if the reader prefers to do so he can follow the argument without reference to the diagrams.

Equations (1) and (2) could be replaced by conditions stating that world production and world consumption of each good must be equal—these alternatives imply each other when combined with equation (9) expressing balance-of-payments equilibrium. Equations (3) and (4), the demand functions for the good which is imported in each country, could be replaced by the demand functions for the good which is exported since all income is spent—the part of domestic expenditure which is not spent on one good must be spent on the other good.

It will be convenient to define an import demand function for each country. The demand for imports is the difference between the quantities of the imported good demanded and supplied, i.e., $I_a = y_a - Y_a$, and $I_b = x_b - X_b$, where I_a and I_b are, respectively, the demands for imports in A and B. Then since the demand and supply functions depend only on domestic expenditure and the terms of trade, the import demand functions must also depend on these variables. Thus we have two more equations and two more unknowns:

$$(10) \quad I_a = I_a(D_a, P) \quad \text{and} \quad (11) \quad I_b = I_b(D_b, 1/P).$$

If we now substitute (10) and (11) in the balance-of-payments equation (9) we obtain

$$(12) \quad T = I_b(D_b, 1/P) - P I_a(D_a, P).$$

The task of the following analysis is to introduce into these equations various policy parameters, and to show how the equilibrium values of the variables are affected by changes in these policies. It is first convenient, however, to outline the procedure by which the effect of these changes may most simply be obtained.

II. Procedure

The purpose of comparative-statics analysis is to compare two positions of equilibrium distinguished from each other by a shift in some parameter. In this paper we call the parameter shift a "policy change." A policy change disturbs the initial equilibrium by causing an excess demand for one of the commodities which must be eliminated by an adjustment in some other variable at the new equilibrium. The adjusting or "equilibrating" variable may be another policy change, a reversal of the original policy change, or a process of adjustment which is sufficiently traditional—hence predictable—to be called "automatic."

In classical theory the adjustment mechanism was automatic. A policy change disturbed balance-of-payments equilibrium, induced a gold flow and, through changes in relative price levels, a change in the terms of trade. Today this mechanism is not so automatic, i.e., central bank and government reaction to disequilibrium in the balance of payments is less predictable. Besides the traditional inflation-deflation

method of the gold standard a disequilibrium may be corrected by borrowing (in the short run), trade controls, tax changes, technological change (in the long run), or exchange rate adjustment. Most of these methods have been used by one country or another since the breakdown of the gold standard system to resolve balance-of-payments crises.

Because of this change in institutional response to disequilibrium any analysis of policy changes must be taxonomic. Questions like: "Do tariffs improve the balance of trade?" cannot be given an unequivocal reply—the answer depends on the other policies followed by the government. A tariff disturbs the initial equilibrium and therefore requires, for a new equilibrium to be reached, a change in some other policy; it may involve changes in any or all of the policies listed above.

But exploring all conceivable policy alternatives would be tedious and unrewarding; limits have to be imposed somewhere. For that reason I shall tentatively assume that the classical mechanism is operative, that the terms of trade "automatically" adjust to correct disequilibrium.² The first part of the comparative-statics analysis will therefore determine the effect of policy changes on the terms of trade. It will be shown later how the results can be manipulated to demonstrate the working of other mechanisms of adjustment.

The simplest way to derive the effect of a policy change on the terms of trade is to differentiate the balance-of-payments equation (12) with respect to the change in policy, and to substitute in the result the conditions necessary to satisfy the other conditions of equilibrium. A more intuitive way of getting the criterion, however, is to employ a device implicit in all comparative-statics analysis. This is to compute the excess demand caused by the policy change on the assumption that the adjusting variable (the terms of trade) is constant; and to equate this excess demand to the excess supply created by the actual change in the terms of trade. If, for example, we wish to find the criterion for the effects of a tax on the terms of trade, we first determine the excess demand caused by the tax at constant terms of trade and translate the coefficient of the tax change into the appropriate income or price elasticity; we then compute the excess supply of the same good created by a change in the terms of trade, translating its coefficient into the relevant elasticities. By equating the excess demand and the excess supply the criterion is established.³

² The terms of trade may be assumed to change by means of exchange rate or price level adjustments. If e is the price of a unit of B's currency in terms of A's currency, and P_a and P_b are the factor cost prices of exports in A and B, the terms of trade are $P = e.P_b/P_a$ in the free trade case. From a position of initial equilibrium an increase in e (devaluation by country A) would ultimately cause an offsetting change in P_b/P_a leaving the terms of trade and the balance of payments unaltered.

³ This procedure, which may be called the "method of comparative statics," can be illus-

One further point must be investigated. The adjustment mechanism implies a type of dynamic behavior and thus a condition of dynamic stability. If the system is unstable it would not tend to approach the new equilibrium given by the comparative-statics analysis, and there would be little point in pursuing the comparative-statics analysis. On the other hand if the system is stable, a useful clue may be obtained from the stability conditions about the sign of the coefficient of the adjusting variable, the terms of trade.⁴ The first step, then, is to examine the conditions of dynamic stability.

trated by two familiar examples drawn from economic theory. In the Marshallian demand-supply system, with mnemonic terminology, we have at equilibrium the condition that supply equals demand:

$$(1) \quad D(p; \alpha) = S(p)$$

where α is a parameter representing the position of the demand schedule. To determine the effect on price of a shift in the demand schedule first determine the excess demand caused by the shift at constant price; this equals

$$(2) \quad \frac{\partial D}{\partial \alpha} d\alpha.$$

The excess supply caused by an increase in price is equal to

$$(3) \quad \left(\frac{\partial S}{\partial p} - \frac{\partial D}{\partial p} \right) dp.$$

At the new equilibrium (2) and (3) must be equal so the criterion is

$$(4) \quad \frac{dp}{d\alpha} = \frac{\frac{\partial D}{\partial \alpha}}{\frac{\partial S}{\partial p} - \frac{\partial D}{\partial p}}$$

which is the criterion we set out to find.

In a simple Keynesian system we have, at equilibrium, equality of saving (S) and investment (I), i.e.,

$$(5) \quad I(\alpha) = S(y)$$

where y is income and α is a parameter representing autonomous investment. To find the effects of a shift in the investment schedule on income, first consider the excess demand for goods at constant income, i.e.,

$$(6) \quad \frac{\partial I}{\partial \alpha} d\alpha$$

and then the excess supply induced by a change in income, i.e.,

$$(7) \quad \frac{\partial S}{\partial y} dy = s' dy$$

where s' is the marginal propensity to save. At the new equilibrium the excess demand caused by the shift in investment and the excess supply created by the change in income must be equal. Equating (6) and (7) and rearranging terms we then get the familiar multiplier:

$$(8) \quad \frac{dy}{d\alpha} = \frac{\frac{\partial I}{\partial \alpha}}{s'}.$$

⁴ By the correspondence principle [22, Ch. 9]. Thus in the first example cited in the pre-

III. *Stability Conditions*

An equilibrium is stable if a small displacement is followed by a return to equilibrium. I assume that the above system is stable if a displacement of the terms of trade from equilibrium sets in motion forces inducing a return to that equilibrium. Now a disequilibrium in the classical system induces a gold flow and a deterioration of the terms of trade of the deficit country: the system is therefore stable only if a fall in the terms of trade of the deficit country causes an improvement in its balance of payments. To find the stability conditions we need to compute the excess supply caused by a change in the terms of trade. This excess supply will simultaneously establish the coefficient of a change in the terms of trade for use in the comparative-statics analysis.

With no lending, and expenditure in each country constant, the balance of payments (B) of country A in terms of home goods can be written as follows:

$$(13) \quad B = I_b (1/P) - P I_a (P).$$

In equilibrium this must be zero. Now choose commodity units so that P is initially equal to unity; then at equilibrium the volume of B's imports equals the volume of A's imports so that we can write $I_b = I = I_a$ initially. Differentiating (13) we get

$$\frac{dB}{dP} = - \frac{P dI_b}{d(1/P)} - \frac{P dI_a}{d(1/P)} - I_a = I \left(- \frac{P}{I_b} \frac{dI_b}{d(1/P)} - \frac{P}{I_a} \frac{dI_a}{dP} - 1 \right).$$

The first two terms in the bracket are, respectively, the elasticities of demand for imports in A and B; write these terms as η_a and η_b . For stability a fall in A's terms of trade must improve A's balance of payments so that the system is stable or unstable depending on whether:

$$(14) \quad \frac{dB}{dP} = I(\eta_a + \eta_b - 1) \geq 0.$$

In words, the system is stable depending on whether the sum of the elasticities of demand for imports is greater or less than unity.⁵ This is,

ceding footnote, on the dynamic hypothesis that excess demand induces an increase in price, stability requires that:

$$\frac{\partial S}{\partial p} - \frac{\partial D}{\partial p} > 0.$$

Therefore the change in demand has the same sign as the resulting change in price.

In the second example, on the dynamic hypothesis that income rises when there is excess demand for goods, stability requires that:

$$s' > 0$$

so an increase in investment induces an increase in income.

⁵ The dynamic behavior of the system may be approximated by the following differential equation:

$$(1) \quad \frac{dP}{dt} = k[P I_a(P) - I_b(1/P)]$$

of course, the familiar Marshallian condition.⁶ (See Figure 1.)

The stability condition can be expressed in terms of one good only. To see this, recall equation (1) which expresses the equality of income (plus borrowing) and expenditure in country A. With lending zero this equation can be written:

$$X_a - x_a = P(y_a - Y_a) = P I_a.$$

i.e., offers of exports equal the value of imports demanded. Substituting in (13) and making a similar substitution for I_b , we can write the balance of payments (with no lending) as follows:

$$B = (x_b - X_b) - (X_a - x_a).$$

Differentiating and rearranging terms we get:

$$\frac{dB}{dP} = \left(\frac{d(x_a + x_b)}{dP} \frac{p}{(x_a + x_b)} - \frac{d(X_a + X_b)}{dP} \frac{P}{(X_a + X_b)} \right) \frac{X}{P}$$

where X is world production and consumption at equilibrium, and the arguments in the bracket are, respectively, the world elasticity of demand for X and the world elasticity of supply of X :

$$\left(\eta_x = - \frac{d(x_a + x_b)}{d(1/P)} \cdot \frac{(1/P)}{(x_a + x_b)} \right)$$

$$\left(\epsilon_x = \frac{d(X_a + X_b)}{d(1/P)} \cdot \frac{(1/P)}{(X_a + X_b)} \right).$$

which states that the speed of the change in the terms of trade is proportional to the discrepancy between foreign exchange payments and receipts. Expanding (1) in a Taylor series, omitting nonlinear terms, and choosing time units to make $k=1$, we obtain

$$(2) \quad \frac{dP}{dt} = I(1 - \eta_a - \eta_b)(P - P^0)$$

where P^0 is the terms of trade at equilibrium. Equation (2) has a solution

$$(3) \quad P = P^0 + A e^{-I(\eta_a + \eta_b - 1)t}.$$

The equilibrium point is stable only if P eventually approaches P^0 ; this can only be the case if the other term in (3) disappears, i.e., if $\eta_a + \eta_b - 1 > 0$.

⁶ Marshall's dynamic postulates differ from those described in the text and in the preceding footnote. The latter assumes that the budget equations in each country are instantaneously satisfied (each country is always at a point on its offer curve) but that markets are not necessarily cleared. Marshall's postulates are based on adjustments of offers toward the budget equations (offer curves) [11]. This difference corresponds roughly to the distinction made between instantaneous and lagged adjustments analyzed in Arrow and Hurwicz [1]. Marshall's adjustment process, which is rationalized by the varying profitability of export industries, admits the possibility of complex roots and therefore an oscillatory path to equilibrium. See Samuelson [22, pp. 266-68].

This discussion refers to the stability of an *equilibrium* rather than to the stability of a *system*. John Stuart Mill recognized the possibility of multiple equilibria without reference to stability [17, pp. 154-63], but his treatment was faulty. Marshall, in 1879, was aware [11, pp. 24-25] that a point of unstable equilibrium must be flanked by points of stable equilibria, that the number of equilibria must be odd, and that (therefore) if an equilibrium were unique it would be stable.

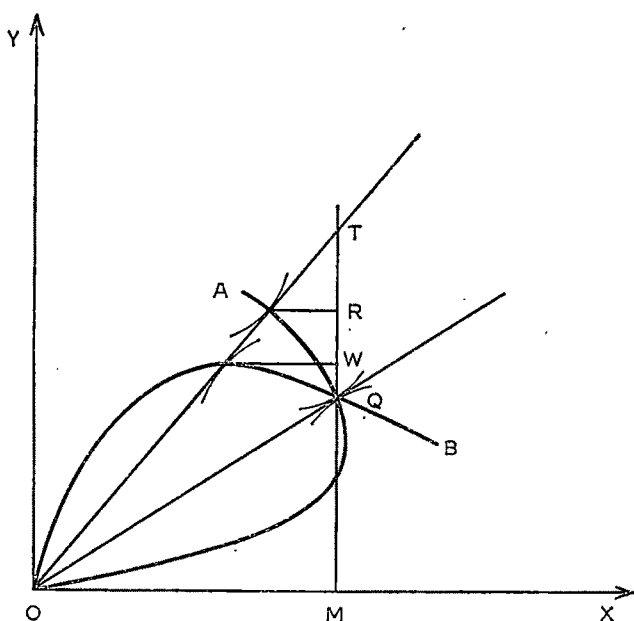


FIGURE 1. STABILITY CONDITIONS

The offer curves OA and OB intersect at initial equilibrium Q . A change in the terms of trade in the proportion TQ/QM in favor of A creates an excess demand for B 's good and a deficit in A 's balance of payments equal to RW (in terms of Y). The situation is therefore stable: Country A loses and country B gains gold, causing deflation in A and inflation in B until the gap RW is closed at the original equilibrium Q .

The stability conditions are derived as follows: Define the elasticities of demand for imports in each country,

$$\eta_a = \frac{RQ}{QM} \bigg/ \frac{TQ}{QM} \quad \text{and} \quad \eta_b = \frac{TW}{QM} \bigg/ \frac{TQ}{QM}$$

and the balance of payments deficit in A in terms of foreign goods,

$$\frac{dB}{P} = RW.$$

Then:

$$\begin{aligned} \frac{dB}{P} &= RW = QM \cdot \frac{TQ}{QM} \cdot \frac{RW}{TQ} = QM \cdot \frac{TQ}{QM} \cdot \frac{RQ + TW - TQ}{TQ} \\ &= QM \cdot \frac{TQ}{QM} \left[\frac{RQ}{QM} \cdot \frac{QM}{TQ} + \frac{TW}{QM} \cdot \frac{QM}{TQ} - 1 \right] \\ &= (\text{by definition}) I \frac{dP}{P} (\eta_a + \eta_b - 1). \end{aligned}$$

Hence:

$$(15) \quad \frac{dB}{dP} = X(\eta_x + \epsilon_x),$$

where units are chosen so that P is, at equilibrium, equal to unity. By

similar reasoning

$$\left[\text{from the balance of payments equation, } \frac{B}{P} = (Y_b - y_b) - (y_a - Y_a) \right]$$

we find that:

$$(15') \quad \frac{dB}{dP} = Y(\eta_y + \epsilon_y),$$

where η_y and ϵ_y are the elasticities of world demand for, and supply of, Y .⁷

The elasticities of demand for X and Y are defined to be positive provided that these goods are not Giffen goods; and the elasticities of supply of X and Y are defined to be positive provided that opportunity costs are not decreasing. It may then be seen that the system is necessarily stable if neither good is a Giffen good and opportunity costs are not decreasing. But even if the goods are Giffen goods, positive supply elasticities may yet make the system stable.

In the remainder of this paper I assume that the stability condition is satisfied, an assumption which, as the foregoing remarks suggest, does not appear very restrictive.⁸

IV. *Unilateral Payments*

The first policy change we shall consider is a unilateral payment from one country to the other. This involves two parts: a *financial* transfer and a *real* transfer. The financial transfer refers to the accumulation and liquidation of debt on the part of individuals or governments in each country, while the real transfer refers to the induced movement of goods. Assume that A is the transferring country.

In the case of a private flow of capital (ignoring interest payments) lenders in A buy the debt of borrowers in B, the former financing the purchase out of an excess of saving over investment, the latter disposing of the proceeds by an excess of investment over saving. Because of the identity of income-less-lending and expenditure [equations (1) and (2)] the excess of investment over saving in B, and saving over investment in A, must each equal the transfer.

In the case of intergovernmental transfers such as reparations payments or foreign aid, the government in A (the paying country) grants credits to B, the former financing the credits by means of, say, an in-

⁷ Mosak derived stability conditions in terms of one good only [18, Ch. 4]. See also Johnson [6, p. 98].

When trade is not initially in balance a slight adjustment is required in the stability condition. See Hirschman [5].

⁸ Marshall's judgment is probably too strong: "... it is not inconceivable, but it is absolutely impossible" [12, p. 354].

come tax, the latter disposing of the proceeds by means of, say, an income subsidy. Again, because of the identity of income-less-lending and expenditure, the budget surplus in A and the budget deficit in B are each equal to the transfer.

Whatever the type of transfer and however it is financed and disposed of, domestic expenditure in A is reduced, and in B is increased, by the amount of the transfer. These changes in expenditure induce changes in demand which, at constant terms of trade, create disequilibrium in the balance of payments. The transfer problem may then be posed as the problem of determining the direction and extent of the change in the terms of trade required to eliminate the balance-of-payments disequilibrium.

The Terms of Trade. To find the effects of a transfer on the terms of trade we first determine the excess demand created by the expenditure changes at constant terms of trade. This can be done in terms of either good since an excess demand for one good implies an excess supply of the other good.

The reduction in domestic expenditure in A decreases the demand for Y in A at constant terms of trade by:

$$P \frac{\partial y_a}{\partial D_a} dD_a = m_a dD_a$$

where m_a is the marginal propensity to spend on imports in A. The increase in domestic expenditure in B increases the demand for Y in B by:

$$\frac{\partial y_b}{\partial D_b} dD_b = c_b dD_b$$

where c_b is the marginal propensity to spend on home goods in B. The excess demand at constant terms of trade is therefore the sum of these changes or:

$$m_a dD_a + c_b dD_b = (c_b - m_a) dT,$$

noting that the changes in expenditure in each country are equal to the change in lending, i.e., $-dD_a = dD_b = dT$. Now expenditure in each country is divided between home goods and imports so that the sum of the marginal propensities to spend on home goods and imports is unity; thus $c_b + m_b = 1 = c_a + m_a$.⁹ We can now make use of this result to translate the above criterion into a number of equivalent forms. The

⁹ Differentiation of $D_a = x_a + P y_a$ and $D_b = x_b/P + y_b$ with respect to D_a and D_b yields:

$$1 = \frac{\partial x_a}{\partial D_a} + P \frac{\partial y_a}{\partial D_a} = c_a + m_a, \text{ and}$$

$$1 = \frac{1}{P} \frac{\partial x_b}{\partial D_b} + \frac{\partial y_b}{\partial D_b} = m_a + c_a.$$

most convenient for our purposes is the familiar one:

$$(16) \quad (1 - m_a - m_b)dT,$$

which states that transfer creates an excess demand for, or excess supply of, the good of the transferring country depending on whether the sum of the marginal propensities to spend on imports is greater or less than unity. Only in the special case where the receiving country increases its consumption of the two goods in the same proportion that the paying country does without them ($1 - m_a - m_b = 0$) will no change in the terms of trade be required. If $m_a + m_b > 1$ the receiving country experiences a deficit; if $m_a + m_b < 1$, the paying country suffers a deficit. (See Figure 2.)

To correct the disequilibrium equal to $(1 - m_a - m_b)dT$ a change in the terms of trade is required. But we already know from the stability condition (14) that a change in the terms of trade causes an excess supply of B's good (or an excess demand for A's good, or improves A's balance, or worsens B's balance) by an amount equal to:

$$(17) \quad (\eta_a + \eta_b - 1)IdP.$$

The excess demand for B's good at constant terms of trade must, at the new equilibrium, be equal to the excess supply of B's good caused by the actual change in the terms of trade. Equating of (16) and (17) therefore provides the general criterion for the change in the terms of trade:

$$(18) \quad \frac{dP}{dT} = \frac{1 - m_a - m_b}{I(\eta_a + \eta_b - 1)}.$$

It may be seen that the higher are the price elasticities of demand for imports the smaller will be the change in the terms of trade (a small change relieves a large excess demand). In the limiting case where one of the elasticities is infinite, no change in the terms of trade is required. Similarly, the closer to unity is the sum of the marginal propensities to import, the smaller is the excess demand to be eliminated by a change in the terms of trade, and so the smaller is the actual change in the terms of trade.¹⁰

¹⁰ To obtain the criterion directly, differentiate the balance of payments equation:

$$T = I_b(D_b, 1/P) - P I_a(D_a, P)$$

with respect to T . This yields:

$$1 - m_b \frac{dD_b}{dT} + m_a \frac{dD_a}{dT} = \frac{\partial I_b}{\partial (1/P)} \frac{d(1/P)}{dT} - P \frac{\partial I_a}{\partial P} \frac{dP}{dT} - I_a \frac{dP}{dT}.$$

Expenditure changes are equal in absolute value to the transfer so

$$\frac{dD_b}{dT} = - \frac{dD_a}{dT} = 1.$$

Then by forming elasticities from the terms on the right, and taking P initially equal to unity, we get (18).

For a sample of recent literature on the transfer problem see [18, Ch. 4] [15] [7] [23]; and for a survey of earlier literature see Viner [24, pp. 290-377].

Real Income. How is real income affected by a grant or gift from one country to the other? In the reparations discussions of the interwar period the view was widely held that the terms of trade of the paying country must fall, thus imposing an additional burden. The change in real income implicit within the change in the terms of trade was called the "transfer burden."

The change in real income due to transfer is composed of two effects—the direct effect of the change in expenditure, and the income effect implicit within the change in the terms of trade. Thus the real income of the receiving country improves by more or less than the transfer itself depending on whether the terms of trade improve or worsen. For small changes we can approximate the income effect of a change in the terms of trade by the change in cost of the initial volume of imports, i.e., by IdP . The change in real income as a result of transfer is therefore approximately:

$$(19) \quad \frac{dU_b}{dT} = - \frac{dU_a}{dT} = 1 + I \frac{dP}{dT}$$

where U_a and U_b are, respectively, the real incomes of A and B. Now substituting for dP/dT from (16) we obtain an approximate quantitative measure of the change in real income evaluated at pretransfer prices:

$$(20) \quad \frac{dU_b}{dT} = - \frac{dU_a}{dT} = 1 + \frac{1 - m_a - m_b}{(\eta_a + \eta_b - 1)}.$$

It will now be convenient to introduce a relationship between price elasticities and income propensities based on Slutsky's separation of price effects into income and substitution effects. A price elasticity of demand can always be written as the sum of a compensated (pure substitution) price elasticity and an income propensity. We can therefore write the price elasticity of demand for imports as the sum of the compensated elasticity of demand for imports and the marginal propensity to spend on imports. If the primes denote the compensated elasticities we have $\eta_a = \eta'_a + m_a$ and $\eta_b = \eta'_b + m_b$.¹¹ Since pure substitution effects

¹¹ Consider any demand function of the form $I = I(D, P)$ and differentiate with respect to P . This yields:

$$\frac{P}{I} \frac{dI}{dP} = P \frac{\partial I}{\partial D} \frac{dD}{IdP} + \frac{P}{I} \frac{\partial I}{\partial P}$$

after multiplying by P/I . Now $-P/I \partial I/\partial P$ is the (money-income constant) elasticity of demand, η , and $P \partial I/\partial D$ is the marginal propensity to spend, m . Thus:

$$\frac{P}{I} \frac{dI}{dP} = m \frac{dD}{IdP} - \eta.$$

A change in price can be associated with a change in real income approximately equal to the change in cost of the initial amount bought, IdP . If expenditure is adjusted to compensate for

are, in the two-good case, always positive the ordinary elasticity of demand for imports is always larger than the marginal propensity to import by an amount which depends on the size of substitution effects. (See Figure 3.)

Using this relationship we can manipulate (20) to get:

$$(21) \quad \frac{dU_b}{dT} = - \frac{dU_a}{dT} = \frac{\eta_a - m_a + \eta_b - m_b}{\eta_a + \eta_b - 1} = \frac{\eta'_a + \eta'_b}{\eta_a + \eta_b - 1}.$$

From this criterion it can be seen that the real income of the receiving country can only decline, as a result of transfer, if the system is unstable.¹² Assuming stability, the higher are m_a and m_b (the marginal propensities to import) the smaller is the increase in real income of the receiving country, since income effects enter only in the denominator of (21).¹³

V. Productivity Changes

Assume that the country experiencing the productivity increase is completely specialized and that expenditure increases by the full amount of the increase in output. We shall first determine the effects of a change in productivity on the terms of trade, and then its influence on real income.

The Terms of Trade. To determine the effect of a change in productivity on the terms of trade we first determine the excess demand created for one of the goods on the assumption that the terms of trade

this change in real income, i.e., if $dD=IdP$, then $-P/I \, dI/dP$ becomes the compensated elasticity of demand, η' , and we get

$$\eta = + \eta'.$$

If indifference curves are convex all substitution effects are positive, so $\eta' > 0$. From this it follows that the elasticity of demand for imports is always greater than the marginal propensity to import. *A fortiori* the sum of the elasticities of demand for imports is greater than the sum of the marginal propensities to import so that if $m_a + m_b$ is equal to, or exceeds, unity the exchange market is necessarily stable. Alternatively, an unstable exchange market implies that the sum of the marginal propensities to import is less than unity. J. E. Meade has made extensive use [14] of this relation; his stability condition is split into income and substitution effects and, in our notation, is $\eta'_a + \eta'_b + m_a + m_b - 1$.

¹² Leontief produced an example [9] consistent with convex indifference curves, where the change in the terms of trade in favor of the paying country is so great that the real income of the latter improves as a result of the transfer. Equation (21) proves that this cannot happen unless the system is unstable. The identification of this "Leontief Effect" with instability is due to Samuelson [22, p. 29].

¹³ Transfer analysis has many applications in economic theory. It applies to any redistribution of income between sectors, individuals or groups within a country. In the Keynesian problem of income redistribution a gift or tax-cum-subsidy from the rich to the poor increases or decreases effective demand depending on whether the marginal propensity to spend (MPS) of the rich is less or greater than that of the poor. In public finance theory an increase in government spending financed by new taxes stimulates effective demand if the MPS of the government is greater than that of the public. And in monetary theory a fall in the price level stimulates effective demand if the MPS of creditors is greater than that of debtors (including governments and central banks).

of trade. The criterion is therefore:

$$(22) \quad \frac{dP}{dX^*} = \frac{m_a}{I(\eta_a + \eta_b - 1)}.$$

This result is obvious: an increase in the world output of X must lower the relative world price of X . The only exception (apart from constant costs and incomplete specialization, or perfect substitution in consumption) is the case where the country which has grown spends all of its increased income on its own good ($m_a = 1 - c_a = 0$).¹⁴

Equation (22) can be translated into proportional changes by multiplying both sides by X (output) and dividing by P , with the following result:

$$(23) \quad \frac{dP/P}{dX^*/X} = \frac{m_a X / P I_a}{\eta_a + \eta_b - 1} = \frac{\sigma_a}{\eta_a + \eta_b - 1},$$

where σ_a is the marginal propensity to spend on imports divided by the average propensity to spend on imports—i.e., the income elasticity of demand for imports. Alternatively we can express the criterion in terms of annual rates of change as follows:

$$(24) \quad \frac{\dot{P}}{\dot{R}} = \frac{\sigma_a}{\eta_a + \eta_b - 1}$$

where \dot{P} is the percentage deterioration of A's terms of trade per year and \dot{R} is the annual rate of growth.

By following a similar procedure for country B we can find the annual percentage change in the terms of trade when both countries are growing:

$$(25) \quad P = \frac{\sigma_a R_a - \sigma_b R_b}{\eta_a + \eta_b - 1}$$

where σ_b and \dot{R}_b are, respectively, the income elasticity of demand for imports in B and the annual rate of growth of B.

Real Income. A more interesting question is whether or not real income will increase or decrease as a result of a change in productivity. An increase in productivity affects real income in two ways which work in opposite directions. On the one hand real income increases by the full amount of the change in output at constant terms of trade; but, against this may be set the negative income effect of the actual deterioration in the terms of trade. We may very simply derive a measure of the change

¹⁴ To derive this criterion directly, differentiate $T = I_b(D_b, 1/P) - P I_a(D_a, P)$ with respect to X^* (output in A). With lending (T) constant and zero, this yields

$$\left(-\frac{\partial I_b}{\partial(1/P)} - \frac{\partial I_a}{\partial P} - 1 \right) \frac{dP}{dX^*} = m_b \frac{dD_b}{dX^*} - m_a \frac{dD_a}{dX^*}.$$

Expenditure in B is constant (in terms of B's good) so $dD_b/dX^* = 0$; but expenditure in A increases *pari passu* with output, i.e., $dD_a/dX^* = 1$. Then, forming elasticities from the terms of the left, we get equation (22) above.

in real income by adding the effects of the change in real income at constant terms of trade, and the reduction in income due to the change in the terms of trade. The change in real income is $dU_a^* = dX_a - IdP$. Dividing by dX_a^* and applying equation (22) we obtain the following criterion:

$$(26) \quad \frac{dU_a}{dX_a^*} = 1 - I \frac{dP}{dX_a^*} = 1 - \frac{m_a}{\eta_a + \eta_b - 1}.$$

Real income will be increased by growth provided that $\eta_a + \eta_b > 1 + m_a$, i.e., if the sum of the elasticities of demand for imports is greater than unity plus the marginal propensity to import. Note that violation of this condition is consistent with stability. (See Figure 4.)

It will clarify the meaning of (26) if we make use of the relationship used in the previous section between "ordinary" and "compensated" elasticities of demand for imports—in particular, $\eta_a = \eta'_a + m_a$. Manipulating (26) we get

$$(27) \quad \frac{dU_a}{dX_a} = \frac{\eta_a - m_a + \eta_b - 1}{\eta_a + \eta_b - 1} = \frac{\eta'_a + \eta_b - 1}{\eta_a + \eta_b - 1}.$$

Thus real income will increase in the growing country if $\eta'_a > 1 - \eta_b$, i.e., if the compensated elasticity of demand for imports in the growing country is greater than unity minus the ordinary elasticity of demand for imports in the other country. This criterion may be simplified somewhat by substituting in it the well-known relation between the elasticity of demand for imports and the elasticity of supply of exports (ϵ): $\eta_b - 1 = \epsilon_b$. The criterion is therefore $\eta'_a + \epsilon_b \geq 0$.¹⁵

¹⁵ This relationship can be derived from the income = expenditure condition. From equation (2), with no lending, we have:

$$x_b/P + y_b = X_b/P + Y_b$$

$$\text{or} \quad Y_b - y_b = \frac{1}{P} (x_b - X_b)$$

$$\text{or} \quad E_b = \frac{1}{P} I_b$$

where E_b represents B's offers of exports. Differentiating, and taking P as initially unity, we have:

$$\frac{P}{E_b} \frac{dE_b}{dP} = \frac{P}{I_b} \frac{dI_b}{dP} - 1,$$

after dividing by $E_b = I_b/P$.

Thus $\epsilon_b = \eta_b - 1$, recalling that

$$\frac{P}{I_b} \frac{dI_b}{dP} = - \frac{1/P}{I_b} \frac{dI_b}{d(1/P)} = \eta_b.$$

Note that when the elasticity of demand is unity the elasticity of supply of exports is zero; this means that the same amount of exports is spent on imports regardless of the terms of trade.

A geometric proof of this relation can easily be got from Figure 3 or from Marshall's analysis [11, pp. 337-38].

An alternative, direct method of getting this criterion is to apply the "method of comparative statics." To find whether real income in the growing country increases or decreases first determine the excess demand for imports on the tentative supposition that real income in A is constant. If real income is constant the terms of trade move against A so there is a pure substitution effect, $-\eta'_a IdP$, which measures the increase in demand for imports in A. On the other hand the increase in supply of imports forthcoming from B due to the change in the terms of trade is $\epsilon_b IdP$. The excess demand at constant real income is therefore $-(\eta'_a + \epsilon_b) IdP$. The deterioration in A's terms of trade which leaves A's real income unchanged is therefore too great or too little to relieve the excess demand due to the productivity change depending on whether $\eta'_a + \epsilon_b \geq 0$. The criterion for the change in real income is thus established since, depending on whether the terms-of-trade change has been too great or too little, real income increases or decreases.

It appears then that a country may be worse off with, than without, the improvement in productivity. Growth may be "damnifying."¹⁶ Too rapid growth of the export industries of one country, and the resultant attempt to push exports onto world markets, results in such a large decline in the terms of trade that the negative income effect of the change in relative prices is greater than the positive income effect of the increase in domestic output at constant prices. The case of the group of primary-producing countries readily presents itself. And, if the model were applied to different sectors within a single economy, it might be found that U.S. agriculture is another example.

Nevertheless, this possibility does not present a valid argument against growth. In the first place the conditions under which increasing productivity can affect real income perversely are quite strict: the foreign elasticity of demand must be less than unity and, perhaps, appreciably so if home substitution effects are high. Second (and more important), since world income as a whole increases, by compensation both countries (or sectors) could be made better off than before. Finally, the damnified country can always impose taxes on trade sufficient to reap at least some of the benefits of the productivity increase. Obviously the perverse effect is not possible if the growing country is following an optimum tariff policy since that implies an elasticity of demand in the foreign country greater than unity.

¹⁶ Mill was aware [17, pp. 150-53] that an increase in productivity would lower the commodity terms of trade and even the factorial terms of trade if foreign demand, in the latter case, were inelastic. Edgeworth interpreted [3, p. 10] Mill's passage as indicating that a country could be "damnified" by growth, supplying the necessary assumption to make Mill's analysis correct. The first derivations of criteria (23) and (26) are due to Meade [14, e.g., p. 153] and to Johnson [6] [8].

VI. *Taxes and Subsidies on Trade*

A tax or subsidy on trade introduces a divergence between foreign and domestic price ratios. Equal taxes on exports or imports create the same divergence between foreign and domestic price ratios (if trade is balanced) so that the *real* effects of import and export taxes are symmetrical. A tax on imports at constant terms of trade raises the relative price of imports in the taxing country and therefore *draws* resources away from export industries into import-competing industries. A tax on exports at constant terms of trade lowers the relative price of exports in the taxing country and thus *pushes* resources into import-competing industries. With balanced trade the revenues collected by the two taxes are the same. We may therefore speak of trade restriction or trade promotion without specifying whether the tax or subsidy is on exports or imports.¹⁷

There are two analytic methods of disposing of the tariff proceeds. We may assume that the government spends the tariff proceeds on the two goods in a given proportion; or we may suppose that tax proceeds are redistributed as income subsidies to consumers. The latter method, which is used here, is simpler because it avoids the necessity of introducing a government demand equation, and does not give rise to asymmetries when dealing with trade subsidies. In the following analysis it should be remembered that we are in fact examining the effects of tariffs combined with this method of disposing of the proceeds.

The Terms of Trade. To determine the effect of a tariff on the terms of trade first compute the excess demand for imports at constant terms of trade. At constant terms of trade the relative price of imports in the tariff-imposing country (A) rises by the full amount of the tariff. Then, with t_a representing unity plus the ad valorem rate of tariff, the change in demand for imports before redistribution of the proceeds is:

$$\frac{\partial I_a}{(P t_a)} \frac{d(P t_a)}{d t_a} d t_a = \frac{\partial I_a}{\partial (P t_a)} \frac{P t_a}{I_a} I_a d t_a = -\eta_a I_a d t_a$$

assuming initial free trade ($t_a=1$). To this change in demand must be added the increase in demand for imports occasioned by the redistribution of the tariff proceeds, i.e., $m_a I_a d t_a$. Adding the terms we get the

¹⁷ Marshall writes [12, pp. 180-81]: "... The considerations which can be urged for and against the levying of an import tax on a particular commodity differ widely from those appropriate to a particular export tax: and this is perhaps the origin of an opinion, which seems to pervade a good deal of economic discussion, that a general tax on all imports would have widely different effects from a general tax on all exports. In fact the two taxes would have the same effect: provided they were evenly distributed, equal in aggregate amount, and their proceeds were expended in the same way." He then shows how this can be proved. Bastable, Edgeworth and others were also aware of the symmetry. For a modern treatment see Lerner [10].

excess demand for imports due to the tariff at constant terms of trade, i.e., $(-\eta_a + m_a)Idt_a = -\eta'_a Idt_a$. This excess demand must be eliminated, at the new equilibrium, by a change in the terms of trade. We then have the following criterion:

$$(28) \quad \frac{dP}{dt_a} = \frac{-\eta'_a}{\eta_a + \eta_b - 1}.$$

Tariffs normally improve the terms of trade.¹⁸

The degree to which the terms of trade change following a tariff depends on the elasticities. The more elastic is the foreign offer curve the smaller will be the improvement in the terms of trade due to a tariff, and in the limiting case where the foreign offer curve is perfectly elastic, the terms of trade remain unchanged (the only exception). On the other hand if the foreign offer curve is elastic¹⁹ the greater is the compensated elasticity of demand for imports at home the more effective will a given tariff be in improving the terms of trade; in the limiting case where η'_a is infinite the terms of trade will improve in the same proportion as the (ad valorem) tariff. Now both the above propositions are related to the classical notion about the division of the gains from trade between large and small countries. Because small countries tend to be more completely specialized than large countries, it was generally believed that the offer curve of a small country was less elastic than that of a large country. This means that the gain per unit of trade going to a small country was likely to be larger than that going to a large country. Then since the small country already reaped a large proportion of the gain from trade, opportunities for increasing that gain further through tariffs were small. On the other hand large countries, gaining little from trade, could exact a larger gain by imposing tariffs and forcing small countries to trade at a less favorable price ratio.

The Domestic Price Ratio. A common motive for trade restriction is the protection of import-competing industries. In order that these industries be protected the domestic relative price of imports (inclusive of

¹⁸ To derive (28) directly differentiate:

$$T = 0 = I_b(D_b, 1/P) - P I_a(D_a, P t_a)$$

with respect to t_a . With D_b constant this yields:

$$\begin{aligned} (\eta_a + \eta_b - 1)I \frac{dP}{dt_a} &= P \frac{\partial I_a}{\partial D_a} \frac{dD_a}{dt_a} - \frac{\partial I_a}{\partial (P t_a)}, \\ &= (m_a - \eta_a)I \end{aligned}$$

since $dD_a/dt_a = I$; i.e., expenditure in A rises by the value of the tariff proceeds.

The qualitative direction of change in the terms of trade was admitted by Ricardo and known to most of the later classical economists. The algebraic criterion can be got from Meade's analysis [14].

¹⁹ Note that if the foreign offer curve is inelastic the terms of trade may improve by more than the tariff, provided that the home offer curve is not perfectly elastic; if the latter is perfectly elastic the maximum change in the terms of trade is equal to the rate of the tariff.

the tariff) must rise. We shall therefore develop a criterion for the change in the domestic price ratio following a tariff.

The domestic price ratio in country A is Pt_a (where t_a is unity plus the ad valorem rate of tariff). We are interested in how Pt_a will change as a result of an increase in t_a , i.e., in the sign of

$$\frac{d(Pt_a)}{dt_a} = P + t_a \frac{dP}{dt_a},$$

where P and t_a are initially equal to unity. Substituting for dP/dt_a , from equation (26) we obtain the following criterion:

$$(29) \quad \frac{d(Pt_a)}{dt_a} = 1 - \frac{\eta'_a}{\eta_a + \eta_b - 1} = \frac{\eta_b + m_a - 1}{\eta_a + \eta_b - 1}.$$

A tariff raises the domestic price of imports if the sum of the foreign elasticity of demand for imports and the domestic marginal propensity to import is greater than unity.²⁰ The only exception is the case previously mentioned where the home offer curve is infinitely elastic, in which case the domestic price ratio remains unchanged. (See Figure 5.)

The meaning of the criterion will be clarified if we consider the excess demand for imports on the assumption that the domestic price ratio remains unchanged. In that event the terms of trade improve by the full amount of the tariff, so that the change in supply of imports from B is $-\epsilon_b Idt_a = (1 - \eta_b) Idt_a$. On the other hand the redistribution of the tariff proceeds and the resulting increase in domestic expenditure in A increases the demand for imports by $m_a Idt_a$. Subtracting the change in supply of imports from B from the increase in demand for imports in A we get the excess demand for imports at constant domestic prices, i.e., $(\eta_b + m_a - 1) Idt_a$. Now if the foreign demand is less than unit-elastic (foreign supply elasticity is negative) an improvement in A's terms of trade results in an increase in supply of imports from B; but against this must be set the increased demand for imports in A resulting from the spending of the tariff proceeds. If the former effect is greater than the latter, the relative price of imports must fall. If, for example, the foreign elasticity of supply were $-.6$ (implying an elasticity of demand equal to $.4$) while the domestic marginal propensity to import were $.5$, the tariff would cause, at constant domestic prices, an excess supply of B's good equal to $.1 Idt_a$; to eliminate this excess supply of B's good, the relative (tariff-inclusive) price of imports must fall, and the terms of trade must improve by more than the tariff.

²⁰ The classical economists, many of whom tried to determine whether a country gained more or less than the amount of the tax, generally employed the criterion $\eta_b \gtrless 1$, assuming, implicitly or explicitly, that the tax proceeds were spent on domestic goods, or that the tax was on the transit of goods that would be re-exported. For modern discussions see Lerner [10] and Metzler [16].

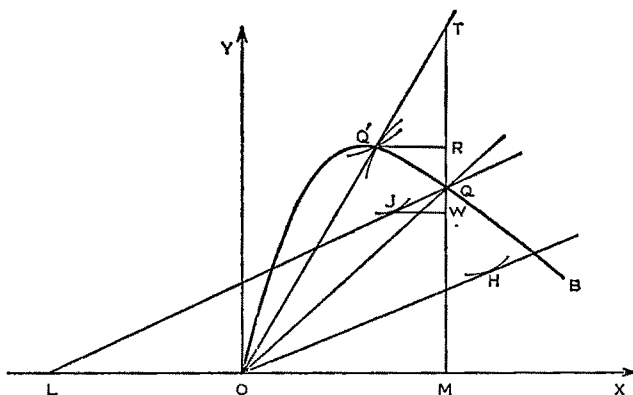


FIGURE 5. TARIFFS, THE TERMS OF TRADE AND THE DOMESTIC PRICE RATIO

The offer curves of A (not drawn) and B (*OB*) intersect at the free-trade equilibrium *Q*. Suppose that this equilibrium is disturbed by the imposition of a tariff by A's government. At constant terms of trade the price of *Y* in A rises by the full amount of the tariff *LO/OM* and trade equilibrium in A moves to a point on A's original offer curve, such as *H*. But when tariff revenues equal (approximately) to *OL* in terms of *X* are redistributed to consumers the demand for both goods increases. The equilibrium point for A at constant terms of trade therefore moves to a point on A's revenue-redistributed offer curve, such as *J*. At constant terms of trade the tariff-cum-income subsidy results in an excess supply of *Y* and a surplus in A's balance of payments equal to *QW* since the new equilibrium at *J* must be below *Q* if substitution effects are positive. Gold therefore flows into A from B and A's terms of trade improve until the excess supply *WQ* is relieved. Assume that the new equilibrium is at *Q'*.

The excess demand for imports in A at constant terms of trade is:

$$-\frac{dB}{P} = WQ = \frac{WQ}{OM} \cdot QM = \frac{WQ}{OM} \frac{OM}{OL} \cdot \frac{OL}{OM} \cdot QM = \eta'_a Idt_a.$$

On the other hand we have:

$$\frac{dB}{P} = WQ = WR - TR - TQ = \left[\frac{WR}{QM} \frac{QM}{TQ} - \frac{TR}{QM} \frac{QM}{TQ} - 1 \right] QM \frac{TQ}{QM} \\ \left(\eta_a + \eta_b + 1 \right) I \frac{dP}{P} \cdot$$

It follows that:

$$\frac{dP}{dt_n} = \frac{-\eta_a'}{\eta_a + \eta_b - 1}.$$

Note that η_a refers to the elasticity of the revenue-redistributed offer curve in A.

For the domestic price ratio in A to be unchanged at the new equilibrium, as a result of the tariff, the slope of the A-indifference curve at Q' must be the same as the slope of the indifference curves of A and B at Q. But in that case the marginal propensity to import in A is equal to RQ/TQ (for small tariffs); and since

$$\epsilon_b = \eta_b - 1 = -\frac{RQ}{OM} \frac{QM}{TO} = \frac{RQ}{TO},$$

it follows that $\eta_b - m_a - 1 = 0$. This is the borderline case. It is easily seen that the slope of A's indifference curve at Q' is flatter or steeper than that at Q as $m_a \gtrless \eta_b - 1$.

This possibility is consistent with stability. It means that a tariff may have an adverse protective effect. To protect the domestic industry imports must be subsidized instead of taxed! Under no circumstances, however, would a country ever find it beneficial in fact to subsidize imports to protect the domestic industry. For the adverse protective effect to occur the foreign demand must be inelastic, and in that case a tariff must always result in an improvement in national welfare (more imports are obtained for fewer exports). If an optimum tariff policy is being followed a further increase in the tariff always raises the relative price of imports in the tariff-imposing country and thus has a normal protective effect.

Tariff Changes in Both Countries. If both countries adjust their tariff rates the extent and direction of the disequilibrium depends on the size of the tariff changes and the elasticities of demand. In bilateral tariff negotiations it may be useful to know what adjustment in the tariffs of both countries will leave the balance of payments unaltered. If, for practical reasons, changes in the terms of trade (through exchange rate or wage changes) must be ruled out, countries embarking on, say, a customs union experiment may need to know the rate at which each can reduce tariffs so as to leave the balance of payments unchanged. The answer to this question can readily be obtained from equation (28) making appropriate adjustments for country B. If we write P as the annual deterioration of country A's terms of trade, i_a as the annual change in A's tariff and i_b as the annual change in B's tariff we can obtain the following criterion:

$$(30) \quad P = \frac{\eta'_b i_b - \eta'_a i_a}{\eta_a + \eta_b - 1}.$$

In order to prevent any change in the terms of trade the numerator must be zero; tariffs must then be changed at a rate inversely proportional to the compensated elasticities of demand for imports. [Note that equation (28) applies with a changed sign to subsidies since subsidies are simply negative tariffs.]

Income Transfers and Trade Taxes. Suppose the authorities in one country wish to make a grant to another country but, for political or other reasons, are unable to make the gift official. Alternatively, suppose that one country wishes to exact a transfer of real income from another country without imposing a formal tribute. Can this be accomplished efficiently by changes in trade taxes? (See Figure 6.)

To show that it can consider first the relation between trade taxes and subsidies. Suppose that country A subsidizes exports (or imports) while country B taxes imports (or exports). In that case the same goods are being taxed, the only difference being that the customs duties are

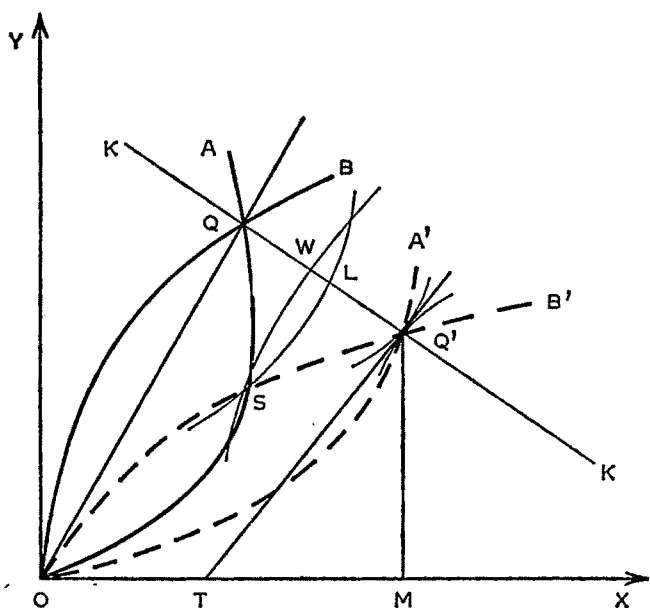


FIGURE 6. INCOME TRANSFERS AND TRADE TAXES

Initial equilibrium is at Q on the contract curve KK . Suppose that A subsidizes, and B taxes, trade at the same rate OT/TM . Then A 's and B 's offer curves bend down to OA' and OB' , respectively, intersecting at Q' (they still originate from O). Since price ratios remain the same in both countries Q' must be on the contract curve. The slope of the indifference curves at Q' are, from the transfer analysis, greater or less than the slope at Q depending on whether the sum of the marginal propensities to import is greater or less than unity. The terms of trade are now OQ' , necessarily worse for A if the system is stable. Real income in A falls to the same extent as if A had made a gift of OT of X to B .

Suppose now that only B has a tax (equal to OT/TM) so that trade equilibrium is at S . To restore efficiency (Pareto optimum), A can impose a subsidy equal to B 's tariff, attaining the equilibrium (worse for A , better for B) Q' . A more interesting possibility is for A to bribe B to remove the tariff, the value of the bribe being the transfer necessary to effect an equilibrium between W and L (better for both than S). Or if A is already receiving gifts from B then tariff reduction in B and the elimination of gifts to A (i.e., "Trade, not Aid") can make both A and B better off.

collected by officials of different nationality. Trade is subsidized in A and taxed in B . Now if the increase in trade subsidies in A is equal to the increase in trade taxes in B this combined policy is equal to a transfer of income from A to B equal to the value of the tax-subsidy payments. Since the tax in B cancels the subsidy in A , price ratios in the two countries must be the same. The change in real income, evaluated at the original price ratio, in each country is given by:

$$-dU_a = dU_b = IdP = \frac{(-\eta'_a t_a + \eta'_b t_b)I}{\eta_a + \eta_b - 1}.$$

Now if $-dt_a = dt_b = dt$ we have:

$$(31) \quad \frac{-dU_a}{Idt} = \frac{-dU_b}{Idt} = \frac{\eta'_a + \eta'_b}{\eta_a + \eta_b - 1}.$$

But (31) is the same as the criterion for the change in real income after transfer [equation (19) above] if Idt , the value of the tax-subsidy receipts, is substituted for the transfer.

VII. *Consumption and Production Taxes*

Taxes on commodities, as distinct from taxes on trade, make it necessary to distinguish between consumers' and producers' price ratios. A consumption tax or subsidy creates a divergence between the price ratio facing consumers in the taxing country and all other price ratios, while a production tax causes a discrepancy between the price ratio facing producers and all other price ratios.

There are eight taxes and subsidies in each country which are possible, but it will not be necessary to consider more than two. A subsidy is a negative tax so that we need only consider taxes. And a tax on one good is equivalent to a subsidy on the other good because of our assumptions about the disposal of tax proceeds and the financing of subsidy payments. Because of these assumptions an equal tax on the two goods has no effect on equilibrium; this follows because each tax is combined with an income subsidy, and an income subsidy has the same effect as an equal subsidy (or an equal reduction in taxes) on the two goods. But if an equal tax on the two goods does not affect equilibrium, then neither does a tax on one of the goods combined with the elimination of a subsidy of equal amount on the other good—hence it follows that a tax on one of the goods is equivalent to a subsidy on the other. Thus we need only consider one consumption tax and one production tax.

Consumption Taxes and the Terms of Trade. To find the effects of a consumption tax on the terms of trade first determine the excess demand caused by the tax at constant terms of trade. Let us suppose that a tax is imposed by country A on the imported good, Y . Then at constant terms of trade the price of Y to consumers in A rises by the amount of the tax. Before the redistribution of the tax proceeds the change in demand for y in A, at constant terms of trade, is

$$\frac{\partial y_a}{\partial (Pt_{cya})} \frac{Pt_{cya}}{y_a} y_a dt_{cya} = -\eta_{ya} y_a dt_{cya}$$

where t_{cya} is unity plus the rate at which commodity Y is taxed in A

and η_{ya} is the elasticity of demand for y in A.²¹ (P and t_{cya} are both *initially* taken to be unity.) Now the tax proceeds amount to $y_a dt_{cya}$ so that the increase in demand for y due to their disposition is $m_a y_a dt_{cya}$. Adding the two effects we get the excess demand at constant terms of trade,

$$(m_a - \eta_{ya}) y_a dt_{cya} = - \eta'_{ya} y_a dt_{cya},$$

where η'_{ya} is the compensated elasticity of demand for y in A, i.e., the elasticity of demand for Y after consumers have been compensated for the change in real income implicit within the price change. We now have the following criterion:

$$(32) \quad \frac{dP}{dt_{cya}} = - \frac{y_a}{I} \frac{\eta'_{ya}}{\eta_a + \eta_b - 1}.$$

The compensated elasticity term is positive (it represents the elasticity of a consumption indifference curve) so that a consumption tax on the imported good generally improves the terms of trade of the taxing country. This conclusion is to be expected since a tax on the imported good diverts demand away from that good, thereby causing an excess world supply.

There are some special cases we may consider: (1) in the unusual case where η'_{ya} is zero—implying no substitution in consumption (a kinked consumption indifference curve at that point)—the terms of trade do not change; (2) if the foreign offer curve is perfectly elastic any excess demand can be eliminated by shifts in production or consumption at constant cost in the foreign country so there results no change in the terms of trade; (3) if the domestic offer curve is perfectly elastic (η_a is infinite) it is now necessary to know whether this is due to perfect substitution in production (incomplete specialization at constant cost) or perfect substitution in consumption; if the former is the case the denominator of (32) is infinite, so the terms of trade do not change; but if

²¹ The elasticity of demand for the imported good is never larger than the elasticity of demand for imports. The exact relation can be derived from the definition of the demand for imports. From $I_a = y_a - Y_a$ we get, by differentiation,

$$\frac{\partial I_a}{\partial P} = \frac{\partial y_a}{\partial P} - \frac{\partial Y_a}{\partial P}$$

and

$$- \frac{P}{I_a} \frac{\partial I_a}{\partial P} = - \frac{\partial y_a}{\partial P} \frac{P}{y_a} \frac{y_a}{I_a} + \frac{\partial Y_a}{\partial P} \frac{P}{Y_a} \frac{Y_a}{I_a}$$

hence

$$\eta_a = \eta_{ya} \frac{y_a}{I} + \epsilon_{ya} \frac{Y_a}{I}$$

where ϵ_{ya} is the elasticity of supply of Y in A. (A similar result, holds for country B.) The elasticities η_{ya} and η_a coincide only when there is no home production of the imported good.

the goods are perfect substitutes in consumption both the denominator and the numerator are infinite so that the terms of trade change by the full amount of the tax; (4) if there is no domestic production of the imported good the tax has the same effect as a tariff ($y_a = I$ and $\eta'_{ya} = \eta'_a$).

This analysis applies also to a subsidy on the consumption of the good which is exported and, with a change of sign, to a subsidy on the imported good or tax on the exported good.

Consumption Taxes and the Ratio of Market Prices. Does a consumption tax necessarily raise the market price of the taxed good relative to that of the untaxed good? By analogy to the effect of a tariff on the domestic price ratio we should not expect this to be so. The market (relative) price of importables is Pt_{cya} , which we assume to be initially unity. The change in this price ratio due to the tariff is:

$$(33) \quad \frac{d(Pt_{cya})}{dt_{cya}} = 1 - \frac{dP}{dt_{cya}} = 1 - \frac{y_a}{I} \frac{\eta'_{ya}}{\eta_a + \eta_b - 1}$$

$$= \frac{\eta_b + m_a + \epsilon'_{ya} \frac{Y_a}{I} - 1}{\eta_a + \eta_b - 1}$$

where ϵ'_{ya} is the compensated elasticity of supply of Y in A , and represents the elasticity of the production transformation curves.²² Only by

²² The compensated elasticity of supply requires some explanation. From the two relations:

$$\eta_a = \eta'_a + m_a, \quad \text{and} \quad \eta_a = \eta_{ya} \frac{y_a}{I} + \epsilon_{ya} \frac{Y_a}{I}$$

we can obtain:

$$\eta_a = \eta_a - m_a = (\eta_{ya} - m_a) \frac{y_a}{I} + (\epsilon_{ya} + m_a) \frac{Y_a}{I}.$$

It can now be shown that $\eta_{ya} - m_a = \eta'_{ya}$, the compensated elasticity of demand for Y in A ; and that $\epsilon_{ya} + m_a = \epsilon'_{ya}$, the compensated elasticity of supply of Y in A . We have $dI_a = dy_a - dY_a$ from the definition of the demand for imports. Now dI_a contains an income effect equal to $-m_a I_a dP$ where $-I_a dP$ measures the change in real income of country A looked at as a whole. The term dy_a contains an income effect equal to $-m_a y_a dP$, where $-y_a dP$ measures the change in real income of people of country A looked at in their role as consumers alone. Finally the term dY_a contains an income effect equal to $m_a Y_a dP$ where $Y_a dP$ measures the change in real income of producers of Y in country A . We now have the following relations:

$$dI_a = (dI_a)' - m_a I_a dP; \quad dy_a = (dy_a)' - m_a y_a dP; \quad \text{and} \quad dY_a = (dY_a)' + m_a Y_a dP$$

where the primes denote pure substitution effects. Substituting in $dI_a = dy_a - dY_a$ we get

$$(dI_a)' - m_a I_a dP = (dy_a)' - m_a y_a dP - [(dY_a)' + m_a Y_a dP].$$

The income effects on the two sides cancel and the proof of the relation between compensated elasticities follows readily. Dividing by $I_a dP$, multiplying by P , and changing signs we obtain

$$\eta'_a = \eta'_{ya} \frac{y_a}{I} + \epsilon'_{ya} \frac{Y_a}{I}$$

where the primes denote that the elasticities contain no income effects.

this term does the last criterion in (33) differ from the criterion for a change in the domestic price ratio after the imposition of a tariff [see equation (29)]. If a tariff will raise the domestic relative price of imports so will a consumption tax on the import good. The converse is not true. Even if the foreign offer curve is inelastic and the domestic marginal propensity to import is low, high substitution effects in production will be sufficient to ensure a rise in the tax-inclusive price of importables.

Production Taxes and the Terms of Trade. To find the effects of a production tax on the terms of trade first consider the excess demand caused by the production tax at constant terms of trade. If a tax on the production of the import good is imposed and the proceeds of the tax are redistributed to producers there remains only a pure substitution effect, a movement along the production-possibility curve. At constant terms of trade (which is also the price ratio facing domestic consumers) the excess demand for imports, after the redistribution of the proceeds, is equal to:

$$\epsilon'_{ya} Y_a dt_{pya}$$

where t_{pya} is equal to unity plus the tax, and ϵ'_{ya} is the elasticity of the transformation curve. This excess demand must be eliminated by a worsening of A's terms of trade. The criterion is therefore:

$$(34) \quad \frac{dP}{dt_{pya}} = \frac{Y_a}{I} \frac{\epsilon'_{ya}}{(\eta_a + \eta_b - 1)}.$$

Since with increasing opportunity costs ϵ'_{ya} is always positive the terms of trade of the taxing country fall. Again this applies to a production subsidy on the exported good and, with a change of sign, to a production subsidy on the imported good and a production tax on the exported good. This conforms to common sense. A tax on the production of any good decreases the production of that good and increases the production of the other good causing a rise in the relative world price of the taxed good.

Production Taxes and Relative Prices at Factor Cost. The price ratio

These elasticities have a simple interpretation: η'_a is the elasticity of a trade-indifference curve; η'_{ya} is the elasticity of a consumption-indifference curve; and ϵ'_{ya} is the elasticity of a production-indifference (production-possibility) curve. The identification is formally valid for small changes only.

Criterion (32) can be derived directly by differentiating:

$$T = 0 = I_b(D_b, 1/P) - P[y_a(D_a, Pt_{cya}) - Y_a(P)]$$

noting that D_b is constant and that

$$\frac{dD_a}{dt_{cya}} = y_a.$$

facing producers is P/t_{pya} , which is initially taken to be unity. This will change as a result of a tax depending on the sign of:

$$(35) \quad \frac{d(P/t_{pya})}{dt_{pya}} = \frac{dP}{dt_{pya}} - 1 = \frac{Y_a}{I} \frac{\epsilon'_{ya}}{\eta_a + \eta_b - 1} \\ = - \frac{\eta_b + m_a + \eta'_{ya} \frac{Y_a}{I} - 1}{\eta_a + \eta_b - 1}.$$

The analogy to equations (29) and (33) above readily presents itself.²³

Relation between Commodity Taxes and Trade Taxes. A tariff, at constant terms of trade, raises the price of imports to both consumers and producers in the taxing country by the full amount of the tariff. Any other system of taxes which does the same thing will have the same effect on the terms of trade as a tariff. Thus taxes on trade can be duplicated by taxes on commodities. We can establish these relations either by showing that the tax combination affects the price ratios facing consumers and producers in the same way as a trade tax, or by adding the effects of the criteria obtained above in each case. By either method it is readily shown that a tariff (on Y) or an export tax (on X) is equivalent in real terms to: (a) a consumption tax on Y plus a production subsidy on Y ; (b) a consumption tax on Y plus a production tax on X ; (c) a consumption subsidy on X plus a production subsidy on Y ; (d) a consumption subsidy on X plus a production tax on X . And because trade subsidies are negative trade taxes, an export subsidy (on X) or an import subsidy (on Y) can be duplicated by: (e) a consumption subsidy on Y plus a production tax on Y ; (f) a consumption subsidy on Y plus a production subsidy on X ; (g) a consumption tax on X plus a production tax on Y ; (h) a consumption tax on X plus a production subsidy on X . From these relations it follows that: (1) the effects of devaluation can be duplicated or frustrated by changes in commodity taxes and subsidies (since devaluation is equivalent to an import tariff plus an export subsidy); (2) the optimum tariff can be duplicated by commodity taxes; and (3) income transfers can be duplicated by changes in

²³ Criterion (34) can be derived directly by differentiating:

$$T = 0 = I_b(D_b, 1/P) - P \left[y_a(D_a, P) - Y_a \left(\frac{P}{t_{pya}} \right) \right]$$

noting that

$$\frac{dD_a}{dt_{pya}} = Y_a.$$

To my knowledge the effects of consumption and production taxes on the terms of trade have not been formally treated in the literature before, although the general direction of their influence is indicated in many classical writings. See, for example, Viner [24, p. 363].

commodity taxes in both countries. A further application is to customs unions; an agreement over tariff reduction has little force if it is not combined with agreement over the domestic tax structures.

Consideration of the commodity-tax structure is necessary before evaluating the desirability of tariff reductions. If there are commodity taxes and subsidies in each country none of the well-known welfare propositions of international trade theory holds. In particular from a "free-trade" (i.e., no *trade* taxes) position, it can be shown that, if there are commodity taxes and subsidies: (1) both countries simultaneously may be better off without, than with, trade; (2) a country may gain by a deterioration in its terms of trade even if the initiating cause occurs in the foreign country; (3) a small tariff may worsen the welfare of the tariff-imposing country even if the foreign offer curve is not infinitely elastic; and (4) the imposition of a tariff may simultaneously improve the welfare of both countries. These propositions follow because commodity taxes overextend or underextend trade.

VIII. *Other Mechanisms of Adjustment*

Thus far we have dealt with the effect of policy changes on the terms of trade, the latter adjusting through price-level or exchange-rate variations. It was argued earlier that authorities may adopt other policies which prevent, or render unnecessary, changes in the terms of trade. This would be the case if authorities pegged the exchange rate and stabilized domestic price levels, relying on, say, trade controls to correct disequilibria. The purpose of this section is to show how the results already obtained can be applied to other mechanisms of adjustment. The procedure to be followed is the same as before: First state the postulate on which dynamic behavior is based, deduce the condition of dynamic stability, and then examine the excess demands caused by the policy changes.

Fiscal Policy and Capital Movements. Suppose that authorities peg the exchange rate and stabilize the domestic price level in each country. The price level may be stabilized in a variety of ways but the simplest for our purpose is to suppose that authorities inflate domestic expenditure by means of a budget deficit when there is deflationary pressure (excess supply of its export good) and deflate domestic expenditure by means of a budget surplus when there is inflationary pressure (excess demand for its export good). We may assume that the deficits and surpluses are financed and disposed of by drawing on or accumulating credits with an international agency—say, the International Monetary Fund. Now since an excess supply of one country's good implies an excess demand for the other country's good it follows that one country will be borrowing at the time another country is lending; and because of

the identity of income (including loans) and expenditure, the rate of lending by one country is equal to the rate of borrowing in the other country, and both are equal (with appropriate signs) to the rate at which the budgets are out of balance.

Whether or not a system based on these rules is stable depends on the effectiveness of the deflation-inflation policy in relieving excess demand for the deflating country's good and excess supply of the inflating country's good. But it is easily seen that this is equivalent to whether a transfer from one country to another will cause an excess supply of the transferring country's good. The system is therefore stable or unstable depending on whether the sum of the marginal propensities to import is less than or greater than unity. The term $1 - m_a - m_b$ also gives the denominator of the criterion showing the direction and amount of lending required to eliminate a given excess demand. (See Figure 7.)

To determine the effects of policy changes on lending in a system obeying the above rules we (as before) find the excess demand due to the policy change with no lending. For example, the excess demand for imports due to a tariff in country A is $-\eta'_a I dt_a$. The change in the trade balance and lending of country A is therefore:

$$(36) \quad \frac{dT}{dt_a} = \frac{\eta'_a I}{1 - m_a - m_b};$$

if the system is stable the tariff improves the trade balance.²⁴ Or we may consider the change in lending and the trade balance due to an increase in productivity in, say, country B:

$$(37) \quad \frac{dT}{dY_b^*} = \frac{m_b}{1 - m_a - m_b};$$

assuming stability, country A must lend to country B to maintain equilibrium in the balance of payments.

In a similar fashion we can find the effects on lending of all the policies discussed in previous sections. It may be helpful to consider two cases. Suppose that country A devalues its currency. Applying the same method we find that the criterion for the change in the balance of trade and lending is:

$$(38) \quad \frac{dT}{dP} = \frac{(\eta_a + \eta_b - 1)I}{1 - m_a - m_b}.$$

It should be noticed that (38) is the reciprocal of (18), the criterion for the change in the terms of trade after transfer. The interpretation is

²⁴ This criterion has been used by Meade [14, p. 155] and derived geometrically by Ozga [21] although in neither case is a distinction made between stable and unstable situations. Ozga's analysis, which was presented to a seminar in London in 1956, greatly improved my own geometrical representations.

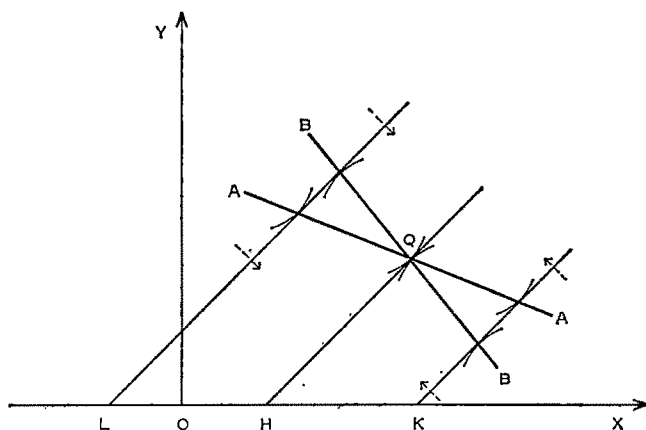


FIGURE 7. STABILITY OF THE "IMF SYSTEM"

Let equilibrium be initially at Q with the government of A lending or giving to the government of B the annual payment OH . It is assumed that exchange rates are fixed and that each government, by means of fiscal policy, stabilizes export price levels.

Now suppose that the equilibrium is disturbed by, say, a private flow of capital of HL from B to A, and that this induces an excess of saving over investment in B, and an excess of investment over saving in A, equal to the transfer. Now if the sum of the marginal propensities to import is less than unity, as in the diagram, the Engel curve of A (AA') must be flatter than the Engel curve of B (BB'); the capital flow therefore induces an excess demand for A's good and an excess supply of B's good, and a surplus in A's and a deficit in B's balance of payments.

To correct the disequilibrium, A's government deflates expenditure by means of a budget surplus and turns the proceeds over to the IMF; and B's government inflates expenditure by means of a budget deficit borrowing from the IMF. This process continues until the inflationary pressure in A and the deflationary pressure in B are eliminated, i.e., until the equilibrium Q and the net lending position OH are restored. By similar analysis it can be shown that a movement of capital from A to B in excess of OH (say to OK) will cause deflationary pressure in A and inflationary pressure in B, necessitating government action in each country to eliminate the disequilibrium. In either case the equilibrium Q is stable.

But if the sum of the marginal propensities to import exceeds unity the system is unstable. This may be seen by considering again a movement of capital from B to A of HL . This time the capital movement causes an excess supply of A's good and an excess demand for B's good. A's government therefore inflates expenditure and B's government deflates expenditure, moving the system ever further from equilibrium.

As a practical problem it would be necessary to distinguish between the Engel curves appropriate for different types of transfer.

fundamentally different. In (18) the stability condition is that the sum of the elasticities is greater than unity, while in (38) the stability condition is that the sum of the marginal propensities to import is less than unity. In (18) lending induces—because of the “rules of the gold-standard- (or flexible-exchange-rate-) game”—a change in the terms of trade; in (38) devaluation induces—because of the “rules of the IMF game”—a change in the balance of trade and lending. An interesting result is the following: If the IMF system is unstable the gold standard (or flexible exchange) system is stable; and if the gold standard (or

flexible exchange) system is unstable the IMF system is stable. *Instability* of one system therefore implies *stability* of the other system, though not vice versa. This relation holds because the sum of the marginal propensities to import is less than the sum of the elasticities of demand for imports.

Finally, consider a trivial case. A change in capital exports has no ultimate effect on net lending! In the IMF system there is only one equilibrium rate of lending (in the absence of other trade policy changes), just as, in the classical system, there is only one equilibrium value of the terms of trade (assuming that the equilibrium is unique). This trivial case is cited for purposes of comparison with the classical contention that devaluation, from a position of equilibrium, does not change the terms of trade or the balance of trade; instead, it initiates price level changes which restore the equilibrium terms of trade. A displacement of the variable of adjustment from equilibrium initiates dynamic forces which induce a return to equilibrium.

Similar analysis can be applied to systems of adjustment based on tariff, tax or productivity changes.

IX. Summary

The results of the preceding analysis may all be summarized by introducing all policy parameters into the balance of payments equation and differentiating. We obtain:

$$(39) \quad (1 - m_a - m_b)dT - I(\eta_a + \eta_b - 1)dP - I\eta'_a dt_a + I\eta'_b dt_b - \gamma_a\eta'_{ya} dt_{ca} + x_b\eta'_{xb} dt_{cb} + Y_a\epsilon'_{ya} dt_{pa} - X_b\epsilon'_{xb} dt_{pb} + m_a dX_a^* - m_b dX_b^* = 0$$

where the same terminology is used as before except that *effective* tax rates are used. (Thus dt_a and dt_b refer to the effective rate at which trade taxes are changed in A and B; changes in trade subsidies are subtracted from changes in trade taxes. Similarly dt_{ca} and dt_{cb} represent the effective rate at which consumption taxes or subsidies are changed in A and B; a tax on the consumption of import goods combined with an equal tax on the consumption of home goods would leave the effective rate unchanged. Similarly for production taxes dt_{pa} and dt_{pb} .)

The policy equation (39) shows the relation between policy changes which are necessary to maintain equilibrium in the system; it can be used to show the policy changes which are necessary to offset the disequilibrium caused by other policies. Suppose, for example, country A wishes to know the rate at which it must tax import goods in order to relieve a disequilibrium caused by an increase in productivity in the foreign country. To find the answer set all policy changes except dt_{ca} and dX_b^* equal to zero. This leaves the equation

$$-\gamma_a\eta'_{ya}dt_{ca} - m_b dX_b^* = 0$$

and the answer

$$\frac{dt_{ca}}{dX_b^*} = - \frac{m_b}{\gamma_a \eta_{ya}'} .$$

The productivity change in B causes a surplus in A's balance which can be relieved by a reduction in the rate at which consumption of import goods (export goods) are taxed (subsidized) in A. Any other relation between two or more policy changes can in this way be determined.²⁵

X. Extension of the Model: The Multiple Country Case

The propositions now established have been derived from a model consisting of only two goods and two countries. The traditional use of this model in international trade literature is based on a belief that it suggests "theorems which may be seen to admit of extension to more concrete cases" [3, p. 31]. In this final section general results are derived and it is shown that in at least one important case the propositions previously established for two countries hold for an arbitrary number of countries.

Suppose that there are $n+1$ countries, and let all prices and balances of payments be expressed in terms of the currency of country 0 (e.g., dollars). If prices (of export goods or currencies) are flexible then the balance of each country depends on all prices. The conditions of equilibrium can be written as follows:

$$(40) \quad \begin{array}{c} B_1(P_1, \dots, P_n; \alpha) = 0 \\ \cdot \cdot \cdot \cdot \cdot \cdot \cdot \cdot \cdot \cdot \\ B_n(P_1, \dots, P_n; \alpha) = 0 \end{array}$$

where α is a parameter representing a particular policy.²⁶ Note that only

²⁵ All of the criteria contained in equation (39) are capable of simple geometric proofs when the variables are taken two at a time.

The above analysis of policy changes has been expounded on the assumption that transport costs are absent; for a geometrical representation of transport costs employing Marshallian offer curves see Mundell [19].

²⁶ Let x_{rs} and X_{rs} be, respectively, consumption and production of the r th good in the s th country, and let T_s be the capital exports of country s . Suppose that there are n countries and $m+1$ goods. Then a general model can be expressed by the following equations:

$$(1) \quad Y_s - D_s \equiv \sum_{r=1}^m P_r(X_{rs} - x_{rs}) = T_s \quad (s = 1, 2, \dots, n)$$

The national "budget" equations: $\text{Income} - \text{Spending} = \text{Lending}$, for each country. If these equations are satisfied each country is on its m -dimensional offer curve.

$$(2) \quad \sum_{r=1}^n (X_{rs} - x_{rs}) = 0 \quad (r = 1, 2, \dots, m)$$

The market clearing equations: World Supply=World Demand, for every good. Notice that the excess supply function of the numeraire, commodity 0 (e.g., gold), is omitted; if equations (1) are satisfied, then the last equation in (2) can be deduced from the others (or vice versa).

where

$$\Delta \equiv \begin{vmatrix} b_{11} & \cdots & b_{1n} \\ \cdot & \cdot & \cdot \\ b_{n1} & \cdots & b_{nn} \end{vmatrix} \quad \text{and where } \Delta_{ji} \text{ is the cofactor of the } j\text{th row and the } i\text{th column of } \Delta.$$

Equations (42) provide a general framework into which specific policy changes can be introduced. But in order to evaluate any of the signs, $dP_i/d\alpha$, it is necessary to know the values, or at least the signs, of two kinds of terms: the coefficients $\partial B_j/\partial\alpha$ and the ratios Δ_{ji}/Δ . Now the coefficients $\partial B_j/\partial\alpha$ describe the change in the balances of payments arising from the policy change *at constant prices*: to evaluate these coefficients, then, we can apply the method of comparative statics. The ratios Δ_{ji}/Δ , on the other hand, indicate the interactions of price changes in multiple markets, and the effectiveness of price changes in relieving the initial disequilibrium. In the two-country case these signs were determined by the stability conditions; in the multiple-country case, however, it is easily shown that some of the ratios may be positive while others are negative without conflicting with the conditions of stability. It appears, then, that in evaluating equations (42) we will be left with some positive and some negative terms, and no general presumption about the sign of $dP_i/d\alpha$.

To make progress some restriction on the signs of the elements b_{ij} in the basic determinant Δ is required. The most interesting special case, for present purposes, is that where $b_{ij} > 0$ for $i \neq j$. This assumption means that an increase in the price of the exports of any country, other prices being held constant, improves the balance of payments of every other country; it also implies, by Cournot's Law, that a rise in the price level in one country worsens that country's balance of payments. From this assumption flow two important deductions: (1) the system is stable under the usual dynamic postulates²⁸ [1][4][20];

²⁸ There are two approaches to the stability of international equilibrium. One approach is to treat the world economy like the domestic economy and postulate that the price of any good rises and falls in proportion to the excess demand and supply of that good. For example, the dynamic system may be written as follows:

$$(1) \quad \frac{dP_r}{dt} = k_r \sum_{s=1}^n (x_{rs} - X_{rs})$$

where the summation is over countries. By linearizing (1) (retaining only linear terms of a Taylor series), and translating the resulting partial derivatives into demand and supply elasticities, we obtain:

$$(2) \quad \frac{dP_r}{dt} = k_r X_r \sum_{q=1}^m (\eta_{rq} - \epsilon_{rq})(P_q - P_q^0)$$

where the *own* elasticities of demand (η_{rr}) and supply (ϵ_{rr}) in the world as a whole are defined to be, normally, negative and positive respectively. The linear system can be stable only if the real parts of the roots of the following equation:

$$(3) \quad |k_r X_r (\eta_{rq} - \epsilon_{rq}) - \delta_{rq} \lambda| = 0$$

and (2) every ratio Δ_{ji}/Δ is negative [18, pp. 49-51]. With this information we can evaluate the direction of change in the terms of trade resulting from many policy changes.

Productivity Changes. Suppose that output and expenditure in country 0, the numeraire country, increase by dX_0^* . At constant prices, assuming no inferior goods, inhabitants of country 0 buy more of all goods, creating a deficit in their own balance and a surplus in the balance of every other country. The surplus in the balance of country i is $m_{i0}dX_0^*$ where m_{i0} is the marginal propensity to spend in country 0 on the goods of the i th country. The typical term in equation (42) is therefore

$$-\frac{\partial B_j}{\partial \alpha} = -m_{j0}.$$

are all negative (δ_{rr} is the Kronecker delta). The theorem on gross substitutes states that if all cross elasticities are positive (including the cross elasticities of demand for the numeraire good) the system is stable.

The above system focuses attention on world markets for particular commodities. Classical international trade theorists on the other hand (with the exception of F. D. Graham) emphasized the importance of disequilibrium in the balance of payments, gold flows and changes in the terms of trade. If we now let P_i denote the world price of the exports of country i we have the following system which is more compatible with the postulates of classical theory:

$$(4) \quad \frac{dP_i}{dt} = h_i B_i(P_1, \dots, P_n) \quad (i = 1, \dots, n)$$

assuming that there are now $n+1$ countries, and that prices are expressed in terms of the exports of country 0. Following the same procedure as above we find that stability requires that the real parts of the roots of the following equation:

$$(5) \quad |h_i b_{ij} - \delta_{ij} \lambda| = 0$$

must all be negative. Again, stability is assured if all $b_{ij} > 0$ for $i \neq j$. If prices were all constant and exchange rates were all flexible the system would be stable if all currencies were gross substitutes.

Generally the systems (1) and (4) are fundamentally different. Goods may all be gross substitutes while some currencies are gross complements, and vice versa. The gap between the two systems narrows, however, when only one country produces each good: In that case an excess world demand for a good implies an excess demand for the currency of the country producing that good.

I have said that the system (4) conforms more closely to the classical system than does system (1). This is not meant to imply that classical theorists would accept even as an approximation the rigid dynamic laws postulated. Consider, for example, the following passage from Marshall's privately circulated manuscript of 1879 [11, pp. 19, 25]:

... so that if we chose to assign to these horizontal and vertical forces any particular laws, we should obtain a differential equation for the motion of the exchange index ... Such calculations might afford considerable scope to the ingenuity of those who devise mathematical problems, but ... they would afford no aid to the economist.

For the mathematical functions introduced into the original differential equations could not, in the present condition of our knowledge, be chosen so as to represent even approximately the economic forces which actually operate in the world. ... Whereas the use of mathematical analysis has been found to tempt men to expend their energy on the elaboration of minute and complex hypotheses, which have indeed some distant analogy to economic conditions, but which cannot properly be said to represent in any way economic laws.

By substitution, then, we get the criterion for the change in the terms of trade of the growing country

$$(43) \quad \frac{dP_i}{dX_0^*} = - \sum_{j=1}^n m_{j0} \frac{\Delta_{ji}}{\Delta}.$$

Every m_{j0} is positive in the absence of inferior goods, and every Δ_{ji}/Δ is negative if all exports are gross substitutes. Therefore *an improvement in productivity unambiguously worsens the commodity terms of trade of the growing country*: the prices of the exports of every other country rise relative to the price of the exports of the growing country.²⁹

Tariff Changes. Suppose that country 0 applies an undiscriminatory tariff equal to dt_0 on all imports. At constant foreign prices the tariff-inclusive price of all imports in country 0 rises by dt_0 . On the assumption that the government spends the tariff proceeds on home goods, the excess demand for the goods of typical country j , at constant foreign

²⁹ Why has country 0, whose exports are numeraire, been chosen as the growing country? Suppose instead that country i grows by dX_i^* . Then substituting in equation (42) the terms

$$-\frac{\partial B_i}{\partial X_i^*} = m_i$$

(the aggregate marginal propensity to import in country i), and the terms

$$-\frac{\partial B_j}{\partial X_i} = -m_{ji},$$

when $i \neq j$, we obtain the criterion:

$$(1) \quad \frac{dP_i}{dX_i^*} = -m_{ii} \frac{\Delta_{ii}}{\Delta} - \dots + m_i \frac{\Delta_{ii}}{\Delta} - \dots - m_{ni} \frac{\Delta_{ni}}{\Delta}.$$

But $m_i \Delta_{ii}/\Delta$ is negative while all the other terms are positive. It would therefore appear that no unambiguous result is possible, and that the method of treating the change as occurring in the numeraire country is a special case.

Nevertheless the economist's intuition tells him that in static analysis the choice of numeraire cannot affect the ultimate change in *relative* prices; and that if the terms of trade of the numeraire country deteriorate when that country grows, the terms of trade of any other country must fall when it grows. He may therefore conclude that P_i must fall in equation (1), that the negative term dominates.

This is in fact correct. Making use of the definition of the aggregate marginal propensity to import, i.e., $m_i = m_{0i} + m_{1i} + \dots + m_{ni}$, we can rearrange (1) to get:

$$(2) \quad \frac{dP_i}{dX_i^*} = m_{0i} \frac{\Delta_{ii}}{\Delta} + m_{1i} \frac{\Delta_{ii} - \Delta_{1i}}{\Delta} + \dots + m_{ni} \frac{\Delta_{ii} - \Delta_{ni}}{\Delta}.$$

Now the first term on the right of (2) is clearly negative. The other terms will be negative if the *principal* cofactor of Δ dominates each of the other cofactors, i.e., if $|\Delta_{ii}| > |\Delta_{ji}|$ ($j \neq i$). But by subtraction of the two cofactors it is easily shown that the resulting $(n-1)$ th order determinant $\Delta_{ii} - \Delta_{ji}$ has all the characteristics of Δ (positive off-diagonal elements and dominant negative diagonal elements) except that its sign is opposite to the sign of Δ . (An analogous theorem has been proved by Metzler in analysis of the matrix multiplier.) All the terms on the right of (2) are therefore negative, so P_i must fall as a result of growth in country i .

To avoid these complications I have, in the text, supposed that the policy change occurs in the country whose exports are numeraire, though the result is perfectly general. The same applies to the analysis of tax and tariff changes analyzed below.

prices, is $-\eta_{j0}I_{j0}dt_0$, where η_{j0} and I_{j0} are, respectively, the elasticity of demand for imports (with respect to own price) and the level of imports, from country j to country 0. The typical term from the general equation (42) becomes

$$\frac{-\partial B_j}{\partial t_0} = \eta_{j0}I_{j0}.$$

The criterion for the change in the terms of trade of country 0 is therefore:

$$(44) \quad \frac{dP_i}{dt_0} = \sum_{j=1}^n \eta_{j0}I_{j0} \frac{\Delta_{ji}}{\Delta}.$$

The elasticities are all defined to be positive (Giffen goods are ruled out by the assumption of gross substitution) so the conclusion is again unambiguous: *An increase in tariffs raises the price of the exports of the tariff-imposing country relative to the prices of the exports of all other countries*, on the assumption that tariff proceeds are spent on home goods.³⁰

Consumption and Production Tax Changes. The effects of a consumption tax on import goods are equivalent to the effects of a tariff if there is no domestic production of these goods. If there is domestic production of import goods then the typical term, from the skeleton equation (42), is $\eta_{cjo}y_{j0}$, where η_{cjo} is the elasticity of consumer demand in 0 for the products exported by country j , and y_{j0} is the level of domestic consumption of these products. This assumes again that tax proceeds are spent on domestic goods. The criterion for the change in the relative price of the exports of the typical country i is therefore:

³⁰ If the tariff proceeds are redistributed to consumers an income effect must be added to each term in (44). The tariff proceeds are equal to $I_0 d\ln$, where I_0 is the initial aggregate level of imports, so the typical income term is $-m_{j0}I_0$. The criterion then becomes:

$$\frac{dP_i}{dt_0} = \sum_{j=1}^n [\eta_{j0}I_{j0} - m_{j0}I_0] \frac{\Delta_{ji}}{\Delta} = \sum_{j=1}^n [\eta'_{j0}I_{j0} - m_{j0}(I_0 - I_{j0})] \frac{\Delta_{ji}}{\Delta}$$

which makes use of the relation between net and gross elasticities, $\eta_{j0} = \eta'_{j0} + m_{j0}$. Notice that the sign of dP_i/dt_0 is not unambiguously determined; in both equations the coefficients of the cofactors are composed of one positive and one negative term, and we have no information about which term is dominant. It is therefore possible that some prices rise relative to the price of the exports of the tariff-imposing country.

This indefinite result differs from the two-country analysis where the net effect of the tariff is a pure substitution effect away from imports and to home goods. On the assumption that all exports are gross substitutes, and that no goods are inferior, all substitution effects are positive in the multiple-country analysis also; but it cannot, as far as I know, be proved that the net effect of the tariff is a shift of demand away from the imports of every country. A tariff applied to the exports of a particular country creates a deficit in that country's balance of payments equal to a pure substitution effect; but if, in addition, tariffs are applied to the exports of third countries, the income effect resulting from the distribution of the tariff proceeds of the third countries to some extent offsets or reverses the initial deficit in the first country's balance.

It should be possible to prove, however, that at least some foreign prices fall relative to the price of the exports of the tariff-imposing country, by making use of the proposition that net substitution must be away from imports and to home goods.

$$(45) \quad \frac{dP_i}{dt_{c0}} = \sum_{j=1}^n \eta_{cj0} y_{j0} \frac{\Delta_{ji}}{\Delta}$$

which is necessarily negative. Thus *the terms of trade improve with respect to all countries as a result of a tax on imported goods*. By similar analysis it can be shown that the terms of trade worsen as a result of an increased tax on the consumption of export goods. And by changing signs we get the criteria for the effects of subsidies on the consumption of import and export goods.

By similar analysis it can be shown that the typical coefficient of (42) resulting from a tax on the production of exportable goods is $\epsilon'_{pj0} Y_{j0}$, where ϵ'_{pj0} is the elasticity of supply in country 0 of the good exported by the j th country, and Y_{j0} is the domestic production (i.e., the production in 0) of the good exported by the j th country. The criterion is therefore:

$$(46) \quad \frac{dP_i}{dt_{p0}} = \sum_{j=1}^n \epsilon'_{pj0} Y_{j0} \frac{\Delta_{ji}}{\Delta}.$$

Thus if supply elasticities are all positive *a tax on the production of exportables improves the terms of trade of the taxing country with respect to all other countries*. [Note that each term in (46) is zero if country 0 is completely specialized.] The same criterion applies, *mutatis mutandis*, for production taxes on imported goods, or production subsidies. (See Figure 8.)

Unilateral Payments. Suppose that country 0 pays country S an annual tribute, or gift, or loan (ignoring interest payments). Then expenditure in 0 decreases and in S decreases by the amount of the payment. The demand for all goods in 0 decreases and in S increases if there are no inferior goods. At constant prices the deficit created by these expenditure changes in the balance of payments of the j th country is $(m_{j0} - m_{js})dT_{s0}$, where dT_{s0} is the value of the transfer. The criterion for the change in the price of exports in the receiving country, relative to the price of the exports of the paying country, is therefore:

$$(47) \quad \frac{dP_s}{dT_{s0}} = \sum_{j=1}^n (m_{j0} - m_{js}) \frac{\Delta_{js}}{\Delta}.$$

[In the term $j=s$ the coefficient is $(m_{s0} - m_{ss}) = (m_{s0} - c_s) = (m_{s0} + m_s - 1)$ where c_s and m_s are, respectively, the marginal propensities to consume and import in country S.] The unilateral payment, through expenditure changes in the transferor and transferee, rearranges demand throughout the world in a way which does not permit any a priori generalization, a result which can be expected from the analysis of transfer between two countries.³¹

³¹ The propositions in this section require modification if there are limiting cases such as infinite or zero elasticities, and zero or negative marginal propensities to spend.

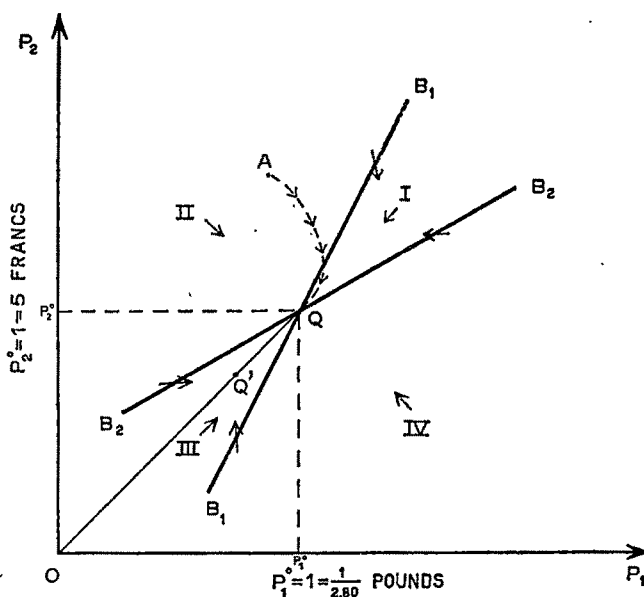


FIGURE 8. MULTIPLE-COUNTRY STABILITY CONDITIONS

Assume three countries, the U.S. (0), the U.K. (1) and France (2) with the respective currency prices (expressed in dollars) 1, P_1 and P_2 . Choose units of pounds and francs so that, at equilibrium, P_1 and P_2 are each unity. (If equilibrium ratios are $\$1 = 1/2.80$ pounds = 5 francs then $1/2.80$ pounds and 5 francs become the British and French currency units.) The two curves B_1B_1 and B_2B_2 trace the loci of pound-franc prices which allow equilibrium in the British and French balances. At Q both balances are in equilibrium implying, by Cournot's Law, equilibrium in the U.S. balance.

If all currencies are gross substitutes both curves have positive slopes: this follows because appreciation of the franc must be associated with appreciation of the pound to maintain equilibrium. Moreover, B_1B_1 has a slope greater than 45 degrees and B_2B_2 has a slope less than 45 degrees; this follows because of our choice of currency units and because appreciation of the dollar is equivalent to depreciation of the franc and pound in equal proportion; a movement along the line OQ from Q to Q' must improve the French and British balances and worsen the U.S. balance. Four quadrants can then be identified: east and west of B_1B_1 there are deficits and surpluses, respectively, in the British balance; and north and south of B_2B_2 there are deficits and surpluses in the French balance.

On the dynamic assumption that the dollar prices of pounds and francs rise and fall in proportion to the disequilibrium in the British and French balances, the arrows in each quadrant indicate the forces impelling a return to equilibrium. The reader may easily satisfy himself that the equilibrium position, once disturbed, will be restored. From the disequilibrium position A , for example, the path may follow the broken line AQ , becoming "trapped" in Quadrant I; or it may become trapped in Quadrant III and hence move to equilibrium.

To determine geometrically the movement of the prices as a result of policy changes it is necessary to indicate the direction in which the two curves shift. If U.S. output and expenditure increase, more British and French goods are demanded at constant prices improving both British and French balances; the two curves therefore shift away from the origin and the new equilibrium point moves to somewhere in Quadrant I. Similarly, for the tariff and tax changes analyzed in the text both curves shift toward the origin with an unambiguous improvement in the U.S. terms of trade.

One special case, however, is of interest in view of recent discussions of foreign aid and "tied" exports. Suppose that country 0 makes a gift to a foreign country but requires that the gift be spent on its own exports. (It may be supposed that the gift is financed by taxation and that the financial transfer involves the grant of credits from an export-import bank.) In this case the price of the exports of the paying country rises relative to the price of the exports of the receiving country (all the terms m_{ji} are zero). This does not imply, however, that the transfer "burden" is negative—that real income in the transferring country declines by less than the gift—since the prices of the exports of some of the third countries may rise. It further assumes that the receiving country does not re-export the tied exports to third countries.

Comparison of Two-Country and Multiple-Country Models. The foregoing account of policy changes in a system containing many national units naturally leads to more complicated conclusions than in the case of a system containing only two countries. Applying only the restrictions of stability, I have not been able to show that two-country and multiple-country systems lead to substantially the same conclusions. The difficulty lies in relations of gross complementarity among the exports of the various countries. Gross complementarity is consistent with stability in the multiple-country system but inconsistent with stability in the two-country system. The two-country model cannot therefore be expected to suggest "theorems which admit of extension to more concrete cases" when gross complementarity is involved.

However, when gross complementarity is absent—when all exports are gross substitutes—there is a remarkable similarity between the conclusions of the two models. Productivity, tariff and tax changes move the terms of trade in the same direction in the many-country system as in the simpler system. The explanation of this similarity lies in what may be called the Law of Composition of Countries. If all foreign exports are *perfect* substitutes for each other, the foreign countries can be aggregated into a composite country and called the "rest of the world"; in that case the conclusions of the two models will agree both qualitatively and quantitatively. But if foreign exports are only *imperfect* substitutes for each other exact results cannot be obtained by the use of a composite country. However, while the *quantitative* conclusions of the two models will in this case differ, the *qualitative* conclusions, with one qualification,³² remain. There is a presumption, then, that the use of two-country models will not be subject to serious error provided that all exports are gross substitutes.

³² The qualification refers to the treatment of income effects examined in footnote 31.

The most important generalization of the classical system has been provided by Mosak [18].

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MARX'S "INCREASING MISERY" DOCTRINE

By THOMAS SOWELL*

Economists often assume as almost self-evident that Karl Marx's prediction of ever-increasing misery for the workers under capitalism refers to a decline in the amount of goods and services they will receive. Some writers have implied that only the intellectually dishonest could deny this view. It is readily inferred that the interpretation of Marx to mean a decline in labor's relative share is only an afterthought of latter-day Marxists seeking to salvage something from the ruins of the prediction [18, p. 383] [1, p. 213] [23, pp. 155-57] [3, p. 324] [22, pp. 34-35] [16, p. 61]. While labor's relative share has not declined, this at least has the dignity of a plausible prediction which went unfulfilled, while a theory of absolute misery would be thoroughly discredited by history. That some consideration of this sort has in fact provided the subjective motivation for some statements on this point by latter-day Marxists is probable, but to say that this is the only possible basis for the "relative misery" interpretation is something very different. It will be argued here that relative misery was precisely what Marx's prediction referred to, in so far as it was concerned with the purely economic aspect of the workers' condition. It will be further argued that Marx was not solely concerned with this aspect.

A standard argument against the relative-misery interpretation is that while "some passages in Marx . . . bear interpretation in this sense, this clearly violates the meaning of most" [22, p. 35]. In order to avoid this charge, the argument that follows will not cite passages from Marx which "bear interpretation" as relative misery, but only such passages as bear interpretation in no other way. This argument, however, will not be simply a passage-quoting one, but will attempt to show how the substantive meaning of Marx's increasing misery prediction turns in part on the Marxian conception of the "value of wages"—which depends in turn on the whole value framework of Marxian economics, derived from Ricardian economics, whose peculiar conceptual framework caused similar misunderstandings of Ricardo long before Marx wrote *Capital*.

It will be shown that Marx was fully aware of Ricardo's peculiar

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conception of the value of wages, and explicitly endorsed it. In addition to exploring (1) the meaning of the value of wages in Ricardian-Marxian terms, and especially the meaning of a rise or a fall of wages in such terms, it will be necessary to consider (2) the meaning of Marxian "subsistence" and its relevance here, as well as (3) some of the arguments used to support the absolute misery interpretation, and (4) the noneconomic dimension of Marxian "misery."

I. *The Value of Wages*

Conventional economic theory has long made a distinction between the amount of money which the worker receives as wages, and the amount of goods and services which this money will buy. Adam Smith spoke of "real wages" and "nominal wages" in this sense.¹ David Ricardo, from whom much of Marx's economic apparatus is derived, also used the terms "real wages" and "nominal wages," but because of the nature of his system, they meant something very different. What Adam Smith and other economists called "real wages" was included under "nominal wages" by Ricardo [24, p. 50]. Real wages, in Ricardo's terminology, meant the value of wages, that is, the amount of *labor* contained in the commodities which the worker received. Ricardian real wages measured the degree to which the workers shared in total output, not the absolute amount of goods and services which they received.² If, due to increased productivity, the workers should receive a greater quantity of goods representing a smaller share of output, then—in Ricardian terms—wages would have *fallen*. Ricardo declared that "it will not the less be a real fall, because they might furnish him with a greater quantity of cheap commodities than his former wages" [24, p. 50]. Marx follows this same line of reasoning:

... it is possible with an increasing productiveness of labour, for the price of labour-power to keep on falling, and yet this fall to be accompanied by a constant growth in the mass of the labourer's means of subsistence [5, p. 573].

More important than such parallel statements is the fact that Marx saw the peculiarity of the Ricardian conception of wages and deliberately made it his own, not simply as an accidental by-product of using

¹ He was not always consistent as to the basis for the dichotomy.

² [24, pp. 49-50]. The point is more succinctly expressed by J. S. Mill: "In his [Ricardo's] language wages were only said to rise, when they rose not in mere quantity but in *value*. . . . Mr. Ricardo, therefore, would not have said that wages had risen, because a labourer could obtain two pecks of flour instead of one, for a day's labour. . . . A rise of wages, with Mr. Ricardo, meant an increase in the cost of production of wages . . . an increase in the proportion of the fruits of labour which the labourer receives for his own share . . ." [17, pp. 96-97].

Ricardo's so-called "labor theory of value," but because he, Marx, felt that there was a valid social philosophy implicit in this conception:

The value of wages has to be reckoned not on the basis of the quantity of necessities which the worker receives, but on the basis of the quantity of labour which these necessities cost—actually the proportion of the working day which he appropriates for himself . . . It is possible that, reckoned in use value (quantity of commodities or money), his wages may rise as productivity increases, and yet reckoned in value they may fall . . . It is one of Ricardo's greatest merits that he made an examination of relative wages and established them as a definite category. Previously wages had always been looked upon as a simple element, and consequently the worker had been regarded as an animal. In Ricardo, however, he is considered in his social relationship. The position of the classes in relation to each other depends to a greater extent on the proportion which the wage forms than on the absolute amount of the wage. [10, p. 320]

Marx seemed particularly concerned to emphasize the relative-share approach because of the economic prerequisites of increased wages under capitalism:

A noticeable increase in wages presupposes a rapid growth of productive capital. The rapid growth of productive capital brings about an equally rapid growth of wealth, luxury, social needs, social enjoyments. Thus, although the enjoyments of the worker have risen, the social satisfaction that they give has fallen in comparison with the state of development of society in general. Our needs and enjoyments spring from society; we measure them, therefore, by society and not by the objects which serve for their satisfaction. Because they are of a social nature, they are of a relative nature. [12, Sec. IV, p. 37]

But while it is clear that Marx regarded a relative decline in wages as a real fall in wages, this does not dispose of the possibility that he may have, in addition, felt that real wages in the conventional sense would also fall over time under capitalism. And in fact, the evidence seems to indicate that at an early period in his writings, probably up through the time of the *Communist Manifesto* in 1848, he did in fact believe that absolute impoverishment would be the lot of the working class under capitalism. The *Manifesto* flatly declared: "The modern labourer . . . instead of rising with the progress of industry, sinks deeper and deeper below the conditions of existence of his own class" [13, p. 36]. Marx's unfinished manuscripts of this period also seem to suggest a belief in either stable or declining real wages (again, in the conventional sense) [4, pp. 274-79] [15, p. 351]. All of this, however, was written before Marx's long years of study in the British Museum, and also at a time ("the hungry 'forties") when in fact the standard of living of workers seemed to be either unchanging or deteriorating.

Between this period in the 1840's and the publication of the first volume of *Capital* in 1867, German socialists and communists put forth the so-called "iron law of wages," which declared that wages could not rise under capitalism. Some latter-day critics have attempted to associate Marx with this "iron law" [21, pp. 531-32] [3, p. 311], but in fact Marx heaped scorn and ridicule on it in his *Critique of the Gotha Programme* in 1875. Significantly, he characterized it as a view that was now outmoded and constituted an "outrageous retrogression" in the light of recent, more "scientific" understanding of wages [7, Sec. II, p. 15]. Engels was less indirect in referring to the publication of the first volume of *Capital* which, he declared, now showed that the laws of wages "are in no sense iron but on the contrary very elastic," in contrast to the "antiquated economic view" represented by Lassalle's "iron law" [14, p. 335]. These vague and oblique references to "antiquated" ideas and "retrogression" only hinted at what Engels was later to state openly, that Lassalle's iron law was derived from his and Marx's writings of the 1840's and represented views now discarded.³

From the point of view of the developed Marxian theories, Lassalle's iron law represented not merely a false statement but, more importantly, a false issue. It was the exploitation of the worker that was the central issue to Marx, who declared that this exploitation must grow worse, *regardless* of whether wages go up or down:

... the wage worker has permission to work for his own life, i.e., to *live*, only in so far as he works for a certain time gratis for the capitalist ... consequently, the system of wage labour is a system of slavery, and indeed of a slavery which becomes more severe in proportion as the social productive forces of labour develop, whether the worker receives better or worse payment. ...

It is as if, among slaves who have at last got behind the secret of slavery and broken out in rebellion, a slave still in thrall to obsolete notions were to inscribe on the programme of the rebellion: Slavery must be abolished because the upkeep of slaves in the system of slavery cannot exceed a certain low maximum! [7, Sec. II, p. 15]

The idea that increasing misery accompanies the growth of capitalism "whether the worker receives better or worse payment" occurs also in *Capital*, where Marx declares that "in proportion as capital accumulates, the lot of the labourer, be his payment high or low, must grow worse" [5, pp. 708-9]. In fact, this statement occurs in the very same

³ Marx declared in *The Poverty of Philosophy* (1847): "The natural price of labour is no other than the wage minimum." Engels, in the German edition of 1885, attached to this statement of Marx's the following footnote: "The thesis that the 'natural,' i.e., normal, price of labour power coincides with the equivalent in value of the means of subsistence absolutely indispensable for the life and reproduction of the worker was first put forward by me [in 1844]. ... As seen here, Marx at that time accepted the thesis. Lassalle took it over from both of us" [8, p. 45n] (emphasis added).

paragraph as that statement so often quoted to support the interpretation of the "absolute misery" school:

Accumulation of wealth at one pole is, therefore, at the same time accumulation of misery, agony of toil, slavery, ignorance, brutality, mental degradation at the opposite pole. . . [5, p. 709]

This and similar statements do not support either interpretation, since it is precisely the meaning of such statements which is at issue.

II. *Subsistence*

An increasing misery which can occur in spite of increasing wages (in the ordinary sense) does not, however, preclude decreasing wages. Neither does the definition of rises and falls in relative terms, of itself, preclude absolute misery, since relative misery carried beyond a certain point would also be absolute misery. In order to see what limits, if any, Marx assigned to this relative misery, some consideration of Marxian "subsistence" is required.

Marx's subsistence has sometimes been regarded as being minimum physical subsistence, or something very close to it—or, at least, something *fixed* at a definite level. Some writers have extended this idea to mean that Marx assumes a subsistence level toward which wages might tend to fall over time [20, pp. 908-11, esp. 910n.]. But this particular theory is entirely absent from Marx. There is not a secular tendency for wages to fall *to* subsistence; rather, workers tend to be *at* subsistence, but the content of this subsistence changes, consisting as it does of both "natural wants" and "so-called necessary wants" which are "the product of historical development" [5, p. 190]. However one might object to Marx's (and other economists') use of subsistence in this sense, the substance of his meaning is plain. The value of a worker's labor-power is that "value" or embodied labor "required for the conservation and reproduction of his labour-power, regardless of whether the conditions of this conservation and reproduction are scanty or bountiful, favorable or unfavorable" [6, p. 956]. It is sometimes claimed that a wage level fixed at subsistence (in the ordinary sense) is a necessary condition for Marx's theory of surplus value [23, p. 94], but in fact it is only necessary to show a difference between the output of labor and the output required to sustain the laborer.⁴

Marx's picture of the worker at subsistence, therefore, does not preclude increases in real wages in the conventional sense. Once a new

⁴ Marx made this point in his criticism of the Physiocrats who assumed a fixed subsistence or value of labor-power: "If they made the further mistake of conceiving the wage as an unchangeable amount, in their view entirely determined by nature—and not by the stage of historical development, a magnitude itself subject to fluctuations—this in no way affects the abstract correctness of their conclusions, since the difference between the value and the profitable use of labor power does not in any way depend on whether the value is assumed to be great or small" [10, p. 45].

higher standard of living becomes established, it too becomes subsistence, and represents the new value of labor-power, i.e., the real-wage level. Marx does not have a determinate theory of wages; how labor shares in the increasing productivity is a matter of bargaining power: it "depends on the relative weight, which the pressure of capital on the one side, and the resistance of the labourer on the other, throws into the scale" [5, pp. 572-73]. The Ricardian-Marxian conception is here manifested in the word "resistance." The worker is resisting a *fall* in wages, although Marx declares that the "lowest limit" of this fall is a wage which will purchase the former sum of commodities [5, p. 572]. If wages fall to any point above "the lowest possible point consistent with its new value," then despite this fall, "this lower price would represent an increased mass of necessaries" [5, p. 573]. Marx credits Ricardo with the original formulation of this law. Far from being a law of increasing misery in the conventional sense, it represents a law of a customary floor under wages, which would *prevent* such an occurrence.

A crucial but unstated assumption in Marx's increasing misery doctrine is that the workers themselves will judge wage movements from this relative point of view; otherwise Marxian "misery" when accompanied by material prosperity need never provoke revolution. Another assumption in both Marxian and Ricardian illustrations is a falling price level with increased productivity, so that it is meaningful for them to speak of a fall in wages in money terms, as well as in value terms, and to speak of a "cheapening" of commodities.

III. Arguments for the "Absolute Misery" Interpretation

It is sometimes asserted that Marx's theory was that increasing productivity with the same capital-labor ratio would raise wages, but that capital-intensive (labor-saving) growth tended to decrease wages by causing technological unemployment, with the "reserve army of the unemployed" dragging down wages. This is true as long as it is kept in mind that wage movements are in value terms, and may be said to fall in money terms only on the Ricardian-Marxian assumption of increasing purchasing power of money with growing productivity. Moreover, capital-intensive growth introduced an element of *absolute* misery for those particular workers displaced by new technology. Marx brings this point in against those who argue a necessary connection between the growth of capital and the material well-being of the worker [12, Sec. V, p. 43]. He is further concerned to explode the contention that this is only a "temporary" inconvenience, by asserting that "since machinery is continually seizing upon new fields of production, its temporary effect is really permanent" [5, p. 471]. Capital-intensive growth,

for Marx, not only deprives the worker of his rightful share in increasing productivity by lowering his ability to "resist" the capitalist's encroachments, it causes an absolute decline in living standards for those displaced by machinery.

Marx's theory of the growth of what he calls "official pauperism" is sometimes cited as an argument for the "absolute misery" interpretation. However, as Marx defines his terms, "official pauperism" covers those no longer in the labor force, that is, "that part of the working-class which has forfeited its condition of existence (the sale of labour-power) and vegetates upon public alms" [5, p. 717]. The view that this group will tend to increase with the growth of capitalism is not the same thing as a theory of increasing absolute misery for the working class as a whole, and in fact is only a small part of Marx's picture.

Marx often speaks of a "tendency" of capitalists, or capitalism, to reduce wages in terms which suggest absolute impoverishment. The other side of Marx's coin, however, is the countertendency of the workers to increase wages as much as possible. He speaks of

... the continuous struggle between capital and labour, the capitalist constantly tending to reduce wages to their physical minimum, and to extend the working day to its physical maximum, while the working man constantly presses in the opposite direction.

The matter resolves itself into a question of the respective powers of the combatants. [11, Sec. XIV, p. 67] [*cf.* 5, pp. 657-58]

Although Marx seems to have a purely "bargaining power" theory of wages, the possible range of wages is limited by the customary standard of living as a floor, and by a ceiling representing wages high enough to threaten the existence of capitalism itself: "the oscillation of wages is penned within limits satisfactory to capitalist exploitation" [5, p. 843].

It has been claimed that Marx's increasing misery theory is logically inconsistent with his theory of a falling rate of profit [19, p. 36]. Obviously if one dichotomizes output into wage income and property income, both cannot fall simultaneously (unless output declines). But here again Marx's theory must be understood in Marx's terms. He divides output *three* ways: into wage income ("variable capital"), property income ("surplus value") and replacement of depreciated machinery and raw materials, etc. ("constant capital"). There is nothing to prevent the proportions of the first two from being less in later periods than in earlier periods, with a secular rise in the proportion of the third. In the Marxian theory it is the *rate* of profit which tends to decline, while the mass increases,⁵ and the share of labor that falls rather than ordinary real wages.

⁵ "The same laws, then produce for the social capital an increase in the absolute mass of profit and a falling rate of profit" [6, p. 256].

IV. *The Noneconomic Dimension of "Misery"*

The noneconomic aspect of increasing misery rests on Marx's philosophical approach and his underlying conception of man. The Marxian mode of analysis follows the Hegelian dialectical approach in so far as it tends to analyze things not simply as they are, but as they are potentially. This applies not only to social and economic systems, but also to man in general and the worker in particular. Thus the extent to which the worker is given scope for unfolding his inner potentialities is a vital part—if not *the* vital part—of his well-being.

Work, to Marx, is precisely the instrumentality of this realization of his potentiality by the individual, as well as by mankind. Work is not a mere disutility to be endured for the sake of satisfying material human wants—in which case material production would then be the measure of (absolute or relative) well-being. On the contrary, work is itself "life's prime want" [7, Sec. I, 3, p. 10], because it contributes to the development of the individual. This is dialectical development: "A caterpillar *grows* into a bigger caterpillar; it *develops* into a butterfly" [2, p. 80]. Not so the worker under capitalism: "If the silk worm were to spin in order to continue its existence as a caterpillar, it would be a complete wage-worker" [12, Sec. I, p. 22]. Yet work can be an end in itself, and its performance a satisfaction: "Milton produced *Paradise Lost* for the same reason that a silk worm produces silk. It was an activity of his nature" [10, p. 186]. Marx was annoyed at economists like Adam Smith who treated the expenditure of labor time "as the mere sacrifice of rest, freedom and happiness, not as [at] the same time the normal activity of living beings [5, p. 54n.]. World history, for Marx, represents the drama of "the creation of man by human labour . . ." [9, p. 246].

Under capitalism, Marx argues, work no longer fulfills its vital role in the lives of the people. The division of labor under capitalism "attacks the individual at the very roots of his life" [5, p. 399]. It converts the worker into "a crippled monstrosity" by developing his manual dexterity in a narrow detail "at the expense of a world of productive capabilities and instincts; just as in the States of La Plata they butcher a whole beast for the sake of his hide or his tallow" [5, p. 396]. What is more relevant to the present question, Marx declares that this must grow worse over time under capitalism. The same methods which increase productivity are methods which "mutilate the labourer into a fragment of a man" and "estrangle from him the intellectual potentialities of the labour-process in the same proportion as science is incorporated in it as an independent power [5, p. 708]. This is a vital part of

the picture which leads Marx directly to the conclusion that "in proportion as capital accumulates, the lot of the labourer, be his payment high or low, must grow worse" [5, pp. 708-9].

Although this aspect of well-being (or lack of well-being) is often overlooked by his interpreters, it was supremely important to Marx himself. He described it as "a question of life and death" that the worker under capitalism, "crippled by life-long repetition of one and the same trivial operation, and thus reduced, to the mere fragment of a man" be replaced by "the fully developed individual . . . to whom the different social functions he performs are but so many modes of giving free scope to his own natural and acquired powers" [5, p. 534].

V. *Concluding Remarks*

Because Marx saw a tendency for industry to become more capital-intensive, he postulated a secular decline in the proportion of outlays on wages (variable capital) to outlays on plant and equipment (constant capital) and property incomes (surplus value)—a "fall" in wages. This fall might conceivably go so far as to deprive the workers of any increase in their standard of living, but whether it would go that far depended upon the relative bargaining power of capital and labor, the latter "resisting" the fall in wages through trade unions, etc. If the fall could be arrested at any point above the previously existing subsistence level, then the "lower" wages would represent an increased quantity of goods, although the value of these goods would be less—that is, the workers would be spending less of their working day producing their own livelihood and more of it producing surplus value for the capitalists.

A more important question to Marx than the movement of wages, even in relative terms, was the question of the self-realization of men through their practical physical and mental exertions. Man, as the sum total of his potentialities, is only successively revealed through the exercise of his faculties. Under capitalism, Marx argues, the worker "does not fulfill himself in his work, but denies himself" and "has a feeling of misery . . ." [9, p. 169]. This misery becomes progressively worse with the growth of capitalism as the worker is increasingly estranged "from the intellectual potentialities of the labour-process. . . ." For Marx this is a fundamental deprivation which cannot be remedied by higher wages.⁶

⁶ Marx asserts that this is the situation for the class, but does not deny that gifted individuals may escape the class situation. His attitude towards social mobility in this context is distinctly negative, since he sees it as strengthening and perpetuating the system as a whole: "The more a ruling class is able to assimilate the most prominent men of a ruled class, the more solid and dangerous is its rule" [6, p. 706].

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ON GENERALIZING THE GENERAL THEORY

A Review Article

By ABBA P. LERNER*

{Ever since Keynes' *General Theory of Employment Interest and Money* appeared in 1936, economists have been tempted to show that it is not quite so general and to fit it into a *really* general theory. The latest attempt, and the most grandiose, is one by Professor Sidney Weintraub [5].

The practical problem sparking Weintraub's work appears to be the very important one of sellers' inflation (although he does not use this term). While buyers' inflation is caused by too much spending, i.e., by buyers trying to buy more goods than are available and thereby bidding up prices, sellers' inflation is caused by sellers raising prices even in the face of a deficiency of spending. A failure to distinguish between the two types of inflation aggravates a problem which has become a serious threat to democratic society.

The appropriate treatment for buyers' inflation is to cut down the excessive spending that causes it. This may be done by a restrictive monetary or fiscal policy. But if restrictive monetary or fiscal policy is used against sellers' inflation, spending is reduced when it is *not* excessive, so that we get a deficiency of demand, depression and unemployment. The inflation will continue, however, unless the induced depression is severe enough to destroy the power of sellers to raise their prices. This may call for more depression than the authorities are prepared to impose or the public willing to suffer. We then get insufficient depression to stop the inflation and we suffer from both evils at the same time. In 1958 we seem to have had sufficient depression to stop the inflation, at least temporarily, but this remedy involved a loss of output estimated at some \$30 billion per annum and it severely reduced our rate of economic growth.

Galbraith has suggested [1, Ch. 13, 21] that we are rich enough to pay this price for price stability. But it is not simply a matter of a reduction in our standard of living; we are involved in a competition with communism for the planet. The failure to provide full employment is a serious handicap in this race. The loss of the \$30 billion may be responsible for a fatal economizing in our expenditure on defense. We have a good chance of winning the uncommitted nations to the democratic side by contributing a major share of the cost of a development program which could make their economic development relatively painless; but the loss of the \$30 billion may make us feel we cannot afford it. And the reduction in our rate of growth can make it relatively easy for the communists to realize their dream of "catching up with and surpassing" the U.S. economy. vid
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The immediate practical lesson of Weintraub's book is the futility of treating sellers' inflation with the measures appropriate for buyers' inflation, and his hopes of getting this lesson across are probably heightened by his feeling that he has stumbled on a revolutionary reformulation of economics. The flamboyant publishers' blurb, vying with the all-embracing title of the book, promises "A Sensational Break-Through for the Economics of Price Theory" and announces: "A New Conceptualization of Major Importance Finally Solves the Riddle of the Price Level and Wage Costs . . . Where Keynes' Theory Failed . . . A New Perspective . . . Makes Economics a Predictive Science." And this is modest in comparison with the chapter headings in the book itself.

[One hesitates to put the pin to this bubble for fear of interrupting the salutary lesson on sellers' inflation, but it is important to show that Weintraub's revolutionary claims and the justifiable scepticism they engender do not diminish the validity of his lesson.]

I. Weintraub's Model and His Proposals

Weintraub's starting point is the observed stability of the ratio of the value of the social product or output or income to the wage bill, which ratio he calls k or the "magic constant." This k is the same as Kalecki's degree of monopoly [5, p. 41], or Marx's rate of exploitation, or the ratio between the exponents in the Cobb-Douglas production function. The constancy has been widely observed. Keynes has referred to it as "a bit of a miracle"; and it is regularly used to show that Marx's prophecy of the "immiseration" of the workers cannot be rescued even by supposing it to refer to the relative share of the workers rather than their absolute income. But nobody has ever become as infatuated with this relationship as Weintraub or as fascinated with the tricks that can be played by combining it with the rules of arithmetic in exercises reminiscent of the game of "think of a number" or of H. G. Wells' attribution of the fascination of card games to people's not having learned to count properly up to 13.

[The key trick is to transmute the value of the social output into the price level and the wage bill into the average wage by the appropriate multiplications or divisions. This yields a formula which has much in common with the equation of exchange of economic theory, but which is more useful because it deals with relationships that are much more stable. Unfortunately this is not too much of a recommendation, and unfortunately the new formula can confuse truisms with substantive statements just as much as the old one.

The basic relationship is:

$$(1) \quad Z = kW$$

where Z is the value of output, W is the wage bill and $k \equiv Z/W$.² Z is trans-

*¹ "Think of a number, multiply by 2, add 10, divide by 2, subtract 5, and presto you have the number you started with!"

² I am following Weintraub's notation except that I am keeping capital letters exclusively for observable quantities and lower-case letters for the relationships between these derived by the arithmetical operations, and I have renumbered the equations.

muted into P , the price level, by dividing both sides of the equation by the quantity of output q ($\equiv Z/P$); while the average wage w ($\equiv W/N$, where N is the number of employees) appears in the formula because Weintraub always uses wN for W . We thus get [5, p. 9]:

$$(2) \quad \frac{Z}{q} = \frac{Z}{Z/P} = P = kwN/q.$$

Introducing output per employee, a ($\equiv q/N$), and substituting we get:

$$(3) \quad P = kw/a,$$

and introducing unit wage cost, r ($\equiv w/a$), and substituting we get:

$$(4) \quad P = kr.$$

The price level is k times the unit wage cost.

Equations (2), (3) and (4) are the main forms, and (3) the one most frequently used, of the Wage-Cost Mark-up equation (WCM) that Weintraub puts up against the equation of exchange (EOE) [5, p. 7]:

$$(5) \quad Mv = Pq; \text{ or } P = Mv/q$$

where M is the quantity of money and v ($\equiv Z/M$) is the velocity of circulation of money. His claim is that the use of truism (3) instead of truism (5) would turn economics onto the right path and transform it into a predictive science, because k is much more stable (and therefore also much more predictable) than v , w is more predictable than M , and a is much more predictable than q , so that the MCM equation would enable us to predict P more successfully.

[Weintraub then goes on to derive an empirical value for k by dividing the Department of Commerce figures for business product (Z) by compensation of employees (W), and shows that between 1929 and 1957 k stayed between a high of 2.16 at the beginning of the period and a low of 1.87 at the end of the period [5, p. 14]. Indeed the figures show much greater stability still if we break up the period so as to separate out periods of war and of depression and to make some allowance for a downward trend.³]

[After deriving empirical values for the other variables in the WCM equations and hammering home the constancy of k by repeating the evidence in the form of index numbers and charts, Weintraub has an astonishing section [5, pp. 26-29] entitled "The Detection of the Price Level: Checking the Formula," in which he proves that between 1929 and 1957 the truism was almost

³ From 1929 through 1941 k stayed between 2.16 and 2.13, except for the depression years 1930-1934 and 1937-1939, when it stayed between 2.11 and 2.07. Through the war years 1942-1945 it stayed between 2.02 and 2.08. In the 12 years of prosperity, 1946-1957, it shows a declining trend, keeping between 2.00 and 1.97 through the first half, 1946-1951, and between 1.94 and 1.87 in the second half, 1952-1957. As against a range for 1929-1957 of 15 per cent, the subperiods show ranges between $1\frac{1}{2}$ and $3\frac{1}{2}$ per cent.

exactly true! P' the price level computed from the annual figures for k , w , and a , is shown to be very close to P the "actual price level" (the implicit price deflator for business gross product). This is actually plotted on a chart [5, p. 28] where the closeness of the two curves must greatly impress the impressionable.

The "remarkable confluence" of P' and P is not perfect primarily because the computed w is not really w and computed a is not really a . Data for w or W/N are unattainable because there are no figures for N , the number of people engaged in producing the business gross product and earning W , so Weintraub uses substitute figures (which I will mark with a prime). These are total employment, N' , which includes employment in government enterprise, and the corresponding wage bill W' . From these we can get W'/N' or w' which is a good approximation to w . Similarly, computed a is obtained by dividing q not by N but by N' . We may call this a' and we therefore have:

$$(6) \quad P' = kw'/a';$$

and spelling this out in the capital letters used for the actual data we have:

$$(7) \quad P' = \frac{Z}{W} \cdot \frac{W'}{N'} \cdot \frac{N'}{Z/P},$$

which reduces to:

$$(8) \quad P' = P \cdot \frac{W'}{W}.$$

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atic? The "remarkable confluence" of P' and P is nothing but the similarity of the index number of W' to that of W . Weintraub's recognition of this [5, p. 29, top] does not prevent him from expressing his elation at this "confluence" as a demonstration that "our method checks out" [5, p. 29, middle].

The next chapter (4) is entitled "The Law of k " and is devoted to stressing the constancy of k as "probably the most important economic law, in the true sense, that economists have to work with" [5, p. 33, in italics]. Greater and greater excitement is worked up by reducing k to index numbers, by making charts of k as well as of index numbers of k , by comparing it with more volatile series, by tabulating year-to-year changes in k , and by referring to similar phenomena at other times and in other countries. Weintraub expresses surprise at the high value of k in the depression years as compared to the 1950's [5, p. 36, where "1930's" is presumably a misprint for "1950's"], as well as at the postwar sag in k : "For this heaviness in k has traversed a period in which 'demand-pull' inflation has been widely described, an era in which consumer goods were in short supply, and a series of years in which a sellers' market has prevailed" [5, p. 42]. The main emphasis of this chapter however is not on the movements in k but on k being "perhaps as close as we are ever likely to come to finding a constant in the world of economic phenomena" [5, p. 43].

Chapter 5 is on "The Law of the Price Level" which is described as "an-

other interesting relationship," namely that "the price level will rise only if r rises" [5, p. 45]; and its introduction is celebrated by an impressive fanfare of tables, charts and correlations. But the law of the price level is *not* "another interesting relationship" at all. It is none other than the same k as it appears in equation (4), $P = kr$. ✓

Chapter 6 "On Predicting the Price Level" describes how we can predict P ($=kw/a$) as accurately as we can predict k , w and a . If k is stable (or falls slowly at a known rate), if w is determined by known union policy, and if a grows at a known and steady rate, we are all set to predict P . Furthermore, Weintraub argues, if we want to know the effects on P of a particular wage increase, say in steel, we have only to figure how much this will increase the *average* wage in the whole economy and "magic constant" k will do the rest!

Chapter 7, "The Wedding Ceremony: Price Level and Aggregate Output," ✓ combines the four items of (3), P , k , w and a , with consumption, investment, interest rates, liquidity preference and money, the five items in the simplest Keynesian aggregate output model, to make a model with 9 equations and 9 unknowns. "Money enters into the model only through the interest rate" [5, p. 73] and can affect money wages (and therefore prices) only "through the effect on q and N " [5, p. 73]. Novelty is claimed for making the demand for money "dependent not only on real output but also on the level of prices" [5, p. 72].

Chapter 8, "The Eclipse of the Equation of Exchange," is primarily a renewal of the attack on EOE, this time with charts that dramatize k 's great stability in comparison with v , after quoting some rhapsodies on the stability of the latter by Milton Friedman. In this chapter Weintraub also attacks the "Strange Denial of a Wage-Price Spiral" by certain EOE theorists and insists that WCM is superior to EOE because changes in w influence P directly, while changes in M (even if they are successful in increasing total expenditures Mv or Z) affect P only indirectly through their effects on w . ✓

Chapter 9, "A Miscast Federal Reserve," demonstrates briefly but very forcefully that the Federal Reserve Board, armed only with the power to vary the money supply, can check sellers' inflation only by bringing about severe unemployment. Weintraub goes on to argue that even this does not check inflation *in the long run*, because the retardation of investment by the induced depression must raise P because it slows down a , the increase in output per man, and as we have seen:

$$(3) \quad P = kw/a$$

In Chapter 10, "Wage Policy: A Way Out," Weintraub rejects any kind of regulation of wages or prices and proposes "watch-tower control" by research agencies publishing their findings as to the probable effects of projected wage increases on w , the average wage level, and so on P . He believes that such appeals to reason will work because when union leaders realize that k is constant they "can never again evade price level responsibility for any immoderate wage change exactions" [5, p. 92]. ✓

The rest of the book is devoted to refining k and developing the predictive

application of the new theory. Chapter 11, "Decomposing k " deals with the nonwage part of total output or income (which is equal to $k - 1$ times employee compensation), breaks this down into 8 segments and calculates the ratio of each of these segments to employee compensation for each year from 1929 to 1957. Weintraub then goes into ecstasies over the stabilities uncovered by the analysis, or resulting from "small numbers being compared to large" [5, p. 102], or created by the rounding of small ratios to the nearest percentage point, or appearing in the later part of the series. For example, net interest originating in business as a ratio of employee compensation ranges from .18 to .02 (so that the maximum is 900 per cent of the minimum). But from 1944 to 1957 it stays at .02 or .03 and Weintraub exclaims, "Can a series possibly hold more firmly than this?" [5, p. 95], although the rounding may conceal a range within this stable period reaching almost from $1\frac{1}{2}$ to $3\frac{1}{2}$ per cent with the top of the range 233 per cent of the bottom. Nevertheless, despite the nature of parts to be less stable than their sum, information about how they are expected to behave could improve estimates of k .

Chapter 12, "An Empirical Macroeconomic Theory of Income Distribution," although announced as "large steps toward" this, turns out to be nothing but the reconstitution of $k - 1$ from the parts into which it is split up in Chapter 11. Chapter 13 "A Final Price Level Generalization" is a repetition of the observation that k is about 2, so that if P can be expected to stay around twice r (the unit wage cost), P will fall 2 cents for every 1 cent fall in r . We are told that: "This is a result never reached or even suggested by the EOE. It indicates the wide range and deep penetrative power of the WCM-formulation" [5, p. 110].

[This however is not yet the climax. The last chapter "The Price Level Over Time: The Final Synthesis for a Growing Economy" provides a still higher power of the "think of a number" game. Here Weintraub combines:

$$(3) \quad P = kw/a$$

with a growth truism taken from Harrod:

$$(9) \quad g = s/c$$

where g ($\equiv \Delta q/q$) is the rate of growth of output or increase-in-output per (unit of) output, s ($\equiv S/Z$) is the saving ratio, S standing for absolute money saving, and c ($\equiv i/\Delta q$) is the marginal capital-output ratio, i ($\equiv S/P$) standing for real saving or real investment or marginal capital.

Harrod's truism says that the rate of growth is equal to the saving ratio divided by the marginal capital-output ratio. It is true because the saving ratio divided by the marginal capital-output ratio means saving (or investment) over income (or output) divided by investment (or saving) over the increase in output. The saving (or investment) cancels the investment (or saving) so that we are left with "over output" divided by "over the increase in output," and this is only an unusual way of saying "increase in output divided by output," which is the definition of the rate of growth. In symbols:

$$s \equiv S/Z = \frac{S/P}{Z/P} = i/q \quad \text{and} \quad c \equiv i/\Delta q,$$

so that:

$$(9) \quad s/c = \frac{i/q}{i/\Delta q} = \frac{1/q}{1/\Delta q} = \Delta q/q \equiv g.$$

This is all right as long as we remember that the marginal capital-output ratio c is *not* the marginal efficiency of investment and does *not* mean that an additional \$ c saved and invested would result in an additional \$1 of output per annum. The truism says no more than that, in the period observed, investment was c times the increase in output. The increase in output was due not only to the investment but also to increase in population, increase in skill, increase in technical and scientific knowledge, discovery of new resources, depletion of old resources, and acts of God like the size of the harvest or changes in the terms of trade or the behavior of the Board of Governors of the Federal Reserve System. It is very hard to remember all this all the time and that is why capital-output ratios and truisms like Harrod's are even more conducive to error than the EOE.

So far Weintraub's guilt is only by association, but in combining (3) with (9) Weintraub [5, p. 112] arrives at:

$$(10) \quad p = kw \cdot \frac{s}{c\Delta q/N}$$

and substituting b ($\equiv \Delta q/N$, the increase-in-output per man),⁴ he achieves [5, pp. 113-14]:

$$(11) \quad P = kw \frac{s}{cb}, \text{ or:}$$

$$(12) \quad P = kw \frac{g}{b},$$

"the final synthesis between the price level and the phenomena that are responsible for economic growth" [5, p. 113]. ✓

What these formulae tell us is that instead of saying "output per man" we can say "increase-in-output per man divided by increase-in-output per (unit of) output" because the "increase-in-output"'s cancel out leaving us with "per man" divided by "per (unit of) output" which means output per man. ✓

⁴ It must be noted that b is the increase-in-output per man and not the increase in output-per-man as is suggested by Weintraub's "per employee output growth rate" [5, pp. 112-13]. These two are equal only if N , the number of men, does not change. If q and N both increased by say 10 per cent, increase-in-output per man is 9 or 10 per cent, depending on whether we take the final or the initial N , while there is no increase in output-per-man.

In symbols this means that in the WCM equation:

$$(3) \quad P = kw/a,$$

instead of writing a , which stands for output per man or q/N , we can write $\frac{\Delta q/N}{\Delta q/q}$, because the Δq 's cancel out leaving us with $\frac{1/N}{1/q} = q/N \equiv a$. If we do this we get:

$$(13) \quad P = kw \cdot \frac{\Delta q/q}{\Delta q/N}.$$

If we now write g (increase in output per unit of output) for $\Delta q/q$ and b (increase in output per man) for $\Delta q/N$, we get:

$$(12) \quad P = kw \cdot \frac{g}{b}.$$

If, in accordance with equation (9), we write s/c for g we get:

$$(11) \quad P = kw \cdot \frac{s}{cb},$$

and if we now restore $\Delta q/N$ for b we get:

$$(10) \quad P = kw \cdot \frac{s}{c\Delta q/N}.$$

It is possible to do the same trick with "investment" as with "increase-in-output." Thus instead of output per man we can say investment per man divided by investment per unit of output. Instead of a (or q/N) we can write $\frac{i/N}{i/q}$. Weintraub does just this and [5, p. 115] comes out with:

$$(14) \quad P = kw \cdot \frac{i/q}{i/N}.$$

In exactly the same manner we could say horsepower per man divided by horsepower per unit of output, or the number of filter-tip cigarettes per man divided by the number of filter-tip cigarettes per unit of output. All of these are only queer ways of saying output per man; and the queerer the irrelevant (canceling) items the less the danger of forgetting its irrelevance and supposing that the equation "conveys some new information" [5, p. 115]. But "increase-in-output" is not sufficiently queer to make its irrelevance apparent, so that it leads Weintraub to suppose that the statement "So long as equipment per head grows faster than equipment per unit of output . . ." [5, p. 115] means something more than "So long as output per head increases. . ."

In an arithmetical example of "the predictive application" of the final synthesis, Weintraub uses language that might be interpreted as assuming that c , the marginal capital-output ratio, is indeed the marginal efficiency of investment,⁵ but this does not touch the validity of his conclusions because the i 's (real saving or real investment or marginal capital) cancel out and the Δq 's (increases-in-output) also cancel out, so that c , which $\equiv i/\Delta q$, also cancels out.

In equations (10), (11), (12), (13), and (14) we have five different expressions, each in turn taking the place of the last item in equation (3)— $1/a$ and all meaning exactly the same—men per (unit of) output or the inverse of output per man. If output per man (a) is constant (in which case $1/a$ and all its synonyms will be constant too), P will vary with w , growth or no growth. If we put some growth elements into the equation, sound or unsound, and then take them out again we will always be left with the relationship we started with. ✓

Some even more fanciful developments of this game, breaking up b , the increase-in-output per man, into investment and consumption, lead to "profound implications which are significant for economic development" [5, p. 118], such as that "high savings are thus capable of raising *future* real wages" [5, p. 117]. Reference in connection with this is made however not to Adam Smith or even to Ricardo, but to some puzzling propositions in Joan Robinson's *Accumulation of Capital* [5, p. 118n]. ✓

II. Evaluation

[Notwithstanding all these extravagances, Weintraub has his finger on a matter of the utmost practical importance—the need for anti-inflationary measures more acceptable than bleeding the economy with depression. It will be a pity if this valuable insight should fail to help develop such measures either because of the extreme conservatism of his practical proposals or because of the rather revolutionary claims in which the theoretical analysis is wrapped. Yet, it is necessary, having recognized the value and validity of the insight, to evaluate both the practical proposals and the economic-theoretical claims.

1. The practical proposals for checking inflation without resorting to depression to keep wages and prices from rising are most disappointing. Although his analysis concentrates (or even overconcentrates) on the wage rate as the crucial element that forces prices up, and although he observes that exhortation "blows quick and wears short" [5, p. 89], Weintraub pins his faith on giving publicity to estimated effects of proposed wage increases on the price level. But union leaders, rather than accepting responsibility for the effects wage increases have on the price level and endeavoring to practice restraint, may just as well turn the edge of the "magic constant," charging the monopolists with increasing P and blaming the implacable k for the inevitable increase in w forced on helpless union leaders who cannot buck the Law of k ! At any rate it seems more than likely that particular prices will continue to be raised by businesses and that particular wages will continue to be raised by

⁵ "Suppose that the amount of equipment needed to produce one unit of output—or \$1 worth—is \$5" [5, p. 113].

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isom

unions, neither the former nor the latter being ready to sacrifice particular interests for the sake of relatively small effects on abstractions like the general price level or the average wage level.

Furthermore, even if "watch-tower control" [5, p. 89] should indeed be effective in checking particular wage and price increases that threatened to raise the general price level (i.e., if businessmen and union leaders were persuaded to sacrifice their particular interests for the sake of the general good or for the sake of public applause) this would disrupt the proper price relationships since it would not provide for the relative movements of prices that are necessary in an economy subject to change.

2. The claim that changes in M or Mv can affect P only indirectly through the effect on w , amounts to a denial of the possibility of buyers' inflation. In this Weintraub is just as one-sided as those who deny the possibility of a wage-price spiral generated on the supply side by sellers.

3. Weintraub's proposals are all for stabilizing P by preventing w from rising more rapidly than a and never for holding down w by checking increases in P . This is a consequence of his one-sidedly seeing k as operating only from w to P and never from P to w . But the Law of k is perfectly symmetrical between w and P , and is no better for predicting P from w than for predicting w from P . If there is no excess demand and we know that w will rise in a certain manner, the Law of k tells us that P will rise proportionately. But if excess demand is bidding up P , the Law of k tells us that w will move up proportionately. They may both be pulled up by the market force of excess demand or pulled down by the market force of insufficient demand. Or they may both be pushed up administratively by powerful sellers, or pushed down administratively by powerful buyers. And yet all the time they stay in the same proportion k , so that if we know what will happen to either one of them, we can figure out what will happen to the other. The problem is to predict how *both* will move while held together in the stable relationship k or $1/k$.

4. One wonders why Weintraub should be surprised at k being lower in 1930-34 than in the years immediately before and after, or at the postwar decline in k . A lower k should be expected in depressions because profits fall more in depression than wages do. And it is natural that a long period of continuing prosperity should reduce k .⁶ Prosperity makes entrepreneurs more optimistic, so that they push forward their break-even point and lower their mark-up; it increases their readiness to expand and compete; and it lessens the degree of monopoly and the "rate of exploitation" by reducing the reserve army of unemployed workers. (All of these expressions are after all nothing but different jargon for a reduction in k .)

While the *expectation* of continuing prosperity has the effect of gradually lowering k by lowering the *planned* mark-up, an *actual* depression or recession also lowers k , though it does this suddenly by an unplanned fall in profits. However, it also strengthens the expectation of depressions as part of the natural order of good and bad years, and this has the opposite effect, raising

⁶ "But if the bad times should not be expected to come, the continued high, boom time profits being made would tempt more competition to come in and participate. This increase in competition would result in lower rates of price markup" [2, p. 179].

k . On these grounds I would predict that the (engineered) interruption of prosperity by the depression of 1958, by upsetting expectations of continuing prosperity, will have the effect of reversing the trend and *increasing* k in 1959 and 1960.

5. There is no novelty in making the demand for liquidity depend on the price level. Keynes did not need to make this explicit because it is clearly implicit in his measuring everything in terms of the wage unit instead of the dollar. Indeed the only purpose of this rather troublesome procedure was to take care of the need for more dollars when prices and wages are higher.

6. The stability of k is an essential ingredient of the whole Keynesian revolution. It alone makes it possible for the wage unit to be used as a kind of index number of prices. In an early popularization of Keynes' *General Theory* I spelled out why a change in w (in the short run in which a is given) would lead to a *proportional* change in P [3, esp. pp. 116-20].

7. Weintraub's well-taken and important criticism of the futility of treating sellers' inflation by a restrictive Federal Reserve *monetary* policy, fails to point out that the alternative that comes to mind immediately—a restrictive *fiscal* policy (which reduces v rather than M)—is subject to exactly the same criticisms. This gap is another indicator of the way Weintraub is often really thinking of the falsity of the quantity theory of money while criticizing the irrelevance of the equation of exchange. /

8. There seems to be an unresolved conflict between the academic and the trade titles of the book. The academic title is concerned with a theory that would lead to appropriate governmental or social *policy*, in particular for dealing with sellers' inflation when a restrictive monetary policy is futile or harmful and when P can be controlled only through w . Weintraub recognizes that monetary policy is under the control of the authorities while wage policy is not, and attempts to deal with this in his proposals for "watch-tower control." But the trade title emphasizes *forecasting* and declares that M (monetary policy) is harder to forecast than w (wage policy). Here the policy problem of regulating, controlling, or influencing w seems to have been abandoned or forgotten in favor of the forecasting problem of guessing what the government and everyone else will do or fail to do. A more careful working out is needed of the relationship between the internal problem of telling the government what to do and the external problem of guessing what in fact will happen.

9. The "trade" assumption that the course of w is *given*, in combination with the assumption that the direction of influence is always from w to P and never from P to w , leads Weintraub to argue that a demand or buyers' inflation would be recognized by an increase in k , and if that is not observed we cannot be said to have a buyers' inflation [5, p. 72n]. But if w *can* increase in response to an increase in P , observation of the behavior of k tells us nothing as to whether any inflation is a buyers' or a sellers' inflation.

10. Weintraub argues that even if a sellers' inflation is checked in the short run by an engineered depression, this only aggravates inflation in the long run because the set-back to investment reduces the growth of a , output per man. This argument implies that the time-shape of w is unaffected by the depres-

sion—which is contrary to the assumption that the depression is sufficient to check the inflation in the short run.

The argument is not really needed since the damage done by loss of output is reason enough for not using depression as the treatment for sellers' inflation. It is, however, possible to salvage two less drastic but still significant results: (a) If a , the increase-in-output per employee has been, and is expected to remain at, 2 per cent per annum it may require a 6 per cent level of unemployment to provide price stability by keeping w from rising at no more than 2 per cent. But if depression, by checking investment, reduces a below 2 per cent, it will take more unemployment to stop the inflation. w can no longer be permitted to rise at 2 per cent and unemployment must be increased to 7 per cent if that is the unemployment needed to stop w from rising relatively to a . The reduction in the increase in a necessitates a more severe depression to stop the inflation. (b) If the induced depression reduces a by more than it reduces w it will raise unit costs and aggravate the inflation even in the short run. If the effects on w are rapid while the effects on a are slow (which may be the case because the latter are cumulative results of the diminished rate of investment), the depression may be anti-inflationary in the short period but inflationary in the longer period. This is a truly dynamic effect and it may be what Weintraub has in mind, but he does not make it clear that it depends on the continuing depression reducing a by more than it reduces w .

11. Weintraub claims that his WCM approach integrates the microeconomics of the firm with the macroeconomics of the economy as a whole by applying to the latter the principle of the unit wage cost mark-up that determines the price of the product of the individual firm. This can be very misleading because the unit wage cost mark-up of the firm, k' , is very different from k (or Z/W), when production is split into a series of stages operated by different firms. In general k' will be very much larger than k because direct wages are only a part of the cost. On the other hand the unit *total* cost mark-up of the firm will in general be much less than k since it covers only one element in $k - 1$, namely the profits of the firm. The difference between the individual firm's mark-up and k is only another aspect of the unclosed gap between micro- and macroeconomics.

12. Weintraub's "wedding" of the theory of the price level to the theory of aggregate output is a kind of "silver wedding" or the marriage of a long-wedded couple; but it carries with it a nuptial air because of a mistaken identity. The wedding that economists have been looking forward to for a very long time is between the theory of the determination of the *general* or *absolute* price level and the theory of the determination of *particular* or *relative* prices. The analysis of the general or absolute price level has been integrated with the analysis of aggregate output ever since the Keynesian revolution, and both are part of macroeconomics. The analysis of particular or relative prices has been inseparable from the analysis of the output of the firm or the industry for a much longer period and both are part of microeconomics. The union we are still waiting for is one between micro- and macroeconomics, and not one between the price and output aspects of macroeconomics. That is now in its third decade.

III. *Another Approach to a General Theory*

In my own attempts to generalize Keynes—and nobody seems to be immune from this ambition—I have been led to believe that instead of arranging a marriage by combining macroeconomics and microeconomics in a single set of equations, it would be more fruitful to heed Marshall's warning that *natura non facit saltum* and to see whether "macro" and "micro" cannot be treated as limiting cases at the extremes of a continuum or spectrum. This is the way in which competition and monopoly have been brought into relation with each other. Long-term and short-term analysis are also best bridged in such a manner. There is plenty of precedent; and indeed it is just because such continuity can be utilized throughout economics that Marshall was led to his motto.

To be able to use this method one needs a measure by which one can locate the place of the observed phenomenon in the continuum. In the case of the competition-monopoly spectrum, for example, we may use the "degree of monopoly," or we may use the inverse of the elasticity of demand, but the more general measure is the degree to which one may safely neglect repercussions. A small seller in a large market may neglect the effect of his sales on the market price; this is perfect competition. The larger the seller in relation to the market the less defensible is such neglect, and so we pass through various degrees of imperfect competition to monopoly as some undefined limit. Going in a slightly different direction we observe that a small seller in a large market can disregard the reaction of his competitors to his price policy. The larger he is in relation to the market the more must he take into consideration the reaction of his competitors; and in the interweaving of his policy with the expected policies of his rivals we have the strategies of oligopoly. We may continue further along the same line and consider that when the seller is not only very large in relation to a particular market but of importance for the economy as a whole, he must take into account not only the reactions of competitors but those of other groups in the economy, his suppliers or his customers, who may find it worth while to organize a "countervailing power" against him. Beyond that, a powerful seller must take into account the reaction of the government, and we find ourselves gliding from economics through political economy into the realm of politics.

In the same way a small buyer may neglect the effects on *income* of his decisions to spend or not to spend. Although someone else's income must be reduced by a dollar when he spends a dollar less, he is not concerned with this. It will have no discernible effect on his own income. But as the buyer, or the group of buyers we are considering, becomes larger in relation to the economy, it becomes less and less appropriate to neglect the repercussion on income, because spending by the members of the group will significantly consist of purchases from other members of the same group. If the group consists of a quarter of the economy, then, in the absence of some special information that tells us otherwise, we would expect about a quarter of the spending to consist of buying from other members of the group. If the group wants to increase its saving by a dollar and expects to achieve this by consuming a dollar less, it will be disappointed. Its income will fall by a quarter

so that its saving will increase by only 75 cents. If a group consisting of half of the economy tries to save a dollar by consuming a dollar less, it will find itself saving only 50 cents. And finally, if we take the group that is the economy as a whole, we find that when it spends a dollar less, its income falls by the whole dollar and there is no increase in saving at all. The repercussion which could be entirely neglected for a small seller has now, in the other limiting case, become absolute. This is because there is now nobody else to absorb any part of the repercussion.

It is the inability of many to pay attention to the repercussions that leads them to balk at the bridge from "micro" to "macro" and to insist on the "self-evidence" of the proposition that a cut in prices would cure a depression, because a cut in the price charged by a small seller increases his sales. Such crude anti-Keynesianism is rescued by a more sophisticated and more valid argument in terms of the effect of price reductions on the value of cash balances. It is here that a more careful examination of a spectrum of price flexibility can clarify the issues even if the result is not exactly a wedding between Keynesian and classical positions, but merely their location on a spectrum that goes farther than these positions in both directions.

If there were perfect price flexibility, so that any deficiency in demand would make prices fall so rapidly and excess demand would make prices rise so rapidly that we would not have to worry about what happens while the adjustment is going on, there would be no room for "Keynesian economics," or indeed for any policy at all with regard to effective demand. No policy would be necessary to prevent depression as long as there were some floor under the supply of hard money in existence; and no policy would be necessary to prevent inflation as long as the quantity of money did not increase indefinitely. Any excess of demand would be cured by the reduction in the real value of the money stock as prices rose. Any deficiency of demand would be cured by the increase in the real value of the money stock as prices fell.

Policy is necessary because we do not have, and never can have, this kind of price flexibility. This is why those economists, like Pigou and Patinkin, who have explored the way in which perfect flexibility would work, have been careful to point out that they are not recommending reliance on price flexibility, or even on any available measures for increasing price flexibility, as an adequate public policy. With different degrees of imperfection in price flexibility different policies become appropriate, and it is these differences in the supposed position of the economy on a spectrum of price flexibility that explain the different policy recommendations rather than any subjective attachments of economists to different philosophies.

If there is imperfect flexibility downward of prices, the cure of a deficiency of demand by price deflation involves a protracted period of depression aggravated by continuing expectations of further reduction in prices which are still resisting the downward pressures. It then becomes appropriate to bring about the increase in the real value of the money stock by less painful methods, namely by increasing the money stock in nominal terms. The mechanism of recovery and the attainment of equilibrium and price stability then proceeds in exactly the same way as if the increase in the real value of the money stock

had come about without benefit of policy—namely through perfect price flexibility. This is the Keynesian general case of an increase in the money stock—in terms of “wage units” so as to include the case where it is brought about by deflation.

The next position on our spectrum is where the inflexibility has invaded the money market and the rate of interest fails to fall in response to the increase in the real value of cash balances (the Keynesian special case with the liquidity trap) or where investment and consumption fail to increase in response to a fall in the rate of interest. The secondary inflexibilities would not matter if prices in general were perfectly flexible, since an unlimited fall in prices would constitute an unlimited increase in the wealth of the owners of the cash and this would induce the necessary increase in spending anyway. In the absence of unlimited price flexibility, the same cure could be obtained by an unlimited increase in the nominal quantity of money. But where this is unpractical (and even where it might be practical) resort to a more direct method of increasing demand might still be desirable. The government can increase its own spending or induce more spending by others by reducing taxes (or use a combination of these measures, including a larger budget with its net positive effect on total spending). If monetary policy is inadequate it can be supplemented by fiscal policy.

Farther along the same spectrum we have the situation where prices not only refuse to fall when there is a deficiency of demand, but tend to *rise* if there is a level of demand that provides a satisfactory level of employment. Prices will then be stable only if there is considerable unemployment. In the absence of this degree of depression, wages and prices will be rising in a sellers' inflation, with labor blaming the wage increases on price increases, and business blaming the price increases on wage increases.

In this case, monetary and fiscal policy cannot provide full employment with price stability. If monetary and fiscal policy is applied so as to achieve price stability, it causes depression. If it is directed at achieving full employment, it causes inflation. Full employment together with price stability can be attained now only by providing an artificial market to replace the missing competitive market that would have prevented such price behavior. The administered prices and wages that rise as soon as unemployment is less than, say, 6 per cent (when 2 or 3 per cent is all that the genuine frictions make necessary) must be so administered, in the general interest, that the general price level is constant as long as there is no excess demand. Such price regulation, like the regulation of the prices charged by public utilities, would not be interfering with the free market. It would rather be interfering with the *interferences* with that administration of prices which destroys the free market and makes sellers' inflation possible. Sellers' inflation does not fit into the Keynesian scheme any more than into the classical scheme, because Keynes, like the “classical” economists, assumed that although prices might be reluctant to fall in spite of unemployment, they would not rise unless there was excess demand. Sellers' inflation is possible because prices are raised even at less than full employment as long as the unemployment is not severe enough to hamstring the price and wage administrators.

Another and closely related spectrum may be envisaged in the degree to which money is incorporated in models of the economy. At one end of this spectrum is the barter economy where monetary complications are absent. Moving along the spectrum, we come to models where money is spoken of but not brought into close relationship with the "real" economy, and money is supposed to affect only the absolute level of prices; the relations between prices, including the rate of interest, are determined in the barter department. Here the rate of interest stands for either the marginal efficiency of capital (when stationary states are considered) or the marginal efficiency of investment (where growth and decay are permitted to enter the analysis). Farther along the spectrum are models where changes in the quantity of money are not immediately sterilized by changes in the price level, because price flexibility is less than perfect, so that employment is not always full. This is the Keynesian analysis (including the modern anti-Keynesians) that stresses real-balance effects. Still farther along the spectrum is the region I would like to explore, where money is *completely* integrated in the model, where the enjoyment of liquidity is not considered any different from the enjoyment of any other good or service, and where the cash balance is perfectly at home with all other assets yielding liquidity as part of the income flow while constituting part of the assets of the household, just as houses yield house-room as part of the income flow while constituting a part of the assets of the household. It then becomes unnecessary to bring in real-balance effects as a complicating afterthought and it is much easier to deal with the problems of price levels and employment. In such models it would be convenient to speak of wealth as a vector containing both assets and income, so that an increase in either, the other being constant (as when there is a change in the rate of interest) would constitute an increase in wealth.

Perhaps a more interesting spectrum would be that of the degrees of equilibrium. This might dispose of the tiresome arguments as to whether unemployment equilibrium is not a contradiction in terms. Here we might start with the most equilibrial kind of equilibrium, where there is no excess or deficiency of demand whatever that might sooner or later result in some change in the price even with perfect price flexibility. Such an equilibrium by definition rules out any involuntary unemployment.

Farther along the spectrum we find an equilibrium in which there is involuntary unemployment and it does not disappear—either because there is a rigidity of wages and prices so that the situation is frozen, or because prices and wages are both falling so that there is no incentive for employment to be increased. This may be because the increase in the real value of the money unit is being canceled by decreases in the supply of nominal cash or is being offset by increases in the desire to hold cash, so that the rate of interest does not fall; or because investment and consumption are unresponsive to the rate of interest, while the effects of the increase in the real value of the money stock on the propensity to consume (the Pigou effect) is small in the period we are considering. This is the Keynesian unemployment equilibrium. Any increase in output would result in losses that would push output back to the previous level.

In describing this equilibrium, Keynes tells us that while he has rejected the classical postulate that the wage is equal to the marginal disutility of work, he still maintains the other classical postulate that the wage is equal to the value of the marginal product. (We may extend this to include imperfect competition by changing this to say that the marginal cost of labor is equal to the marginal value product of labor). Workers would be happy to do more work at the same rate of pay, so that labor is off its supply function. This is why this equilibrium is not as equilibrated as the first one on this spectrum. Nevertheless we are justified in calling it an equilibrium because there is a certain stability about the situation. Keynes opened himself unnecessarily to criticism by supposing that an increase in employment would necessarily lead to a fall in real wages (though he later recognized that this was not essential to his model) saying that labor was not in a position to reduce the real wage,

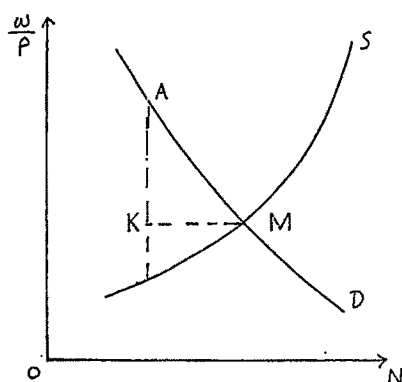


FIGURE 1

because a reduction in the money wage makes prices fall proportionately so that the real wage stayed the same. What is relevant is not the inability of labor to reduce the real wage but its inability to bring about an increase in demand which increases employment—whether this would lower the real wage or *raise* it.

We can deal with this better if we move farther along the spectrum to an even less equilibrated equilibrium, which is at the same time closer to the realities of a depression. At this point on the spectrum, the *second* classical postulate is rejected,⁷ namely that the wage is equal to the marginal product of labor (or the marginal cost of labor is equal to its marginal value product).

In a depression business is no less eager to sell more products at the current price than labor is eager to do more work at the same wage. In Figure 1 (following Patinkin) the actual situation is represented by the point *K*. The *S* curve shows how much labor would be supplied at various real wage rates (w/p is the wage-price ratio— w is the wage and p is the price level). The *D* curve shows how much labor the employers would demand *if they could sell*

⁷ This is done by Patinkin referring to a suggestion by Liviatan [4, p. 218 ff] but Patinkin seems to underestimate the importance of his and Liviatan's innovation.

the corresponding amount of product at each wage-price ratio. But they are unable to sell that much product so they only demand the quantity of labor indicated by K . Full equilibrium with full employment would be at M where workers would find all the work they want and employers would sell all the goods they want to sell at the prevailing wage-price ratio. Since K is off both curves it shows that *neither* of the classical postulates holds. If we were to follow Keynes and reject only the equality between the wage and the marginal disutility of work, we would be at A , and the real wage would be higher than at full employment. If we rejected both postulates we might argue, as Patinkin does, that, starting at M , a fall in demand would result not only in involuntary unemployment (we would be off the supply curve) but also in business depression (we would be off the demand curve too), and both wages and prices would fall. If they both fell at the same rate, we would be at K , with the same real wages as at M . If prices fell faster than wages, real wages would rise and we would be above K ; and if wages were to fall faster than prices we would be below K . What happened to real wages therefore would depend on the relative flexibility of wages and prices; and there is no necessity for the negative relationship between real wages and employment indicated by Keynes.

There is however no need to assume, as Patinkin does, that at any point to the left of M wages and prices must be tending to *fall*. This assumption is itself the result of declaring a dichotomy between full employment and unemployment when we can do better by supposing a spectrum. If we succumb to the temptation to assume a simple dichotomy between pure unemployment, when an increase in demand increases only output, prices staying unchanged, and pure full employment when an increase in demand increases only prices, because output cannot increase, we get a curve of the supply price of output in relation to the wage level that consists of a left-handed L , as in Figure 2—a horizontal line AF showing a constant supply price up to full employment at F when the curve suddenly becomes vertical. No matter how high the price goes (relatively to the wage) the quantity supplied cannot be increased. (The vertical measure in this diagram is the *inverse* of the real wage, which is measured vertically in Figure 1. It is p/w instead of w/p .)

This curve can also be read as showing (the inverse of) the real wage for any level of employment. Up to full employment the real wage is constant. At full employment the real wage is indefinite, depending on how the race goes between rising prices and rising wages in the inflation. Where one would be on the curve would depend on what business is able to sell as determined by the state of business or over-all demand.

With this model one is tempted to identify full employment with price stability because the corner point, F , seems to indicate both. If demand increases above F output cannot increase but prices rise. There will then be abnormal profits and a competitive increase in the demand for labor. Wages will be bid up until the same real wage is restored and the demand curve once more passes through F . If demand decreases there is a reduction in output but the price-wage ratio is unchanged. There are now unemployed workers, business is slack, and there is pressure on wages and prices to fall. If prices and

wages fall rapidly without limit, demand will increase and the demand curve will rise until it cuts at F again. If wages and prices are sticky, or if they fall but it takes a long time before the resulting increase in the value of the money stock raises demand so as to restore full employment, we have underemployment equilibrium. This, however, is temporary, even though it may be temporary for a very long time if nothing else is done about it. We still have prices rising whenever the demand is above F and prices falling or tending to fall whenever the demand falls below F . F therefore seems to represent both full employment and price-level equilibrium.

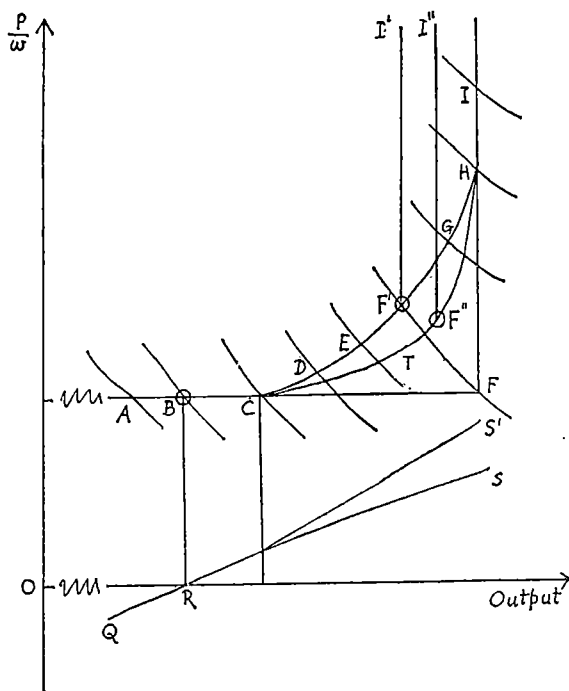


FIGURE 2

But this need not be the case at all. It is quite possible to have less than full employment—to be on the horizontal arm of the supply curve—and for prices and wages to be rising. Of course this cannot be due to excess demand. The increase in prices and in wages cannot be a market response. It can occur only if prices and wages are being pushed up administratively by sellers. We then have a sellers' inflation. Buyers' inflation can be cured only by removing the excess demand that causes it. Sellers' inflation can be cured only by measures that remove the power of sellers to raise prices. Reducing the level of demand may do this, but only by creating a depression severe enough to break down the administrative power of sellers to push up their prices. If we do not want to suffer this degree of depression, which may be quite severe, we must

either continue to suffer from the inflation or we must clip the power of sellers by other means—namely by a regulation of administered prices so as to make these behave the way they would in a competitive market.

As long as sellers retain the power to raise prices administratively, we can mark off on the horizontal arm of the supply curve the point *B* where the degree of unemployment and depression is just sufficient to check sellers' ability or willingness to raise prices. This is the level of employment at which wages rise no more rapidly than productivity so that we have price-level equilibrium. We may call it *price-level equilibrium employment*. To the left of *B* prices will fall and we will have deflation, and to the right of *B* prices will rise and we will have inflation. But it is not buyers' inflation but sellers' inflation. Only when we get above *F* will the inflation be a buyers' or demand inflation. Keynes assumed that the point of full employment is also necessarily the point of price-level equilibrium employment, so that only buyers' inflation is possible.

In my *Economics of Employment* I spoke of price-level equilibrium employment, but I called it "Low Full Employment," and I treated the power of labor to raise wages as one of various frictions that prevented unemployed factors from moving to where they could be used. Because of all these frictions, any further increase in demand could only raise prices. It now seems to me to be misleading to call *B* any kind of full employment; and misleading to lump together the power of sellers to raise prices and genuine frictions that prevent factors of production from moving from one area or one occupation to another.

Through the point *R*, directly below *B*, we can draw a line *QRS* indicating the rate of movement of prices that would accompany any given level of output and employment. To the left of *R* the line lies below the axis showing that prices would fall, and the further to the left, i.e., the lower the volume of output and employment, the lower the point on *QRS* and the greater the rate at which prices would fall. At *R* wages rise just rapidly enough to balance the rate of increase of productivity so that prices are stable. To the right of *R* the line lies above the axis indicating that the price level would rise, and that the greater the volume of employment the greater would be the rate of price rise because of the greater power of the price administrators to raise prices. At *F*, or full employment, the power of sellers to raise prices becomes irrelevant because beyond this point there is excess demand. The market itself then raises prices whether there exists any administrative power to raise them or not. Sellers' inflation then bows out of the picture and buyers' inflation takes over.⁸

A closer look blurs this clear distinction between sellers' inflation and buyers' inflation. Full employment is not reached suddenly; it also is a matter of degree. As we move up the level of employment, long before resources everywhere are fully utilized we will come across bottlenecks and increasing marginal costs as capacity is approached.

⁸ If any level of employment other than *R* persists for some time, the rate of change of prices (whether upwards or downwards) comes to be anticipated, the movement is accelerated, and the rate of price rise or of price fall will be greater. The line *QRS* would swing round *R* in an anticlockwise direction, becoming steeper in the process. The longer the movement persists the steeper will the curve become and the inflation or deflation will develop into hyperinflation or hyperdeflation.

If we were to consider the increase in employment as requiring the application of increasing quantities of labor to a given supply of capital, we might expect to find increasing marginal cost almost from the beginning of the curve, but this would be so only if we were considering a very long run in which capital was properly adjusted for permanent operation with each volume of employment measured along the horizontal axis. If, on the other hand, we were concerned with short-run problems of output and employment varying with an industrial equipment designed for some normal level of employment of the current labor force, then the supply curve would be fairly horizontal for some time before we got into problems of crowding the capacity. We would then have a curve of supply price like that marked in Figure 2 by the alphabet from *A* to *I* at the points where a series of demand curves cut it.

As before, *B* is the point of price-level equilibrium employment, with wages rising no faster than can be matched by rising productivity. At *C* there would be a pure sellers' inflation with prices rising at a rate indicated by the height of the *QRS* curve above the horizontal axis directly below *C*. But now we have increasing costs and bottlenecks before we get to perfectly full employment; and prices rise more than wages, so that the real wage falls as we move along the supply curve up the alphabet. If wages did not change at all, prices would rise as shown by this curve and real wages would fall in that proportion. We now have an additional reason why wages would rise: Not only is the bargaining power of labor raised by the higher level of employment, the pressure for wage increases is intensified by the attempt to make up at least what is lost in real wages by the price rise, and the resistance to wage increases is diminished by the feeling that workers have just cause for complaint. This is indicated by the replacement of *RS* by *RS'* with a greater rate of wage and price increase for each level of output and employment above *C*. At some point, say *F'*, the combined pressure for wage increase becomes so great that any further increase in employment is prevented. A further increase in demand would make wages rise as rapidly as prices, so that p/w would stay the same and we would only get inflation (not visible in Figure 2). We may therefore call *F'* "full employment."

A higher level of employment could be reached only by measures that succeeded in reducing the genuine frictions so that more of the unemployed factors might be moved to where they could be used. This would lower the curve of supply price to a position like *ABCTF''HI*. (The complete removal of all frictions would bring us back to the reverse *L* curve *AFI* that we started with, *CF'H* coinciding with *CFH*. There would then be no bottlenecks or increasing costs until there was perfectly full employment at *F*.) The diminution in frictions would give us *T* as the level of output and employment reached with the same demand conditions, with a higher real wage (lower price-wage ratio), and a greater volume of employment. It would make possible a still further increase in output and employment in response to a further increase in demand to a new full employment level *F''* where wages would once more rise so rapidly as to prevent any further increase in demand from increasing employment any more.

Possible positions are then bounded on the right by a vertical line through the full-employment level F' or F'' so that the effective curve of supply price is $AF'I'$ or $AF''I''$. But the economy may very well find itself off the supply price curve anywhere *above* and *to the left of* it as long as the point is not so high as to show a real wage reduced below the level at which workers are able and willing to work. Such points would be like K in Figure 1 where, in a depression, both workers and employers would be happy if demand were greater and output and employment were greater at the existing price-wage ratio. Prices would meanwhile be rising if the point were to the right of B (price-level equilibrium employment), and they would be falling if the point were to the left of B . The point would be moving up if prices were rising relatively to wages, and down if the converse were true; and this movement might be increasing or decreasing the level of demand and thereby the level of output and employment. These would be short-run effects which could affect output and employment in either direction. The long-run tendency, if the quantity of money had any stability, would be for the value of the money stock to increase if the point was to the left of B and for the value of the money stock to fall if the point was to the right of B . The long-run equilibrium toward which the economy would be tending would therefore not be full employment at F , F' or F'' , but price-level equilibrium employment at B , or anywhere above B as long as it was below the height in Figure 2 at which the real wage is so low that workers are unable or unwilling to work).⁹

At points like F' or F'' it becomes very difficult to say whether we have buyers' inflation or sellers' inflation. I would be inclined to say that F' and F'' , like F , are points where sellers' inflation yields to buyers' inflation. But strictly speaking, at such points there is no over-all attempt to buy more goods than are available, and if sellers were not able to exert any influence on prices there would be no inflation at F' or F'' , or anywhere short of H . The real wage would indeed fall very low, but in the perfectly competitive market where sellers' inflation is impossible nothing could be done about this as long as labor was available, and there might be even more labor available as a result of the low real wage. On the other hand, at any point to the right of C at least a part of the pressure for increased wages, and for increased prices to cover increased wages and other costs, would be the result of increases in *demand*.

It may be that further development of this analysis will turn out to be fatal to the concept of sellers' inflation, of which I have become quite fond, but it seems to me that the unification and clarification of economic theory will be better served by the Marshallian approach of searching for continuities than by the dialectical or ceremonial union of opposites.

⁹ This limit is not a reflection of the supply curve of voluntary employment, since the lower the real wage (in the economy as a whole) the *greater* would be the supply of labor by workers sacrificing leisure for minimum subsistence. It would be a reflection of the conditions of the supply of labor only in the sense that it would be affected by the diminished survival of healthy workers; and this would be better represented by redrawing the whole supply curve to show the changed nature and size of the working population.

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COMMUNICATIONS

Relative-Prices and Income-Absorption Approaches to Devaluation: A Partial Reconciliation

The question whether devaluation would improve the trade balance of the devaluating country has been analyzed, in recent years, with the tools of two different approaches, the relative-prices approach and the income-absorption approach.¹ Quite often it seems that the use of different tools of analysis has called forth also different substantive conclusions. The purpose of the present note is to show that if the same set of assumptions is used, the same conclusion must follow whether one prefers to adopt one analytical method or the other.

Both approaches would grant that when there is a possibility for devaluation to increase employment and output in the devaluing country, the trade balance is likely to improve. The difference of opinion arises when there exists no possibility of an increase in output (that is, starting from a position of full employment). Here relative-prices theorists would still claim that devaluation is bound to improve the trade balance—provided, of course, that the foreign-trade elasticities are “right.” On the other hand, adherents of the income-absorption approach would state that this conclusion is unwarranted unless it could be shown first that domestic absorption is reduced.

In order to focus attention on this contrast, let us assume that the “foreign” elasticities (of supply of imports to the devaluing country and of demand for its exports) are infinite, so that foreign prices of exports and imports are given and unchanged by devaluation. Let us further assume that the starting position, before devaluation, is one of full employment, so that devaluation cannot increase employment and output.

Since foreign prices are given, devaluation means necessarily a proportionate increase in local-currency prices of exports and imports. If other local prices do not change, or at least rise less than in the full proportion of devaluation, profit maximization implies that factors of production should move from purely domestic industries into those which produce export goods or import substitutes; likewise, goods which could be either exported or sold in the local market should now become exports to a larger extent than before. This would mean, obviously, an improvement of the trade balance. This, in essence, is the way the relative-prices proponent looks at the effect of devaluation.

His adversary would, however, start with the basic identity $C + I + G = Y + MS$ (where C = consumption, I = domestic investment, G = government expenditures, Y = national income, and MS = import surplus); would

¹ The former, more traditional approach is best expounded by Fritz Machlup [1]; while the latter approach is largely connected with the name of Sidney S. Alexander, who first gave it an explicit form in [2]. Alexander has recently presented a modified version of this approach in [3].

say that Y is fixed by assumption of full employment; and would therefore argue that MS cannot decrease unless one of the components of domestic absorption— C , I or G —also decreases. If no effect of devaluation on one or more of these components may be shown, it would be concluded—without any regard to relative prices—that devaluation would not affect the trade balance.

Since both arguments must be correct—one by the hypothesis of profit maximization, the other by definition—one must show that if devaluation increases the prices of exports and imports, relative to domestic prices, it also cuts absorption; or, conversely, that if devaluation does not result in the curtailment of absorption, then it also could not end up with a relative increase in the prices of exports and imports. If correctly carried out, both approaches must lead to the same conclusion.

For convenience, I shall start by supposing that devaluation does not reduce the demand for absorption. It has to be shown, then, that the prices of exports and imports relative to domestic prices do not change; that is, that the general price level in the devaluing country increases in the proportion of devaluation. The argument proceeds as follows:

In the first stage after devaluation relative prices of exports and imports rise and the trade balance improves. This means that the amount of goods available for domestic absorption is smaller than it had been before devaluation (since it has been assumed that output and the terms of trade are not changed by devaluation.) But the demand for domestic absorption has not changed, so that (assuming an initial position of equilibrium) there is now an excess of aggregate demand for goods over their aggregate supply, and this tends to raise the price level. The excess demand and the tendency of the level of prices to rise will prevail as long as the price level has not increased in the full proportion of devaluation. Only when the general level of prices increases as much as the price of foreign exchange, and the trade balance returns to its predevaluation level, are we in a new equilibrium position.² In this way the relative-prices approach reaches the same conclusion as the income-absorption approach.

There is, however, one implicit assumption in this argument: that money is a "dependent" variable, that is, that the amount of money is adjusted and is expanded sufficiently so that the price level may increase in the proportion of devaluation. The result would be quite different if instead the amount of money remained fixed. To demonstrate this, I shall employ a very simplified version of Patinkin's model [4]. It will be assumed that absorption consists solely of consumption (that is, domestic investment and government expenditures on goods and services will be disregarded). The demand for consumption is assumed to be a positive function of real income and real cash balances. The bond market is not included in this model so that the rate of interest plays no role. Consumers have the choice of either holding money balances or spending on goods, but not of lending money. These simplifications do not detract, I believe, from the usefulness of the following analysis.

Let us examine two extreme positions; first, the one in which the general

² In the terminology used by Alexander in his more recent paper, this would mean that the "reversal factors" offset completely the initial effects of devaluation.

level of prices increases as much as the price of foreign exchange. The trade balance is unchanged compared with the predevaluation period, and since real income and output do not change, by our assumptions, the amount of goods available for domestic consumption (= absorption) does not change either. But demand for consumption falls, since the increase in prices, combined with a fixed amount of money, means smaller real cash balances. This is not an equilibrium position: an import surplus the same as before is too high relative to the new equilibrium balance. The unsatisfied equilibrium condition is the consumption function.

The other extreme possibility, where the general level of prices (including prices of imports and of exportable goods) is the same after devaluation as before it, cannot be an equilibrium position either. Here, the prices of international goods relative to other prices having been increased, there must be some improvement of the trade balance, so that the amount of goods available for domestic consumption must fall. But demand for consumption is still at its predevaluation level, since neither real income nor real cash balances have changed. Hence there would exist a state of excess demand. This, again, is not an equilibrium position. An increase in the relative price of foreign exchange is not compatible with unchanged demand for consumption.

Equilibrium will be reached, then, somewhere in between the two extreme positions. In the new equilibrium situation the price level will be higher than before devaluation, but less than proportionate to the degree of devaluation. There will thus be an increase in the relative prices of exports and imports compared to prices of domestic goods, and therefore an improvement in the balance of trade. The amount of goods available for domestic consumption will be smaller than before devaluation. At the same time, the demand for consumption will be smaller, too, since real cash balances will be reduced (while real income does not change).³

To summarize, an increase in the ratio of international to domestic prices, which is essential for a decrease in the import surplus according to the relative-prices approach, can take place if and only if there is a decrease of absorption, and a decrease of absorption can occur only if there is an increase in the general price level. Hence the two approaches to the analysis of devaluation must lead to the same conclusions.⁴

This analysis is obviously oversimplified. It stresses the dependence of absorption on the real value of money balances, while it abstracts, for instance, from the real value of other assets as possible determinants of absorption, or from possible effects of changes in relative prices on aggregate demand for goods and services. Yet it would be worth while to conclude by pointing out the crucial role of monetary policy followed after devaluation whenever the assumptions of this analysis are largely applicable. A "neutral" monetary policy, which imposes no limitation on the expansion of credit and on the

³ These results could also be derived more rigorously mathematically.

⁴ Both in his earlier paper [2], and even more strongly in his recent contribution [3], Alexander recognizes and stresses the cash-balance effect as one which may reduce aggregate demand. Alexander does not show, however, the relation between this effect and changes in relative prices.

money supply, would render the devaluation a failure, under these conditions—unless, of course, devaluation is accompanied by other measures, for example fiscal ones, which work directly to curtail domestic absorption. A restrictive monetary policy, on the other hand, would lead to a successful devaluation under the present assumptions—provided that prices are flexible and that the institutional set-up in the devaluing country does not create a situation of “cost inflation,” where a restrictive monetary policy may create unemployment rather than inhibit a price rise.

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A Comparison of the Distributional Effects of Inflation and Taxation

As far as I know, only one attempt has been made to empirically evaluate the burden of inflation [1]. Bach and Ando estimated the distributional burden of inflation by measuring the monetary assets and liabilities of various income groups. They have shown that while the government sector is the most important net debtor, the household sector is a net creditor. This is true about the sector as a whole and about each income group within this sector. However, different income groups were discovered to have different ratios of monetary assets to liabilities. The closer this ratio is to unity, the smaller is said to be the loss caused by inflation.

The conclusion that everybody loses by inflation in proportion to his net money assets is not fully satisfactory, because it rests on the implicit assumption that the alternative to the burden of inflation is no burden at all. Actually, the real alternative facing the public is whether to bear the cost of inflation or the cost of some economic policy designed to achieve equilibrium through other means: direct controls, or monetary policy, or fiscal policy, or some combination of all. Even if inflation originated completely with the consumer and thus could be prevented by the use of self-restraint, the burden of the additional self-restraint needed to prevent inflation would have to be taken into consideration.

In this paper I shall compare the distributional burden of inflation with the distributional burden of taxation. Three types of taxes will be considered: (a) income tax, (b) sales tax with food not taxable, and (c) sales tax with food taxable. A number of assumptions, some of them quite heroic, will have to be made:

1. I assume a situation in which the supply of goods and services is perfectly inelastic and in which the demand for goods and services can be pictured as a rectangular hyperbola; in other words, the demand function is based on the quantity theory of money. Suppose that the demand function shifts because velocity increases. If nothing else happens, prices will increase until the quantity demanded is again equal to the quantity supplied. Alternatively, the government could tax away a sufficient amount of money assets to push the demand function back to the original level. The total loss to the public of real money assets would be the same in either case.

Should the demand function be based on the Keynesian determinants of demand, then the price adjustment would shift the demand function to the left by decreasing the real value of money and thus affecting the money market. Taxation would cut down the demand through its effect on disposable income and on the consumption and investment functions. In this case the losses caused by inflation and taxation need not to be identical: their ratio would depend on the slopes of the relevant functions. It is conceivable that the spending spirit may be dampened more (or less) by taxation than by inflation. Lack of necessary information about the shapes of the relevant functions makes it impossible to use this theoretical framework.

Therefore I rely on the simpler, and empirically more manageable, analysis based on the quantity theory of money. I assume that the market will reach equilibrium if the public loses a given number of real dollars, regardless of whether the loss is caused by a price increase or by an increase in taxes.¹ While not entirely satisfactory on theoretical grounds, I believe this assumption to be adequate for the purposes of the rough estimate which I am undertaking. If important public issues are at stake, it is better to give an imprecise, tentative answer than no answer at all.

2. I assume that the taxes imposed as an alternative to inflation would be such that they would leave unchanged the proportions of the total income or sales taxes paid by each income group.

3. I neglect the fact that a general price increase would push some taxpayers into a higher income-tax bracket and thus lead both to losses of real money assets and of disposable income.

4. I assume that the entire sales tax is passed to the consumer.

5. I assume that the public does not hold shares issued by various corporations but "composite shares" in which all the stocks in existence participate proportionately. This assumption is necessary in view of the fact that the corporate sector as a whole is inflation-proof while many of the corporations are not.² By making this assumption I shall avoid the need to consider the possibility that some population groups are holding a higher proportion of

¹ This really means that I am assuming the income velocity to be uniform throughout the entire income range. If I were using a naïve Keynesian model, this assumption would imply a belief in a uniform marginal propensity to consume throughout the income range.

² It can be shown that a one per cent increase in prices in early 1950 would have caused all corporations to gain \$7 million—a negligible amount. However, some types of corporations would have lost amounts up to \$295 million, while other types of corporations would have gained up to \$128 million [4, Vol. III, Tables W-22 through W-43].

TABLE 1—MONETARY ASSETS AND MONETARY LIABILITIES OF
VARIOUS INCOME GROUPS, EARLY 1950

Family Income before Taxes (1)	Monetary Assets (billion dollars) (2)	Monetary Liabilities (billion dollars) (3)	Net Mone- tary Assets (billion dollars) (4)	Capital Loss Caused by Inflation (million dollars) (5)
1. Under \$1,000	7.50	4.70	2.80	28
2. \$1,000 to \$1,999	10.80	4.20	6.60	66
3. \$2,000 to \$2,999	20.10	10.80	9.30	93
4. \$3,000 to \$3,999	21.80	13.40	8.40	84
5. \$4,000 to \$4,999	18.40	9.90	8.50	85
6. \$5,000 to \$7,499	26.70	12.80	13.90	139
7. \$7,500 and over	35.80	9.40	26.40	264
8. Total	141.40	65.20	75.90	759

Source: Columns 2, 3, 4: Raymond Goldsmith *et al.*, *A Study of Savings in the United States*, Princeton 1956, Table W-46.

inflation-proof shares than other groups. Lack of data makes this evasion a necessity.

6. I assume that changes in the rate of interest resulting from inflation or taxation can be ignored.

I. The Burden of Inflation Compared with the Burden of Three Types of Taxes

What are the costs imposed on various income groups by a uniform one per cent increase in prices? This degree of inflation is a very modest one; as such, it will make less unrealistic the restrictive assumptions. Table 1 contains the data on the monetary assets of various income groups, debts contracted in terms of the monetary unit, and the differences between these two amounts. All the income groups are net creditors to a smaller or greater degree.

While gauging the effects of inflation on various income groups, it would be an error to consider the net position of these groups only. Obviously the various spending units within each group will not be identical; some may be creditors to a greater degree than others, some may even be net debtors. In so far as this is true, in addition to the redistribution of wealth among the various income groups resulting from inflation, there will be a redistribution of wealth within each income group. However, the latter cannot be statistically measured.

Table 1 shows that a one per cent inflation will deprive the public of 759 million (real) dollars. Thus, in Table 2, I "impose" an income tax high enough to collect this amount. The last column of this table quantifies the burden such a tax would impose on the various income groups.

David C. Davies has recently specified the impact of a sales tax on the population of selected cities in 15 states [2]. He uses three alternative income concepts: gross income, net income, and disposable receipts. The gross- and

TABLE 2—ALLOCATION OF AN ADDITIONAL INCOME TAX, 1950 INCOME AND TAX RATES

Family Personal Income Before Tax	Income (billion dollars)	Income Tax Rate	Additional Anti-Inflationary Income Tax (million dollars)
Under \$1,000	1.943	0.0	0
\$1,000 to \$1,999	11.333	2.2	10.2
\$2,000 to \$2,999	20.273	3.8	31.7
\$3,000 to \$3,999	29.983	4.5	55.6
\$4,000 to \$4,999	31.533	5.3	69.8
\$5,000 to \$7,499	51.181	7.5	158.3
\$7,500 and over	71.736	14.7	433.4
Total	217.262	—	759.0

Source: U. S. Dept. of Commerce, *Income Distribution in the United States by Size, 1950-53*, Washington 1955, Table 10.

net-income concepts are identical with the traditional concepts of family income before and after taxes. The concept of disposable receipts includes certain money receipts other than the traditional ones and net changes in the spending unit's asset position with a view to providing a better index of permanent income. To assure comparability with my own data, all of which pertain to gross income, I employ Davies' figures based on the gross-income concept.

By using his figures I am making the assumption that the sales tax pattern

TABLE 3—EFFECTIVE SALES TAX RATES, FOOD TAXABLE AND FOOD NOT TAXABLE, FAMILY PERSONAL INCOME IN THE UNITED STATES, AND ADDITIONAL ANTI-INFLATIONARY SALES TAX

Income Group	Effective Tax Rate—Food Taxable	Effective Tax Rate—Food not Taxable	Family Personal Income (million dollars)	Additional Anti-Inflationary Sales Tax	
				Food Taxable (million dollars)	Food not Taxable
Under \$1,000	2.141	2.697	1.943	12.9	15.8
\$ 1,000 to \$1,999	1.430	1.403	11.333	50.4	47.9
\$ 2,000 to \$2,999	1.374	1.198	20.273	86.6	73.2
\$ 3,000 to \$3,999	1.326	1.202	29.983	123.5	108.6
\$ 4,000 to \$4,999	1.297	1.212	31.533	127.1	115.1
\$ 5,000 to \$5,999	1.261	1.243	25.603	100.3	95.9
\$ 6,000 to \$7,499	1.114	1.241	25.578	88.6	95.6
\$ 7,500 to \$9,999	1.045	1.087	23.364	75.9	76.5
\$10,000 and over	0.690	0.990	43.652	93.6	130.2
Total			217.262	759.0	759.0

Source: D. G. Davies [2], Table 1 C, p. 74. U. S. Dept. of Commerce, *Income Distribution in the United States by Size, 1950-53*, Washington 1955, Table 10.

prevalent in the 15 states analyzed by Davies would be the pattern selected by the federal government should it adopt sales taxes (with food either taxable or not taxable) as the alternative to taxes collected by inflation. The burden of an additional anti-inflationary sales tax of the kind described here is shown in Table 3.

I have considered three alternative methods for equating aggregate demand and supply: increased income tax and increased sales taxes of two sorts. If they are not imposed, excess demand will result in an inflation of one per cent. In all three cases the ratio of taxes paid by various income groups was left undisturbed. In Table 4 I compare the burden which these three equilibrating methods and the alternative of inflation will place on various income groups.

TABLE 4—THE BURDEN OF INFLATION, EQUIVALENT INCOME TAXATION, AND EQUIVALENT SALES TAXATION ON VARIOUS INCOME GROUPS, BASED ON 1950 INCOME, ASSETS, AND TAX STRUCTURE

Family Income Before Taxes	Number of Family Units (millions)	One Per Cent Inflation (in dollars)	Resources Collected per Family by: (in dollars)		
			Equivalent Income Tax	Equivalent Sales Tax	
				Food Taxable	Food not Taxable
Under \$1,000	3.861	7.25	0.00	3.34	4.09
\$1,000 to \$1,999	7.464	8.84	1.37	6.75	6.42
\$2,000 to \$2,999	8.091	11.50	3.92	10.70	9.05
\$3,000 to \$3,999	8.586	9.77	6.47	14.38	12.65
\$4,000 to \$4,999	7.054	12.04	9.89	18.02	16.32
\$5,000 to \$7,499	8.530	18.29	18.56	22.14	22.45
\$7,500 and over	5.304	49.77	81.71	31.86	38.97

Source: Tables 1-3. Numbers of families taken from U. S. Dept. of Commerce, *Income Distribution in the United States by Size, 1950-53*, Washington 1955, Table 10.

According to this table all families with incomes below \$5000 will profit if excess demand pressures are combatted by the use of income taxation. The income group receiving from \$5000 to \$7500 will find inflation to be the least burdensome tool of the equilibrating process, while the highest-income group will prefer a sales tax.

If this analysis is correct, it is surprising that we are continuously threatened with inflation; 35 million families out of the total of some 49 millions have a clear incentive to support the use of an alternative equilibrating tool, namely, the use of income taxation. Why does this majority of taxpayers fail to act? The absence of a vigorous anti-inflationary fiscal policy may be due to the superior influence wielded by the high-income groups. Or it may be that the taxpayers are not faced with a complete set of choices, but only with a very few crude ones. They can press the policy-maker to remain inert—and thus permit inflation—or to initiate some tax action. Such tax action, due to the relatively clumsy nature of all political processes, is bound to be undefined

at first and assume definitive shape only after protracted Congressional discussion. It would not be unreasonable for each group of taxpayers to fear that the taxation finally adopted will not be the one they would choose themselves. If so, then the alignment for and against inflation is likely to change drastically. Given a choice between inflation and the *worst* of the tax alternative considered here, 29 million families would find inflation less burdensome. Only 19 million families would prefer any one of the three taxes to inflation.

II. Sales Tax Reconsidered

Until recently, a sales tax was considered by many as the most disreputable tool of fiscal policy, endangering prosperity and oppressing the poor. In the past year, two voices were raised in defense of sales taxes. J. K. Galbraith in *The Affluent Society* [3, p. 248] points out that currently there exists a stalemate between the conservatives, supporting sales taxation, and the liberals,

TABLE 5—EFFECTS OF A SHIFT FROM ONE PER CENT INFLATION TO EQUIVALENT SALES TAXATION, 1950

Family Income before Taxes	Gains (+) or Losses (−) in Terms of Family Income before Taxes (per cent of income)		Sales Tax as Percentage of Inflationary Tax	
	Food Taxable	Food not Taxable	Food Taxable	Food not Taxable
Under \$1,000	+ .77	+ .63	46.1	56.4
\$1,000 to \$1,999	+ .14	+ .16	76.4	72.6
\$2,000 to \$2,999	+ .03	+ .10	93.1	78.7
\$3,000 to \$3,999	− .13	− .08	147.0	129.3
\$4,000 to \$4,999	− .13	− .09	149.5	135.4
\$5,000 to \$7,499	− .09	− .10	135.9	137.8
\$7,500 and over	+ .13	+ .08	64.1	78.3

supporting progressive income taxation. The resulting failure of the government to increase governmental receipts in turn leads to a failure to provide essential services (education, medical care) to the poor. Davies approaches the problem from a different standpoint. His empirical examination of the burden sales taxes place on various income groups leads him to the conclusion that the sales tax is a regressive tax only if one uses the gross- or net-income concepts as the basis for judging. When one shifts to the concept of disposable receipts, as a better approximation of the economically more meaningful concept of permanent income, the sales tax becomes a progressive tax for incomes up to \$6,000 [2, p. 74].

This paper suggests a revision of the attitude towards sales taxes. While a sales tax is a regressive tax, if we use Davies' gross- or net-income concepts as criterion, an inflationary tax³ is still more regressive (see Table 5). There-

³ The losses caused by a one per cent inflation represent the following percentages of gross income: Gross income under \$1000: 1.44; \$1000 to \$1999: .58; \$2000 to \$2999: .46; \$3000 to \$3999: .28; \$4000 to \$4999: .27; \$5000 to \$7499: .27; \$7500 and over: .37 (Tables 1 and 2).

fore the imposition of a sales tax, regardless whether food is or is not exempt, to avoid inflation results in a tax system that is more, rather than less, progressive. After we shift from inflation to sales taxation, the total real value of accumulated net money assets plus current disposable income becomes greater for all three low-income groups. Their inflation-caused tax liability decreases on the average by 20 per cent if food is taxable and 27 per cent if it is not, while the liability of the lowest-income group drops by 54-44 per cent. The next three income groups bear progressively a higher and higher burden of such a shift. The highest-income group, as also in Davies' study of the sales tax only, is an exception to the general progressivity of the shift. In this respect we find ourselves in a curious predicament: the spending habits and the tax and assets structures are such that an attempt to benefit the lowest-income groups forces us to pay a bounty to the high-income groups. One way out of this dilemma would be to modify our present structure of sales taxes so as to tax more heavily the purchases of "luxury" items; another solution would be to extend the coverage of the sales tax so that it would cover purchases of goods and services which currently are escaping the tax: purchases of life insurance, stocks and bonds, pension rights, etc.

III. Conclusion

Though the statistical evidence available for the purpose of comparing the redistributational effects of inflation, income taxation, and sales taxation with food exempt and with food not exempt is not fully satisfactory, I believe the results of this study show at least the relative order of magnitude of the four alternative tax burdens. The results indicate that the three lowest-income groups suffer more from inflation than they would suffer from the three alternative types of taxation we have considered. A shift away from inflation to any one of the three types of taxation would make our tax system more progressive. Obviously the goal of progressivity would be served most were we able to exchange inflation for income taxation. However, the study reveals that even a shift towards either type of sales taxation would mean the imposition of a more progressive system of taxation, with the exception of the highest-income group.

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Elementary Economics and Depreciation Accounting

Most introductory courses in economics describe depreciation, a technical accounting concept with far-reaching implications for economics, in extremely misleading terms. Students are confused by depreciation in abbreviated explanations of business financial statements or in discussions of national income accounts. Kuhlman and Skinner, in a recent text, are typical of the first. "Businesses, like consumers, save by not spending all of their income. They may set aside depreciation and obsolescence allowances in order to be able to replace plant and equipment" [9, p. 180]. The explanation does not jibe with Paton and Littleton on corporate accounting standards: "Under accrual accounting, depreciation is not a valuation process nor a means of capturing replacement prices from customers" [11, p. 17]. Jome, in a text on money and banking, states that "Good accounting policy seems to dictate that the purpose of the depreciation charge, after all, is to recover the money paid for an asset already constructed" [7, p. 429]. But the accountant May denies this policy: "It is a gross error to assume that it is a part of the function of accounting to insure by depreciation charges or otherwise, the return of the investment that has been made in the entity" [10, p. 24].

The basic problem here is that economists are using a word which has a "dictionary" or layman's meaning, but which also has a technical definition in the field of accounting. If economics insists on precise definitions for its own purposes of such common words as investment, capital, labor, profit, and competition, then should it not also recognize this practice in other fields? Accountants draw a clear distinction between Webster's definition of depreciation and their technical one, which they describe as an artificial convention. The American Institute of Accountants has formulated a standard expression for this arbitrary or conventional usage which can be duplicated by reference to leading authorities in the field of accounting. "Depreciation accounting [is] a system of accounting which aims to distribute the cost or other basic value of tangible capital assets, less salvage (if any), over the estimated useful life of the assets in a systematic and rational manner. It is a process of allocation and not valuation" [3, p. 76].

The outlook of accounting records is to the past. Depreciation charges refer to an expenditure which has taken place, and are merely a special method of writing history. Depreciation accounting enables the business firm to make several ledger entries, instead of one, when a capital expenditure occurs. The problem of replacement, which is not the purpose of depreciation, requires an outlook to the future. Sunk costs, expenditures which have already taken place, have no relevance to present or future investment opportunities. The accountants' use of depreciation—merely a method of recording a cost—ensures that no backward glance to a past expenditure which is over and done with can influence the rational consideration of a new decision on capital investment.

Some text-writers, fascinated by the role of "internal" funds in expanding business capacity, discuss depreciation charges as a "source" of money capital. "Among the various sources of capital, depreciation charges are extremely

important. Indeed, during the postwar years, corporate business as a whole raised more money in this way than it did through the sales of stocks and bonds" [13, p. 134]. "Depreciation reserves against relatively new equipment become available for current use. . . . The tremendous size of modern depreciation allowances means that the business community . . . may greatly increase its productive capacity without ever tapping new outside savings" [1, p. 180].

But nowhere in an elementary discussion have I seen reference to the obvious but vital fact that it is revenues, not charges, which provide money capital, and that depreciation accounting generates "internal" funds only when the firm is making profits, or when its losses do not exceed depreciation expense. As a result, many students rightfully wonder where, if "depreciation is only a bookkeeping entry" and depreciation reserves "do not consist of a pool of cash," the money *does* come from, when replacement time rolls around.

The 1958 depreciation charges of \$34.7 billion amounted to 50 per cent of gross private savings, a proportion which has fluctuated in recent years. Of the total *funds* available to corporations, \$19.6 billion or more than half was attributed to depreciation. Another \$6.0 billion represented retained profits. As funds to be spent by the firms, both these sums consist of money payments by purchasers for the output of corporations. Business prices, and sales, were \$25.6 billion higher than they would have been if depreciation costs had not been figured and if the firms had not made profits. The accounting technique of charging depreciation is no more responsible for the firm's revenues than is the necessity of paying for labor or material. The total funds available for investment by corporations, some \$30 billion, were used to acquire physical and financial assets, and to reduce liabilities. But there is no necessary correspondence between any one form of investment and any one source of the funds thus spent. Any part of retained profits, or borrowed capital, or money acquired through new issues of securities, or the difference between revenues and out-of-pocket expenditures which represents depreciation charges, may be spent on one or many uses. Replacement of worn-out or obsolete equipment is only one kind of possible capital expenditure, and the means of financing such expenditures bear no relationship to the means of recording the cost of previous expenditures. Perhaps the easiest way of emphasizing the lack of correspondence would be to review the years during the 30's and during the second world war, when depreciation charges exceeded expenditures for new plant and equipment. In the first period, depreciation charges did not provide funds for replacement because firms made losses, and their revenues did not cover the amount of the depreciation charges. In the second period, funds were provided, not by depreciation accounting but by higher wartime sales, yet these funds could not be spent on plant and equipment.

This disregard of the process of generating funds leads the text writers, when discussing price changes, to overstate woefully the effect of original cost in depreciation accounting. For example, "Suppose the price of a piece of equipment costing \$10,000 has doubled by the time it wears out. If a reserve had been accumulated at the rate of \$1,000 a year, when the machine had to be replaced at the end of ten years the accumulated \$10,000 would be only half enough to replace it" [6, p. 93]. But where has the \$10,000 been "ac-

accumulated?" If, in fact, the previous condition of making a profit or not making losses in excess of depreciation charges has been met, revenues to the firm will provide a ten-year stream of money capital amounting to \$1,000 a year. This money capital may be invested in inventories, accounts receivable, fixed plant, or the reduction of liabilities. If the firm is profitable, this new money capital presumably earns income, and at the end of ten years, therefore, the firm will be better off not only by the \$1,000 generated yearly, but also by the profits earned on this sum during the ten years, so that considerably more than \$10,000 will accrue. At only 8 per cent per year, for example, the ten-year proceeds will amount to \$14,487. Furthermore, it seems unrealistic to assume that the price doubling took place entirely within the tenth year, so that we may expect the firm's management, in expending this yearly \$1,000 of new capital, to have notice of current cost increases, and, presumably, use these in justification of price increases by the firm.¹

Textwriters can find some support, of course, in the criticisms of original cost depreciation given by business executives. A typical statement from management, in answer to a questionnaire from the *Journal of Accountancy*, reads, "I believe most everyone agrees that the increased cost of almost everything has made the provision for depreciation . . . inadequate to serve the purpose for which it was intended; namely, to provide funds for the replacement of plant and equipment" [2, p. 78]. DuPont Vice-President T. Crawley Davis was quoted recently urging the recovery, through depreciation allowances, of "the original investment in terms of current purchasing power" [5, p. 2]. In 1947 the management of United States Steel, without the approval of their independent auditors, charged extra depreciation expense, beyond that based on original cost, in order "to give some recognition to increased replacement costs" [14]. What chiefly concerns business executives, however, is not the abstract problem of analyzing inflation, but the concrete possibility of securing more favorable tax treatment by altering the definition of allowable depreciation. As Jones points out, "When businessmen say that depreciation is inadequate because it does not provide for replacements, they are simply taking an easy short-cut which avoids the complications of a full explanation" [8, p. 81]. The short-cut presents an impressive argument for altering the base of the corporation income tax; but such a proposal must be considered on grounds of equity of tax treatment and administrative possibilities, rather than being adopted because of a misunderstanding of the nature of depreciation.² The elementary texts do not provide the student with an accurate explanation of the accounting technique, and they infer rather careless ignorance on the part of the accounting profession.

The problem of how to treat changing price levels in accounting statements has been debated fervently and brilliantly for many years within the accounting profession. The advantages of a general price index and specially con-

¹ Cf. detailed calculations of the effects of depreciation charges in a period of rising prices, with given assumptions as to the rate of profit and of price chances [12].

² The report of a symposium conducted by the Tax Institute [15], illustrates clearly that the present concern over depreciation methods in times of inflation reflects the 52 per cent corporation tax rate. It should be remembered that depreciation accounting was generally accepted by U.S. business only after the income tax provided a financial incentive to do so.

structed indices to deflate current dollar figures have been argued at length. The consensus seems to be [4], reflecting the accountants' reliance on the principle of complete disclosure, that attempts to explain the effects of price changes are indeed worthy, but that such explanations should, for now at least, remain supplementary. Conventional accounting statements record the dollars and cents of transactions which have in fact occurred. No one interpretation, stemming from the problem of price changes or from any other single problem, should be allowed to supplant these basic data.

The economist has a perennial concern with real, as opposed to money, values, and a current concern with the influence of changing price levels upon capital investment. But the emphasis on depreciation charges as a means of *replacement* of plant and equipment within the firm leads to a misleading analysis of the capital consumption allowances, based on business depreciation figures, which are deducted from the gross national product.

National income or output can be estimated to measure welfare or to measure productivity, and some of the differences implied in the two concepts have been explored at length. From a welfare point of view (if you believe in freedom of consumer's choice) government expenditures on goods and services differ from business or consumption expenditures because consumers are not free to choose, by money votes, what government services will be produced, and because taxes must be paid. From the productivity angle, government product equals government purchases of goods and services because no market price exists for the output of government. The same problem arises in estimating the amount of capital consumed in any one time-period.

Conceptually, it is easy to say that current net national product consists of the goods and services currently available for consumption and investment after providing for the maintenance of capacity required to produce the amount of net national product. "Capital consumption" therefore would be the *current* loss in productive capacity of *currently* valued capital goods. The major difficulty in measuring this amount, as in measuring the product of government, is that no market prices exist, and hence current dollar figures cannot be used. The textbooks are careful to point out that the "capital consumption allowances" of the Department of Commerce do not coincide with the concept noted above. For example: "Over a longer period of time, the NNP would be a better representation of the economy's productivity, if accurate data could be obtained for depreciation and obsolescence allowances" [9, p. 282]. "In principle we must look at some current production of buildings, machinery, and so forth as simply the replacement of wear and tear on the outfit we use in production. . . . Unfortunately, however, our measures of capital consumption (the technical term for the using up of durable producers' goods and buildings) are very crude" [6, p. 424].

The emphasis on replacement, coupled with the incorrect notion that business depreciation accounting provides for replacement, allows no concern for the welfare implications of capital consumption allowances, which are worth more stress. Students come to economics convinced of the power of technological change in our land, and they find it reiterated throughout any contemporary introduction to economics. But the essence of technological change

in productive capacity is that it does away with replacement per se. Even when they continue to make the same product, firms do not purchase, at the end of a machine's lifetime, a machine identical to that which wore out. And the significant nature of productive capacity as a requisite for current income (or net national product) is that it provides the shifting and altering bundle of goods and services which consumers demand. Economists frequently suggest replacement cost as a basis for figuring aggregate capital consumption allowances, but this overlooks the basic fact that replacement rarely takes place. In fact, our present capital consumption allowances are imperfect not because depreciation allowances based on original cost do not match replacement costs, but because our concept demands that the entire capital structure, not just that which wears out in the current year, should be revalued each year in terms of its potential to satisfy current tastes and preferences. In 1958, \$28 billion, or 88 per cent of the total funds invested by corporations, was spent on plant and equipment. There are no data to divide these expenditures into replacement, expanded capacity, or wholly new forms of capital. Any attempt to do so requires an impossible definition—that of the word "new." But leading firms take pride in their sales of products which did not exist a decade ago, and the growth in productivity is attributed to technological change and new capital equipment.

Consequently, let us abolish the notion that depreciation has anything to do with replacement. From a welfare point of view, we should not want our capital replaced from year to year. Rather, we want it changed as our preferences change. From a productivity point of view, we want our capital improved, not replaced. And from the point of view of describing the accounting practices of individual firms, we are contradicting the express pronouncements of the accountants themselves.

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Measuring the Success of the Elementary Course

There is justifiable dissatisfaction with the state of economic understanding in this country, and therefore with the results achieved by the teaching of economics—especially at the elementary level. The work in the high school field of the Joint Council on Economic Education reflects this feeling, as does the search at various colleges for some new approach.

The purpose of this communication is to present the findings to date of a survey of the success of the elementary course. Identical or closely similar sets of true-and-false questions have been administered—when possible both before and after the course.¹ The experiment has revealed (1) an unsatisfactory rate of progress generally, and (2) particular fields in which students have learned little or nothing.

I. The Questions Used

Eight sets of 50 questions have been used, each containing 5 propositions on production, cost and business organization; 8 each on price, money and banking, income and employment, distribution of income, and international economics; and 5 on public finance. The sets come in pairs, each of the two treating the same topics though in slightly different wording.²

In preparing the questions, the attempt was made to apply the following principles: (1) to use ordinary language rather than technical terms, and to avoid both catch questions and self-evident propositions; (2) to make the statements analytical rather than factual—though there are some exceptions, dealing with topics like rates of growth, distribution of income, banking prac-

¹ The origin of the survey was in the 1953-55 "self-study" of New York University financed by the Carnegie Foundation. All faculty members were asked for suggestions as to how academic work could be evaluated; and this method occurred to the writer as an obvious way of doing it. In 1954-56 it was tried at more than twenty colleges, and in 1958-59 at several more. The survey did not draw on the Carnegie grant.

² A few of the questions selected in 1954 and 1955 were those of other writers, taken or adapted from teaching manuals. Other questions are undoubtedly almost identical with ones used elsewhere. The writer does not claim that, where they differ, his own are superior to those developed by others.

tice and tax structure; (3) to make most of the propositions important ones—including if necessary points not dealt with in textbooks whose solutions might require students to have recourse to their reasoning power; and (4) to make each proposition so clear and objective that any teacher would agree on the right answer, even if he might have preferred to treat some other aspect of the topic.

Regarding the last point, it was sometimes necessary, where qualified economists differ on an issue, to select propositions so extreme that all would agree on them. To use hypothetical examples, "Public debt is always bad" and "Public debt is never dangerous" are false, though "Economists see no reason why public debt should not be made the vehicle of economic expansion" would draw divergent answers, and therefore would be unusable.

The 50 questions are answered in 25 minutes, 100 questions—when used—in 50 minutes. Scoring is on the scale of 100, with 2 points for each correct answer and no penalty for wrong guesses. Since guessing would, on average, yield a score of 50, this is the equivalent of zero. It implies that a student has as many false as true notions about the questions being answered.³

II. General Results of the Test

Scores recorded on the six sets used through June 1959 are summarized in Table 1. The average of all grades before the course, for the different sets of questions, has been between 52 and 58; and the average after it, between 61 and 68. The widest spread among men's colleges on a single set was from 60.6 to 71.0; and among women's colleges, from 56.6 to 70.1 (both were Set E, after the course). The spread was usually much smaller: thus on Set A, for men, it was from 56.6 to 58.2 at the start and from 63.6 to 71.9 at the end.

Male students have begun with a slightly greater knowledge of the subject, and have usually maintained or improved that advantage. At 12 coeducational colleges, the unweighted arithmetic mean of the male advantage was 1.9 points before, and 2.7 points after the course.⁴

³ One 12-year old boy asked to take the test. Scoring 58 on Set A, he proceeded confidently to B, where his mark proved to be 42. It took 100 questions—but only 100—to reveal his true knowledge of economics! Though the score of a single student on 50 or even 100 questions may be misleading, the law of averages has emerged whenever 20 or 25 took the test.

⁴ Numbers of colleges and students taking the tests:

Test Sets	Beginning of Course				End of Full-Year Course			
	Men		Women		Men		Women	
	Colleges	Students	Colleges	Students	Colleges	Students	Colleges	Students
A	4	1,123	3	296	7	978	4	163
B	3	711	3	265	5	581	6	184
C	5	784	3	189	6	573	4	52
D	3	284	3	99	5	392	4	126
E	3	429	3	206	6	921	6	235
F	3	859	3	253	4	359	4	70

TABLE 1—AVERAGE OBJECTIVE TEST SCORES BEFORE AND AFTER
FULL-YEAR ELEMENTARY COURSE

Question Set	Time of Test	Arithmetic Means by Colleges		Arithmetic Means by Students	
		Male	Female	Male	Female
A	Before	57.3	54.8	56.9	54.2
	After	67.3	64.7	67.0	63.8
	Gain	10.0	9.9	10.1	9.6
B	Before	55.9	54.7	55.3	54.0
	After	65.5	62.5	64.7	61.9
	Gain	9.6	7.8	9.4	7.9
C	Before	57.6	54.3	57.5	54.3
	After	68.0	63.1	67.9	63.2
	Gain	10.4	8.8	10.4	8.9
D	Before	53.2	52.8	53.5	52.8
	After	61.7	60.8	61.4	61.0
	Gain	8.5	8.0	7.9	8.2
E	Before	55.9	53.8	56.4	54.7
	After	65.6	61.9	65.8	62.4
	Gain	9.7	8.1	9.4	7.7
F	Before	54.1	53.1	54.0	53.5
	After	62.2	61.3	62.1	61.0
	Gain	8.1	8.2	8.1	7.5

Table 2 analyzes the progress made in another way, by separate colleges. The best class score after the course, 71.9, was not truly a "C" average. The class was not 71.9 per cent of the way along from 0 to 100, but 43.8 per cent of the way (50 being the equivalent of zero). Since this particular class had begun with a score of 58.2, it had progressed in reality only 13.7, out of a potential of 41.8 points. At the start, it could answer 29 out of 50 questions; at the close, 36. At the other extreme, one class could answer 27 or 28 questions at the start, and only 29 at the end.

Several by-products of the experiment are:

1. More than 2,000 of the beginning students were asked to name their high school course whose content was closest to that of the course about to be taken. Those who had taken problems of democracy, social studies, civics, and American history did no better than average on the opening test. Students who had taken economics, however, did better—male students with this background averaging 57.6, as compared to 55.8 for others; while women students

TABLE 2—BEST, AVERAGE AND LEAST GAINS BY COLLEGES GIVING OBJECTIVE TESTS BEFORE AND AFTER COURSE

	College with Best Gain		College with Average Gain		College with Least Gain
	Less than 50 Students	100 Students or More	Median	Simple Arithmetic Mean	
Men (18 colleges)					
Before course	58.2	59.2	54.5	55.7	54.9
After course	71.9	71.3	64.0	65.0	60.4
Gain:					
Points	13.7	12.1	9.5	9.3	5.5
Per cent of potential gain (to 100)	32.8	29.7	20.9	21.0	12.2
Women (15 colleges)					
Before course	53.3	53.3	54.1	54.1	55.1
After course	66.4	59.9	62.7	62.1	58.6
Gain:					
Points	13.1	6.6	8.6	8.0	3.5
Per cent of potential gain (to 100)	28.1	14.1	18.7	17.4	7.8

averaged 55.2 and 53.9, respectively. The value of the high school course is evidently slight, at least by this measurement.

2. Northern institutions averaged 2 or 3 points higher than southern ones, both before and after the course.

3. Improvement in one-semester courses ran between 50 and 70 per cent of that in full-year courses at the same colleges.

4. Evening classes, consisting usually of mature students with jobs, had slightly higher scores before the course, but had lost a little of their advantage at the close.

III. An Example: Set A, Men

Set A is reproduced herewith, along with the average scores of the 1,123 male students taking the test before the course, and the 978 taking it after a full-year course. Teachers using these questions can thus know where their students stand as compared to the 978.

	Average Score (per cent)	
	Before	After
1. The increase in the education received by the average American is the most important single cause of the rising productivity of American workers since 1800. <i>F</i>	53	76
2. The lowest cost combination of the various kinds of labor and capital used to make a product will differ according to the price of each to the firm. <i>T</i>	70	84

3. There is an economic law which limits the amount of labor and capital a farmer can economically put into cultivation of his land.	<i>T</i>	62	85
4. Within each individual industry, the largest firms have a significant cost advantage over smaller ones.	<i>F</i>	14	36
5. A general rise in the stock market will tend to reduce the ability of productive industry to raise money from investors.	<i>F</i>	83	88
6. The free price system is a method of rationing based on ability to pay.	<i>T</i>	51	60
7. When a short wheat crop pushes bread prices up, most economists would endorse a subsidy to bakers if there were no other way to prevent the rise.	<i>F</i>	53	70
8. Sales of the common necessities of life tend to increase sharply when there is a drop in their price.	<i>F</i>	55	84
9. The price of a product is likely to be closer to its average unit cost of production over the long run than in the short run.	<i>T</i>	87	93
10. When a big movement of hogs to market causes pork prices to fall, this will tend to push beef prices down also.	<i>T</i>	61	79
11. The operations of a successful speculator in cotton generally tend to reduce the fluctuations in its price.	<i>T</i>	52	51
12. When a "gray market" (resale by distributors at more than list prices or to favored customers only) exists in a product, it is evidence that its producers are charging more than consumers can afford to pay.	<i>F</i>	65	79
13. Public regulation (assuming it to be politically feasible) is the only long-run economic remedy for a skyrocketing price like that of coffee or rubber at times.	<i>F</i>	49	71
14. The most important function of money is to serve as a store of value.	<i>F</i>	52	71
15. The fact that a country is on the gold standard ensures the stability of its domestic price level	<i>F</i>	76	88
16. When a country is on the bimetallic standard (both gold and silver being coined), a fall in the price of silver due to a rise in output of mines will cause gold to replace silver in monetary circulation.	<i>F</i>	55	62
17. When gold comes into a country, it is likely to mean that the banking system can expand its loans and investments to several times the amount of gold.	<i>T</i>	54	79
18. When business firms draw checks to repay their bank loans, it does not change the nation's bank reserves but it does increase the excess reserves of the banks.	<i>T</i>	69	66
19. An increase in the amount of paper money in circulation outside of banks tends to restrict the expansion of credit.	<i>T</i>	48	49
20. A reduction in legal reserve requirements of commercial banks tends to make it easier for business to borrow money.	<i>T</i>	72	86
21. Depreciation of a country's currency tends to lower the cost of living.	<i>F</i>	71	74
22. Inflation tends to enrich creditors at the expense of debtors.	<i>F</i>	50	69
23. The outstanding evil of peacetime inflation is that it reduces the real purchasing power of the national income.	<i>F</i>	14	18
24. Experience and logic indicate that it is hard for a modern democratic country to have full employment and stable prices at the same time.	<i>T</i>	52	62
25. When corporations draw on their working capital to pay higher dividends, it tends to increase the ratio of national consumption to income.	<i>T</i>	52	46
26. When unemployment is large, seemingly wasteful spending may be defensible because of its effect on employment.	<i>T</i>	67	81

27. Business cycles (booms and depressions) are as characteristic of barter economies as of money economies.	<i>F</i>	35	53
28. One reason business cycles are less severe than they might conceivably be is that a decline in construction causes an offsetting rise in other industries (by releasing men and goods to them).	<i>F</i>	51	66
29. A real difficulty in planning public works to combat depressions is that the expenditures may not be made at the best time from the point of view of those who need the new facilities.	<i>T</i>	79	75
30. In modern capitalist countries the gap between rich and poor is lessening.	<i>T</i>	74	86
31. If it were practical to equalize incomes, the increased incentive to those who now earn little would probably cause total production to expand.	<i>F</i>	74	82
32. Rapid population growth tends to improve the economic status of the average worker compared to that of the owner of capital.	<i>F</i>	81	88
33. Economic science offers no disproof of the claim that an employer who gives superior conditions of work has a right to pay lower wages.	<i>T</i>	47	53
34. An increase in population will tend to increase the rent of land.	<i>T</i>	88	91
35. Interest rates tend to be higher on short-term than on long-term loans to governments.	<i>F</i>	30	36
36. Accumulation of wealth by an economy tends to lower the rate of interest.	<i>T</i>	63	73
37. When national income declines to depression levels, profit as a percentage of income tends to rise.	<i>F</i>	59	67
38. An advantage when people buy goods produced at home instead of imports is that the country doesn't lose the money paid for the imports.	<i>F</i>	34	49
39. A domestic business boom is likely to cause a country's imports to increase faster than its exports.	<i>T</i>	34	36
40. A better economic defense can be made for protective tariffs levied by backward countries than for those levied by advanced countries.	<i>T</i>	54	71
41. American consumers are beneficiaries of our tariff (because it encourages production by the protected industries).	<i>F</i>	39	76
42. The aim of "exchange controls," by which a government allots available foreign currencies among its citizens, is to keep a country's rate of exchange from being controlled by artificial influences.	<i>F</i>	32	47
43. An increase in the world price of cotton is likely to raise the exchange rate of the dollar in terms of foreign currencies.	<i>T</i>	63	58
44. Use of foreign capital to develop a country's industries is demonstrably inferior and more costly in real terms than use of domestic capital.	<i>F</i>	36	51
45. A country which makes a loan abroad stimulates its own business activity thereby.	<i>T</i>	82	85
46. Expanding the public debt does not necessarily impose an over-all net economic burden on a country if the money is used for valuable public works.	<i>T</i>	73	81
47. Most experts in public finance hold that the wisest course for a nation with a large public debt is to pay it off without any delay.	<i>F</i>	79	93
48. An increase in personal income tax exemptions is more likely to stimulate spending by consumers than is a percentage reduction in income tax rates.	<i>T</i>	60	57
49. An excise tax on a commodity normally lays an economic burden on both its producers and its consumers.	<i>T</i>	79	67
50. A tax on the value of land tends to be passed on to the tenant in the long run rather than to rest on the landlord.	<i>F</i>	19	16

On 8 of the 50 questions in Set A, the course improved the average knowledge of male students by 49 per cent or more of the difference between the beginning mark and 100. A brief comment on each of these follows:

No. 1. 49 per cent of potential gain achieved. Good progress was evidently made in learning that productivity is related to capital investment, innovation and similar factors.

No. 3. 61 per cent. The law of diminishing returns was grasped at the end of the course by three-fifths of the students who had not sensed it at the start.

No. 8. 64 per cent. The difference between elastic and inelastic demand was well taught.

No. 15. 50 per cent. Three-quarters of the students already knew that the gold standard does not ensure price stability, and this was learned by half the remaining students.

No. 17. 54 per cent. More than half the students who had not already known it learned the fundamental principle of multiple bank expansion on additions to reserves.

No. 20. 50 per cent. More than 70 per cent of the students realized already that easing bank reserves facilitates lending, and half the rest learned this during the course.

No. 41. 61 per cent. Very good progress was made on the tariff questions of all sets, as on this question from Set A. On a "protariff" question, No. 40, progress was less (37 per cent) than on this "antitariff" question, No. 41. The antiprotection position of textbooks and teachers is known to all.

No. 47. 67 per cent. Almost 80 per cent of students thought at the start of the course that it would be foolish to try to pay off the public debt at once; and two-thirds of the rest learned this during the course. "Prodebt" propositions were well taught in the course, as appears in several of the question sets.⁵

It can hardly be said that there was real success on the other 42 questions, where progress toward the score of 100 was 47 per cent or less. On 8 of them, there was retrogression—though if more students had taken the test, it would probably appear that several decreases in scores were not statistically significant.

No. 11. Both at the beginning and the end of the course, guesses as to the effects of speculation went about fifty-fifty. Speculation would seem to be an important enough subject for students to be given an idea of its fundamental nature and consequences—though not of its technical details.

No. 18. It is perhaps surprising that on so complex a banking question two-thirds of the students should have known, sensed or guessed the answer at the start. The apparent decrease in knowledge during the course may prove not to be statistically significant, although perhaps students became confused by attempting to undertake a complex chain of reasoning.

No. 25. There was a substantial decline in realization that dividends have an impact on consumption different from that of building up working capi-

⁵ It could be argued that students had merely learned the instructor's views, both on this and the tariff question, and given back on the test what they thought he wanted. This might or might not be true as to the tariff, but the fact that 79 per cent of students took a "prodebt" attitude at the start on No. 47 supports the inference that most answers at the end were sincere.

tal. This could well be the result of overemphasis by teachers on the fact that higher incomes are saved more often than lower ones. It appears that the role of dividends in consumption, as well as their role in saving, should be referred to in the course.

No. 29. The decline in understanding on this question indicates that teachers may have oversold public works as a remedy for depression, with the result that a few students lost their original realization that they are not a panacea. A few extra words in class should clear up this confusion.

No. 43. This retrogression may have been a statistical accident, but the absence of improvement is significant. A student not knowing that rising world prices for an American export commodity tend to improve the dollar's position can hardly be said to understand the elements of foreign exchange.

No. 48. The decreased understanding on this question—which opens up an issue that has threatened to divide the national political parties—may be another statistical accident. But there was at least no gain!

No. 49. This rather sharp drop in realization that an excise tax is a burden on the producing industry may be a by-product of teacher emphasis on the impact of such taxes on the consumer. Perhaps some students decided that this meant that industry simply "passes all taxes on" with no loss to itself.

No. 50. The fact that students lost ground on the shifting of land taxes might be due in part to this same emphasis by teachers on tax shifting, except that most students (perhaps tenant-oriented in their thinking) believed from the start that land taxes are shifted. Until they learn the opposite, such a famous American philosophy as Henry George's must be a closed book. But teachers seem to have bypassed the difficult problem of explaining land-tax incidence.

On Set A as a whole, the average grade of 628 male students who had taken one-semester courses was 63.8, as compared to the grade of 67 for the 978 full-year students. On money and banking, and public finance, there was no significant difference in the scores of the two groups. On international economics, the full-year students gained 8 percentage points and on production, cost and business organization 11 points, more than the one-semester students. The largest single difference, 20 points, occurred in No. 4, on economies of scale, and No. 41, on the tariff.

Failure of the course at particular institutions to cover important areas thoroughly is also revealed by the tests. To cite one example, the several hundred students at one college averaged only 65 at the end of the full-year course on question 2 of Set A, whereas at all others the scores were between 80 and 95. Apparently the course at this college skimmed cost analysis (it ranked lowest on question 4 also).

IV. Purposes of the Survey and Possible Objections

1. The first aim has been to test whether the course is achieving generally satisfactory results. When classes score 55 at the start of the course and only 65 at the end, it is hard to boast. Possibly the advanced courses which emphasize institutions do better. One teacher reports having tried the same experiment in his 20-student course in marketing. The class averaged 36 right out

of 60 true-and-false questions on marketing institutions and policies the first day of the course, and 48 right, on the same questions, in the final examination. We need at least this much success in the more important elementary course.

2. The purpose of giving colleges a chance to compare results has been effectuated by circulating scores at intervals, with each school identified by a code number.

3. Individual topics reflecting the greatest, and particularly the least, success, are highlighted by the test. Also, testing before the course enables the teacher to spot some of the areas of special ignorance or prejudice which he will face.

4. Finally, it was hoped to develop 500 objective questions which a teacher could thereafter give, with any selection or in any combination he wished, in the knowledge of average scores elsewhere.

Secondary purposes might be served by a more extended survey. Are scores correlated with size of classes, or with textbook? On what topics do women students do worse or better than men? Has reorganization of the course at a particular college improved results?

These questions, and this whole method of testing, are subject to various possible objections. Some of these objections and possible answers to them are as follows:

1. Will the brightest students be the ones to think of the qualifications to a simple proposition, and hence be more confused than others as to whether to call it true or false? To test this objection, the scores of the 182 whose grades on Set A had been 76 or above, out of the first 1,136 male students to take the set (after either the full or half-year course), were compared with the averages for all 1,136. On every question the 182 averaged significantly better than the rest. The closest the two groups came was on Nos. 49 and 50, for which the 182 made 86 and 88 per cent as many errors per man, respectively, as the other 954 students. On average, they made only 59 per cent as many errors.⁶

2. Is reasoning power tested in true-and-false questions, or do the students merely guess? The answer is that the individual student may often guess, but that it is statistically impossible for every class to have improved its average grade, as has been the case, without an increase in understanding.

In one class, the students were given only 25 questions and were asked to state in a single sentence the reason for each answer. A good reason for a correct answer received 3 points' credit, a weak reason one point, and an illogical reason nothing. Between the beginning and the end of the course, this class increased its average score by 9.3 points; while the computed average validity of the reasons on correct answers improved by 8.6 points. Both on answers and on reasons, the class covered 18 to 19 per cent of the distance from its starting point to 100.

3. Some teachers are opposed to the use of objective questions. It can be admitted that a well-chosen, well-corrected essay question will show more of the

⁶ Those who might doubt that the students scoring highest on these tests are the "brightest" are asked to note the correlation with other marks, reported later in this communication.

student's abilities. The essay question, however, (a) sometimes goes wrong when the student writes so badly that it obscures his real understanding; (b) may call for more of the grader's time, in deciphering handwriting and comparing the different essays, than he may be able to spare; (c) cannot cover as many parts of the course in 50 minutes as 100 true-and-false questions can; and (d) does not permit accurate comparisons of attainments between different colleges, or the same college in different years. Thus the objective question has definite functions in supplementing the essay.

In so far as teachers supplied course grades for their students, a correlation was revealed between these and the test grades. In the largest group for which marks were supplied, 292 students took the test, with the following results, in part:

RELATION BETWEEN TEST SCORES AND FINAL EXAMINATION GRADES

Test Score	Average of Same Students on Final Examination in Course	Range of Scores on Final Examination	Number of Students
80	78.2	86-71	6
78	77.6	85-72	7
76	77.2	84-62	18
74	75.2	85-69	19
72	70.2	80-60	28
70	68.9	81-59	29

Where grades on the final examination just mentioned, or on essay questions generally, differ sharply from those on the objective tests, the latter can supplement the former. Among the 18 students who received objective test grades of 76, one scored 84 and another 62 on the examination, but they may be closer in attainment than these B and D grades would imply.

4. Some have doubted that particular questions on which average class scores after the course were very low (e.g., below 40) should be continued in use. It seems to them unfair to grade students on propositions which were not learned anywhere. However, on most of the questions with lowest scores the average improved during the course—as on Nos. 4, 23, 35 and 39 of Set A, though No. 50 is an exception. This suggests that the propositions should not be excluded, but that their difficulty calls for more attention to them by teachers.

5. It must be conceded that not all the questions in these sets are covered in all elementary courses, and not all could be squeezed into every course under any conditions. For three reasons, this weakness was not considered fatal. First, students should be able to answer certain questions by their reasoning processes as developed in the course, even though the particular point has not been covered. Second, low scores at these points may serve to highlight gaps in the course—in this respect the questions test whether coverage of the course is adequate rather than how well the topics it actually covers are taught. Finally, though no student could possibly score 100 on a test on which he has only half a minute to answer each question and which contains

some not covered in his textbook or lectures, this does not make the broad comparisons invalid. A teacher whose course omits a number of the topics can remind himself that courses elsewhere are also incomplete.

6. The writer has been told that some of his propositions are "opinion questions," answers to which will depend on the student's prejudices, and even that his own answers to a few are mistaken. Each reader must decide for himself how far these criticisms are justified. A teacher who would like to reject a few propositions out of 50 can remind himself that classes elsewhere are being marked on the same basis as his own.

It will be appreciated if any teachers who use Set A, reproduced above, will send full results to the writer—clearly distinguishing type and stage of course, number and sex of the students, and score on each question. The other sets will be sent to anyone making inquiry, along with answer keys (presumably not needed). A full tabulation of results will be circulated among participants.

Suggestions will also be welcomed on topics that should be included in extending the list from 400 to 500 questions.⁷

SIMON N. WHITNEY*

⁷ To illustrate, there are suggestions now on hand to include another pair of questions on the relation of price and cost and a pair on customs unions.

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BOOK REVIEWS

General Economics; Methodology

Jan Tinbergen Selected Papers. Edited by L. H. KLAASSEN, L. M. KOYCK, H. J. WITTEVEEN. Amsterdam: New Holland Publishing Co., 1959. Pp. xii, 318. \$8.00.

This is a very well chosen selection of eleven papers by the well-known Dutch economist Jan Tinbergen. The selection covers five general fields:

1. *Business cycle theory and policy.* Here we have the well-known model of the shipbuilding cycle (published in German in 1931), an essay on types of equilibrium and business cycle movements (published in Norwegian in 1944), "An Economic Policy for 1936" (paper given originally in Dutch), "Lag Cycles and Life Cycles" (published in Dutch in 1938), "Tonnage and Freight" (published in Dutch in 1934).

These essays together with the two volumes published in 1939 by the League of Nations (*Statistical Testing of Business Cycle Theories*) certainly establish Tinbergen as an outstanding contributor in this field. Deep theoretical insight based upon statistical verifications is combined with a passionate interest in and concern for questions of economic policy. Are there for instance now many economists who would dare to publish their proposals for an economic policy for 1936? Tinbergen, because of his deeper theoretical insight and admirable statistical skill has been more fortunate in his predictions and proposals for economic policy than most of his econometric colleagues in other countries. The judicious combination of mathematical theoretical investigations with empirical statistical treatment of concrete data is characteristic of Tinbergen's business cycle theories.

2. *International economics.* This section contains the following essays: "The Equalization of Factor Prices between Free Trade Areas," a critical discussion and amplification of the essay published by Samuelson in the *Economic Journal*, June 1948; "Long-term Foreign Trade Elasticities," published in English in 1949; "On the Theory of Economic Integration" (published in English in 1952); "Customs Unions: Influence of Their Size and Their Effect" (published in English in 1957).

It is not surprising that a citizen of Holland, a country whose historical importance and very life are closely connected with international trade, should have been attracted to the economic problems connected with this field. Apart from his pioneering studies of elasticities, his investigation of the consequences of economic integration and customs unions are most important. The welfare concepts used in these investigations are total production and real income. A number of very interesting results are established which are of great consequence for international trade policy. It is, for instance, shown under somewhat simplified conditions that economic integration may under certain circumstances, lead to a fall of real income in the countries concerned.

3. *Long-term economic development.* Here we have the following essays: "A Simplified Model of the Causation of Technological Unemployment" (published in English in 1939); "On the Theory of Trend Movements" (published in German in 1942). The study of technological unemployment uses U. S. data from 1910 and 1919-1932. The effects of mechanization, rationalization and new combinations are explored. In view of the recent tendency to automatization a continuation of these studies with the help of more recent data would be very desirable. The study on trend movements uses data from Germany, Great Britain, France, and the United States for 1870-1914. The mathematical model is influenced by ideas of Keynes. It is shown that even very simple models yield exceedingly complicated mathematical formulae for trend movements.

4. *Distribution of income.* This section contains the following essays: "The Influence of Productivity on Economic Welfare"; "On the Theory of Income Distribution" (published in English in 1956). In the paper on productivity Tinbergen uses his technique of linearized macroeconomic models to investigate various goals of economic policy ("targets": social equilibrium, balance of payments, employment), which are being pursued by the application of "instruments" (wages, taxes, profit margins). Increase of productivity not always leads to desirable results. The paper on income distribution is purely theoretical. The log-normal distribution of incomes is explained in terms of the demand and supply of productive contributions. The possibility of collecting data in order to verify the ideas statistically is also indicated.

5. *Economic systems.* One essay: "The theory of the optimum regime." This essay, which deals with the most important of all current problems of economic policy, is perhaps the most interesting contribution in this book. Here Tinbergen courageously attacks the crucial problem of our time. A simple welfare function is used, a sum of all individual utility functions involving consumption and effort. This is somewhat reminiscent of Pigou's economics of welfare. A Pareto optimum implies the liberalistic decentralization thesis, but it is pointed out that this proposition may involve lump-income transfers, because the individual optima may not be otherwise attainable. The discussion of external effects and increasing returns proceeds along familiar lines. It is recognized that the decision between free capitalism and collectivism is ultimately based upon ethics; but the author (whose own position is perhaps nearest to Fabian socialism) believes that, nevertheless, discussion is possible and desirable. He sees actually a narrowing of the gap between the systems of the East and the West. This view points in the same direction as recent articles by Oskar Lange.

There is also a comprehensive bibliography of more than 150 items, mostly in Dutch. This splendid collection of essays can be recommended highly to all economists who do not shy away from a little mathematics. It is difficult to imagine any member of the profession who would not profit from a careful study of some or all of these articles.

GERHARD TINTNER

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Economics for Pleasure. By G. L. S. SHACKLE. New York and Cambridge: Cambridge University Press, 1959. Pp. x, 269. \$3.95.

Shackle's reasons for writing this book are made quite clear and may be stated in the following propositions: (1) It is "useful and indeed essential . . . for the leaders of modern life to have some knowledge of economic theory" (p. ix). (2) Noneconomists generally regard economics "as dismal if no longer a science" (p. 1). (3) The unattractiveness of economics to the nonspecialist stems from the economist's propensity to beclutter his work with mathematical symbols—"But how can a subject be readable that requires the brain-teasing study of diagrams and equations?" (p. ix). (4) "The problem, then," according to the dust jacket, "is to bridge the gulf between the economist on one side and the businessman, the banker, the politician, the journalist, the student and the ordinary man on the other. The solution lies in this book—for here, without any mathematics, without any diagrams, and in good plain English, the essence of economics is laid bare."

If Shackle had written a nonmathematical introduction to economics by simply leaving out those concepts which are usually expressed as an equation or a geometric chart, his work would hardly be unique. But he chose the much more difficult task of including all of the ideas usually presented in an elementary economics course; and he expresses these ideas verbally and without the assistance of any mathematical paraphernalia except for an occasional numerical table. In less skillful hands this effort might have resulted in a fiasco, and most economists are likely to feel that Shackle's book is less than completely successful.

One is impressed as he reads this book with the number of economic concepts which are essentially mathematical in character whether they are expressed verbally or in symbols. If such a basic idea as demand is not presented as a functional relationship, it becomes virtually impossible to make the distinction, carefully noted by all teachers of elementary economics, between an increase in demand and an increase in the quantity demanded. Shackle doesn't attempt the distinction—" . . . if at the going price supply exceeded demand a lower price would cause a curtailment of supply and an increase of demand" (p. 23). In discussing elasticity of demand the author is scrupulously careful not to include any fractions, but he does talk about them: "For elasticity is just a ratio, and we can write it like a vulgar fraction with a numerator (the percentage change of quantity) and a denominator (the percentage change of price). When we divide the 'upstairs' part of the fraction (its numerator) by the 'downstairs' part (its denominator), and find that the latter goes exactly once into the former, it is natural to give this vulgar fraction the value of *one*, and say the elasticity it represents is *one* or *unity*."

In at least one place the author attempts a nonmathematical explanation of a rather sophisticated mathematical concept. He addresses himself to the question as to whether the firm's production function is "linear and homogeneous" (p. 119). With remarkable clarity he points out that within the range where the firm will actually operate, *i.e.*, where average costs are at a minimum, " . . . the *total* cost is increasing in precisely the same percentage as the total output" (p. 120). He concludes, "In short, the production function *can*

be of a character which, at the relevant output, gives constant returns to scale; and yet at the same time it can be true, because of some *essentially* 'indivisible' factor of production, that output can neither be greatly smaller nor greatly larger without a rise of average cost" (p. 121). But how clear this would be to a reader who had not cut his eye teeth on cost curves is difficult to guess.

Some of the forty chapters in this book are excellent examples of expository writing. Chapter 37 on the Balance of Payments is one of the best. Chapter 13 on "Circulation," however, is likely to convince the tired businessman that economics, even without mathematics, remains dismal. It is not likely that this book will become a best seller among the noneconomists for whom it was written. Teachers of elementary economics might, however, read it with profit. And it might prove an excellent review for students who have finished the elementary course in economics. But the principal moral to be drawn from this book is, surprisingly enough, that the serious student of economics should include in his program of study a generous sprinkling of courses in mathematics.

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Economic Plan and Action—Recent American Developments. By CHARLTON OGBURN. New York: Harper & Brothers, 1959. Pp. xiii, 287. \$4.75.

This volume is essentially a résumé of, and commentary on, the major economic studies, reports and policy statements of the National Planning Association since 1943. The NPA materials drawn upon were concerned with some of the major economic problems and developments affecting the United States during this period, beginning with the problem of postwar full employment as envisaged during the second world war. As a result, the book deals with subjects to which economists have given a great deal of thought in recent years. The author was well prepared for his task inasmuch as he served as counsel for the NPA for fifteen years and has been a member of its Board of Trustees since 1948.

The National Planning Association is one of several privately sponsored, publicly oriented groups in the United States which have addressed themselves to important economic problems in recent years, and which have as a result of their carefully prepared and considered studies and reports on these problems contributed to better understanding and more intelligent action. The NPA studies have had wide acceptance because of the representative character of the organization's membership, the quality of its leadership, the substantial competence of its staff and outside experts who produced the studies, and the ably manned committees which gave guidance and direction to them.

The NPA studies are treated by Ogburn under the following chapter headings: (1) Full Employment Act [of 1946]; (2) Export of United States Capital—I. Dollar Gap, Private United States Investments in Foreign Countries; (3) Export of United States Capital—II. Foreign Economic Aid; (4) Export of United States Capital—III. Technical Assistance; (5) Farm Commodities Surplus; (6) Federal Budget; (7) United States Business Performance Abroad—I. The Firestone Operations in Liberia, The Standard Vacuum Oil Company Operations in Indonesia, The Casa Grace Operations in Peru;

(8) United States Business Performance Abroad—II. Creole Petroleum Corporation in Venezuela, The Philippines and the Philippine American Life Insurance Company, Sears Roebuck De Mexico, S.A., The United Fruit Company in Latin America; (9) Organized Labor; (10) Collective Bargaining; (11) The Changing Economy of the American People; (12) Productive Uses of Nuclear Energy; (13) Foreign Economic Policy; (14) Dispersal of Industry; (15) Education for Economic Development [not based on NPA studies]; (16) The Common Defense; and (17) Social Effects of Economic Development.

Among the most valuable of NPA's studies, in this reviewer's opinion, have been those dealing with the performance of American companies in less-developed countries. As the author states: "It was believed by the officials of the NPA that in making intensive studies locally of the policies and methods of a few industrial and commercial corporations, they could discover patterns which would prove valuable guides to other United States companies abroad and also to private investors; and that these studies conducted by well-known economists and sociologists would prove useful guides to the United States government, to prospective underdeveloped countries where United States investors could locate their enterprises, and also to international agencies" (pp. 81-82). Chapters 7 and 8, which deal with these studies, are among the most interesting and well written in the book. Chapter 11, summarizing the study on "The Economy of the American People" by Colm and Geiger, is also a very useful and interesting contribution.

A book of this type cannot in the nature of the case develop each of its topics in an exhaustive, scholarly and systematic manner. Sometimes, for example, one finds statements and conclusions rather than the development of ideas and theses. The book is a digest of a number of competent and useful studies with commentary, and in some cases updating by the author, rather than a systematic treatment of a given subject, or of a series of closely related subjects. The result is abbreviated treatment of a number of economic problems, not wholly integrated in presentation. Despite its title and subtitle, the book is not an organized history of recent American economic developments or policies.

Notwithstanding the limitations inherent in a work of this character, *Economic Plan and Action* will prove to be useful to the many readers who are interested in the subjects treated, but who have not had an opportunity to cover the general literature in the field, including the important NPA studies which are the basis of the book.

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Matematika dlia ekonomistov. (Mathematics for Economists.) By A. IA. BOIARSKI. Moscow: Gosudarstvennoie Statisticheskoe Izdatel'stvo, 1957. Pp. 367. Rbl. 8.25.

This book, by a noted Soviet statistician, is the first of its kind to be published in the USSR since the 'twenties. Its first part introduces the reader to the logic of functional relationships and to the elements of differential and

integral calculus, through partial derivatives and Simpson's rule. The remainder covers selected topics of mathematical statistics, including a well-written chapter on probability, organized around the discussion of Tchebyshev's inequality. Mathematical techniques are, as a rule, explained clearly and concisely and some illustrative examples of their applications are provided. The student's task is, however, made needlessly difficult by a complete absence of exercises.

Boiarski's pioneering effort has to be appraised against the background of the climate of opinion that until lately prevailed in Soviet economics. After a brilliant start in the 'twenties, associated with names like Slutski, Feldman, Kondratiev, mathematical economics came into disrepute. After about 1930, the use of calculus by Soviet economists was condemned as a worthless and sterile pursuit, allegedly inseparable from the two *bêtes noires* of Marxian economics: diminishing marginal returns and marginal utility. Gradually, even the use of high-school algebra came to be frowned upon. The authority of Karl Marx has often been invoked in support of this antimathematical, "qualitative" approach, without much justification. Marx held in fact that "a science reaches perfection only when it starts applying mathematical methods."¹ And the author of *Das Kapital* certainly viewed economics as a science.

Boiarski's second major topic, mathematical statistics, fared for a long time little better than calculus among Soviet economists. Its applicability to problems of a planned economy was questioned and its imminent "withering away" predicted. This view was discussed in the widely influential "Theory of Mathematical Statistics"² of which Boiarski was co-author.

Hostility breeds ignorance and authoritative spokesmen have recently complained about inadequate knowledge of mathematics among Soviet economists. Meanwhile, as the USSR's economy grew in size and complexity, the need arose for more sophisticated methods of planning and analysis than the mere 'rithmetic *cum* intuition (cf. p. 4). Boiarski's text fills to some extent an important gap in the Soviet economists' education and its appearance may be viewed as part of the current drive toward greater rationality in Soviet planning.

The rehabilitation of some of the previously outlawed mathematical techniques is undoubtedly a sign of progress. But, from there to a rehabilitation of mathematical economics, or to finding a common language with post-Cournot Western theory, there is still a long and arduous way.

The author sets himself a task that is severely limited by the still prevailing taboos. He provides the three hundred formulas, but he does so without recognizing the relationship of his analysis to Western economists' ideas or their tool-boxes. To give some examples: Functional relationships are discussed on some eighty pages without once mentioning the concept of a production or cost function. Economic applications of differential calculus, such as the concept of marginal cost, the measurement of elasticity of demand or of substitu-

¹ Related by Marx's close collaborator, Paul Lafargue, in: Institut Marksa-Engelsa-Lenina-Stalina, ed., *Vospominania o Markse i Engelse* (Recollections about Marx and Engels), Moscow 1956, p. 66.

² B. S. Iastremski and A. Ia. Boiarski, *Teoria matematicheskoi statistiki*, Moscow 1930.

tion are passed in silence. As a result, Boiarski's mathematics loses much of its economic content. The student is trained in measurement not only without theory but often without tools, and may be sorely tempted to re-invent them for himself. The temptation may become well-nigh irresistible for a student who has dabbled in the practice of economic planning: he has, one suspects, been substituting on the margin, adjusting to dimly perceived elasticities, wrestling with complementarities. . . . Why should Boiarski spare him the thrill experienced once by Molière's hero on learning that he has, throughout his life, talked in prose?

Apart from any such "Jourdain effect," by refilling their tool-boxes and labeling the tools, Boiarski would help his colleagues in their current efforts at assimilating some econometric techniques developed in the West. That the need for such help is urgent, was recognized by the Soviet editors of the 1958 Russian translation of Leontief's *Studies in the Structure of the American Economy*. They state, with some melancholy, that the lack of contact between Soviet and Western economists and "the gradual abandonment of classical international terminology" by the latter has led to a situation where "it has finally become difficult to understand each other." They then proceed to explain to the Soviet reader of Leontief's book the concept of "the so-called production function."³

While mathematics is a language, each science tends to develop a specialized vocabulary of its own. The book under review performs a signal service by introducing the Soviet student, as it were, to the grammar of that language. Let us hope, it will be followed by a conversation textbook.

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The Income of Nations and Persons: An Introduction to Economics. By ALVIN E. COONS. Chicago: Rand McNally and Co., 1959. Pp. xvi, 672. \$6.75.

With the publication of this book another text of formidable size has been added to those available for use in introducing students to economics. The subject matter has been divided into six major parts: Part I is an introduction to economics, its scope, limits, and some basic concepts; Part II concerns national income and is essentially an introduction to macroeconomics; Part III is primarily devoted to price and output and is essentially an introduction to microeconomics; Part IV covers distribution theory or the allocation of income; Part V deals with consumer behavior and the role of consumption; and Part VI covers international trade.

Each of the six major subdivisions is in turn broken down into three subsections. The first subsection serves as an introduction of the subject, the second develops the economic analysis appropriate to the particular area, and the third describes three normative systems or alternatives under the headings "Laissez Faire," "Planned or Central Direction," and "Pragmatism."

³ W. Leontief, *et al.*, *Issledovania struktury amerikanskoi ekonomiki*, Moscow 1958, pp. 5-6 and 4.

The student is constantly reminded that the central subject of economics is the creation and use of real income and that there are various alternative economic systems that can control the creation and distribution of this income. Economic theory is treated as a "thinking tool," a convenient frame of reference for economic analysis; and considerable attention is paid to the institutional factors which influence economic relationships in the "real" world. The author has presented a wide panoramic view of economics by weaving together theory, institutions, economic doctrine and history. Only slight use is made of charts, graphs and mathematical formulas. Basically this text is a descriptive one, and the student is not taken very far into the intricacies of economic analysis.

The literary style is clear and straightforward; most of the major subdivisions are introduced with great simplicity. By covering a wide area on a simple level, this book should appeal to those who feel the introductory course in economics should present the subject with a minimum of analytical detail.

The imperfections or shortcomings that may occur to others as well as to this reviewer concern first the organization of the book. Subjects such as money and banking, public finance, demand, and consumer behavior are scattered among several of the major subdivisions. Logical analysis is sacrificed for the simplest description of economic problem areas. Indifference curves, for example, are presented in a few brief paragraphs, in the chapter devoted to consumer behavior, along with the substitution and income effects of price changes. In an earlier chapter devoted to product markets the demand schedule and demand elasticity are described with the assistance of the marginal utility concept. The advantage of this approach is that a subject is presented quickly and simply, but the disadvantage is that the more complicated analyses are not given sufficient attention to make them fully understandable.

This book must also share the criticism earned by most elementary texts on economics. Too much is included. For the typical undergraduate student, the sheer bulk and variety of the contents, ranging from sprinkled references to major economists and their works to historical events, current institutions, philosophy, abstract models, and comparative systems, leave no area where concentration can be focused. This reviewer doubts whether economics can ever be meaningful or whether students can retain any lasting understanding of economic reasoning when given this broad aerial view of the economic terrain.

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The Economic System. By JOHN M. KUHLMAN and GORDON S. SKINNER. Homewood: Richard D. Irwin, 1959. Pp. xii, 509. \$6.50.

This text is intended for those who take one course in economics, presumably a semester in length. If it is used for a two-semester course, additional readings are recommended. The organization of the text is as follows: (1) methodological introduction and definitions; (2) the structure of the economy and the market; (3) labor; (4) the other factors of production; (5) national income and employment; (6) money and banking; (7) the role of government

vis-à-vis fiscal policy and the control of business; and (8) the United States in the world economy, with gestures towards what is generally called comparative economic systems.

The initial discussion of methodological problems is standard and for the most part in the neoclassical tradition with emphasis on the role of economic analysis in the dispassionate solution of economic controversy. The reviewer believes, however, that methodological excursions at the beginning of a study are not likely to be useful, and this is true of collections of definitions of concepts. All too often they do not find reference in the subsequent material. It is more useful to develop them as needed. Incidentally, although most readers are reconciled to finding in elementary texts the factors of production defined as land, labor, capital and "the entrepreneur," the appearance of form, place, possession, time and service utility brings nostalgic memories of elementary courses dominated by F. W. Taussig and Fairchild, Furniss, and Buck in the present age of Bachs and Samuelsons.

The discussion of the structure of the American economy is useful and informative for the student, but the treatment of demand and cost is much too sketchy even within the page limitations: the determination of the *level* of demand, production, the scale problem and innovations are only alluded to; and the discussion of market structure is so limited as to be primarily definitional. The empirical discussion of market behavior as an application of the theoretical analysis is more successful.

The labor section is long (71 pages), competent, and thoroughly in the dominant contemporary tradition of labor economics *cum* industrial relations: ("... the trend does seem to be towards a more mature [sic] and co-operative approach on the part of both management and labor" [p. 173]; but "... one who is 'pro-labor' or 'pro-management' as the result of emotion can make little contribution to successful public policy" [p. 223]).

The next section on the returns to the other factors of production, which begins with a discussion of capital and interest, is well organized. The loanable funds approach, which the reviewer has always found to be a useful taxonomic device for undergraduate instruction, is used with some imagination. The discussion of financial institutions is appropriately found at this point. Unfortunately too many elementary texts fail to do justice to this important topic. The discussion of rent and profits is traditional and to a considerable extent definitional; i.e., there is no particular attempt to integrate these matters with any broader treatment of the economic process.

The chapters devoted to aggregate analysis begin with an introduction to national income analysis from the point of view of the accounts and then continues with a brief statement of $C + I + G = Y$, from which the S, I relationships are derived. The introduction of interest rate considerations along with an implicit discussion of the investment schedule completes a bare-bones development of the employment model. The subsequent chapter is devoted to an analysis of money and the operation of the banking system. There is no attempt to coordinate in any detail the money with the real level of analysis.

The next group of chapters, devoted to fiscal policy and the control of business enterprise, raises many of the important issues but settles none even ten-

tatively. ("It appears quite probable that neither of these extreme viewpoints regarding the debt is accurate" [p. 364]. "Although it is improbable that the clock shall ever be turned back, it does not follow that we need look forward to a continually larger and larger area of governmental operations" [p. 428].) The authors usually take a position somewhere between two opposing positions. But most of the topics are mentioned and the descriptive material is interesting and well presented. The student will develop an awareness of the extent and complexity of governmental activities at every level, and there is a real and to some extent successful attempt to exorcise a number of the conservative hobgoblins.

The final section on America and the world economy follows the pattern established in the preceding chapters. The relevant institutional material is presented in a competent fashion; the important issues are least raised and, as in every case, moderate positions are taken.

The text leaves an over-all impression of moderateness in all things, a useful taxonomic approach to the usually defined topics in elementary economics, with no particular attempt to integrate the various issues discussed, a much better than average style, and a presentation that will make the subject matter available to the reasonably diligent student in the average state university. It leaves the instructor with all the room desired to develop his own emphasis.

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Price and Allocation Theory; Income and Employment Theory; Related Empirical Studies; History of Economic Thought

Interest—An Historical and Analytical Study in Economics and Modern Ethics. By THOMAS F. DIVINE. Milwaukee: Marquette University Press, 1959. Pp. xvi, 254. \$7.00.

This volume is oriented to the ethics of interest on the basis of Biblical and Christian teaching. Part I (116 pages) reviews the moral teaching with respect to interest-taking as found in the Bible, in Greek and Roman thought, and in the teaching of the Catholic and Protestant Church to the present day. Part II (70 pages) presents a strictly economic analysis within the traditional framework of interest theory. In Part III (60 pages) the author relates the fundamental ethical principles of Part I to the nature of interest as developed in Part II and thus formulates his own conclusions regarding the morality of interest payments.

Father Divine brings to his study both the sympathetic understanding of a Catholic Father and the critical objectivity of a scholar. Value judgments and analysis are sharply distinguished throughout. The economic analysis of interest theory reviews familiar models in order to draw out their implications for discussion of the ethics of interest-taking. The major contribution of the book is the careful review of religious teaching provided in Part I and the thought-provoking application of this teaching to the problem of interest in Part III.

The author's basic ethical presumption is that two questions must be an-

swered favorably before the taking of interest may be judged morally acceptable. One of these questions deals with commutative justice, the other with social justice. Commutative justice concerns the relation between one individual and another; it requires that in every exchange, value given up must equal value received. The principal of the "just price" is here suggested and the problem posed is how to determine the true "value" of goods. The second aspect of justice, social justice, concerns relations between individuals and society. This includes distributive justice and a wide variety of other aspects of economic relation such as the level of employment and economic growth. If interest payments violate commutative justice, they are inherently wrong and should be proscribed forthwith. If they do not violate this standard, they are to be judged in each instance by weighing their various and often conflicting effects upon the achievement of social justice.

The Biblical objection to interest is a qualified one. In some texts it applies only to loans to the poor, or to fellow Jews. At some points objection appears to be to any act that is oppressive or ungenerous, with applicability to the exaction of interest only when it falls within that category. Similarly in the writings of the pre-Scholastic fathers of the Church "we find only reiterations of the Scriptural precepts that it is contrary to charity and mercy to exact usury of the poor, without any intimation that these precepts imply a universal prohibition" (pp. 27, 31).

Under the Scholastics, analysis changed from social to commutative justice: "the ethical consideration was shifted from consideration of the motive of the lender and the social consequences of the loan to that of the intrinsic nature of lending and borrowing" (p. 41). Aquinas' attitude was based on his general proposition that a charge could legitimately be made for the loan of durable goods but not for perishables. The logic was that if one borrows a durable good and later returns it, he will have received the services of the good in the meanwhile and may justly be asked to make appropriate payment. However, perishables offer no service beyond their consumption, and when similar goods are returned to the lender, this repayment is of equal value with the loan, as it should be. Aquinas then argued that money is a perishable good since its function is to serve as a medium of exchange, with the result that it is "consumed" when it is used. A charge for the loan of money is therefore illicit. The "just price" for the loan of a barren good is zero.

Father Divine accepts what he regards as the fundamental position *underlying* the policies of Aquinas, that any charge beyond a just price violates commutative justice and is inherently wrong. But he, like the Church today, uses these principles to support a contrary policy, namely, that interest for the loan of money is legitimate if the rate is not excessive. His basic problem is to find the determinant of a "just price" for borrowed funds, which he attempts to do in Part II by seeking the determinants of the market rate of interest in a competitive society. He begins with a rather detailed explanation of Fisher's theory of interest, after which he admits to his analysis the Keynesian consideration of demand for money for liquidity.

The Fisherian analysis serves at least two purposes for Father Divine. In the first place its emphasis on time preference justifies the conclusion that the

utility of a good today and that of a promise to the same good a year hence are no more equal than the utility of two different goods today. Thus a difference in price for a claim due today and one due tomorrow is quite as legitimate as that for any other exchange. The only issue is the amount of the charge. At this point, too, the theory of interest developed in Part II serves the author's objective: where there is a free and competitive market for loanable funds, competitive price will be determined. And just as the author argues that competitive price is consistent with commutative justice in the exchange of current goods, so he argues it is appropriate in the exchange of differently dated claims.

Having demonstrated his view that the payment of interest is not inherently unethical, he then examines the implications of this payment for social justice. Here the good and evil effects are weighed to determine the context in which interest payments can benefit society. Effects on distributive justice lead him to urge policies that result in more equal distribution of property. A charge for loans at competitively determined rates (modified by monetary and fiscal policy) would then seem to him to be morally justified.

Partly because the basic text was written in 1938, there are a number of ways in which both the interest theory of Part II and the discussion of ethics in Part III are not entirely satisfactory. For example, there are a number of places where confusion results from failure to distinguish between hoards and changes in hoards (pp. 170 ff), and also from the intermittent use of the liquidity preference schedule as referring to the demand for all money balances and to the demand for speculative balances. The introduction of new money to the system and of dishoarding are both referred to as "inflationary," which may contribute to the false impression that such events must cause price increases (pp. 173, 180). The discussion of Keynesian analysis does not do justice to Keynes' emphasis upon the interdependence of his system, but follows instead the narrower interpretation of his exposition (e.g., p. 184). Real balances are not discussed, with the result that unduly restricted conclusions are sometimes stated. In his preceding discussion of nonmonetary interest theory (pp. 152-66) there are a number of places where the author is either unclear or partly wrong. In his comments on Böhm-Bawerk's theory reference is made only to technical superiority (p. 163). It is later stated that the existence of interest may be explained either by time preference or by investment opportunity (p. 167), whereas the dispute between Fisher and Böhm-Bawerk over this point would seem to have been resolved in favor of the view that investment opportunity alone cannot suffice. But none of these errors lead to mistaken conclusions about the nature of interest for which the author uses his analysis.

The ethical standards underlying the study are limited to religious teaching. They do not take advantage of recent studies in welfare economics, or of the extensive associated philosophical thought on the ethics of income distribution. The statement that "there are very few who would not agree with the principle that personal services are justly rewarded in proportion to the value of the contribution of those services" (p. 227) raises a number of interesting questions. There are difficult ethical problems in recommending that those born

with the good fortune of great abilities must, *for reasons of justice*, receive financial rewards in proportion to these abilities.

While these comments suggest that other ethical standards could have been examined, this does not detract from the value of the book as a thoughtful, careful and scholarly achievement of its intended aim: the application of a long line of Church teaching to the problem of the ethics of interest in the light of an essentially valid analysis of the nature of interest.

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A Time Series Analysis of Interindustry Demands. By KENNETH J. ARROW and MARVIN HOFFENBERG, with the assistance of HARRY MARKOWITZ and RONALD SHEPHARD. Amsterdam: North-Holland Publishing Co., 1959. Pp. viii, 292. \$7.25.

Interindustry flows of goods and services are one part—albeit an important part—of the general circular flow in the economic system. Although the circular flow has been an object of considerable interest to economists since the time of the physiocrats, little empirical content was given to the interindustry aspects until Leontief's pioneering work on input-output analysis. Essentially, input-output analysis is a simple, but empirically feasible, way of relating the demands for goods and services in their final uses to intermediate demands and demands for basic resources. Central to all applications which go beyond mere description is the prediction of intermediate demands from known or assumed final demands, or from another point of view, of the total output required of each industry given the final bill of goods and services to be produced by the economy. In econometrics, as in other spheres, it is true that something cannot be gotten for nothing; in particular, quite rigid assumptions are required in order, first, to derive the industry relations between inputs and outputs, and, second, to use the relations to make the required predictions. The assumptions basic to input-output analysis are: (a) Each industry uses each input in a *fixed* proportion to its output; i.e., if x_j is the total output of industry j and x_{ij} is the amount of output produced by industry i used by industry j , then:

$$(1) \quad x_{ij} = a_{ij}x_j, \quad i, j = 1, \dots, n,$$

where a_{ij} is the constant of proportionality. (b) The constants a_{ij} are invariant over time. This last is essential if predictions are to be made, but could be replaced by a more general assumption that the a_{ij} are known functions of other variables. The Arrow-Hoffenberg study is at once a criticism and test of the temporal invariance hypothesis and an attempt at generalization along the lines suggested.

The starting point of the authors' investigation is the observation (p. 16) that "... any assumptions that are intended to be used in applying the results of a statistical analysis to some particular use such as prediction can also be used in the estimation process and should be [so] used for maximum efficiency." If f_i is the final demand for the product of industry i (including

inventory accumulation), then the familiar balance relation of input-output analysis, which is really an accounting identity, may be written:

$$(2) \quad x_i = \sum_{j=1}^n x_{ij} + f_i, \quad i = 1, \dots, n,$$

where n is the number of industries. (2) simply states that the total output of industry i is exhausted by intermediate and final demands. Substitution of (1) in (2) yields:

$$(3) \quad x_i = \sum_{j=1}^n x_{ij}x_j + f_i, \quad i = 1, \dots, n.^1$$

If detailed data on interindustry flows, x_{ij} , and total outputs, x_j , were available over time, the hypothesis of temporal invariance could be tested directly in the context of equation (1).² Unfortunately, such data are not readily available on a comparable basis for very many years, and tests of this type are therefore rather weak. A more indirect test, but one more closely related to the use of input-output models in prediction, is to examine the forecasts of total outputs, x_i , given by solving (3) for known a_{ij} and f_i . The bulk of the tests of input-output models are of this type.³ The shortcomings of such tests are twofold: first, it is necessary to compare the predictions of the input-output model with either an arbitrary standard or another method of prediction (which, unless some very definite purpose is in mind, is also to some extent arbitrary); second, the outcomes of such tests (which have generally been unfavorable to input-output analysis) offer no route to improvement of the model. Arrow and Hoffenberg propose and undertake an alternative test of the temporal invariance hypothesis. Their test is not subject to either of these shortcomings. The chief novelty of their approach is that it makes use of both time-series and cross-section data in the estimation of the input-output coefficients, a_{ij} ; its chief advantage is that it allows for temporal variation of the coefficients in response to changes in economically relevant variables, and therefore theoretically permits both a test of the temporal invariance hypothesis and an improvement of the model in the event the hypothesis fails the test.

The final model adopted by Arrow and Hoffenberg relates the deviation of the input-output coefficient, a_{ij} , from its 1947 value, \bar{a}_{ij} , to the deviations from their 1947 values of real disposable income per capita, the ratio of

¹ A random component u_i may also be introduced in (3). u_i reflects the fact that equations (1) do not hold exactly and corresponds to a sum of similar components which may be introduced in these equations.

² The tests of Leontief and Cameron are of this type: see W. W. Leontief, ed., *Studies in the Structure of the American Economy*, New York 1953, Ch. 2; and B. Cameron, "The Production Function in Leontief Models," *Rev. Econ. Stud.*, 1952-53, 20 (1), no. 5, 62-69.

³ Many are reported in C. F. Christ, "A Review of Input-Output Analysis," in Conference on Research in Income and Wealth, *Input-Output Analysis: An Appraisal*, Stud. in Income and Wealth, Vol. 18, Princeton 1955, pp. 59-66. Arrow and Hoffenberg (pp. 26-33) give a detailed discussion of Selma Arrow's important unpublished test.

defense outlays on goods to private gross national product, time, and the ratio of the excess of j 's highest previous peak to its current output. The last mentioned variable is treated asymmetrically in the sense that it is assumed to be zero if the excess is in fact a deficit; hence, it is intended to serve as a measure of irreversible learning effects and strain on capacity rather than the more symmetrical differences between the long and short runs encountered in cost theory. Many of the objections which may be made against the choice of these variables are raised by the authors themselves; chief among these is the one which comes most readily to mind, namely the fact that factor and product prices are not introduced as at least a partial explanation of the variation of input-output coefficients over time. To support this choice the authors argue that: (a) in the short run factor proportions do not respond to price changes; (b) the degree of aggregation, which is generally vertical in nature, reduces the possibility of substitution; and (c) some of the factors which give rise to relative price variations are partially represented by other variables. Although these arguments do not seem very conclusive, the model would become quite unwieldy were relative prices to be introduced, and this is an excellent reason for leaving them out—at least in a first attempt.

The test of the temporal invariance hypothesis proposed by Arrow and Hoffenberg can best be explained in terms of the balance relation (3). Suppose, in fact, that the input-output coefficients were constant at the 1947 levels, \bar{a}_{ij} , apart from a random element; then if the residuals:

$$(4) \quad r_i(t) = x_i(t) - \sum_{j=1}^n \bar{a}_{ij}x_j(t) - f_i(t), \quad i = 1, \dots, n,$$

are computed for known total outputs of each industry and final demands over a period of time, they too should be random and unrelated to variables of economic significance.⁴ However, if the relations indicated in the previous paragraph are correct, the $r_i(t)$ should be functions of total outputs, final demands, and the previously mentioned variables:

$$(5) \quad r_i(t) = g_i(x_1(t), \dots, x_n(t), f_i(t), \text{other variables}), \quad i = 1, \dots, n.$$

The obvious test of the temporal invariance hypothesis is thus to use time series data to fit a function relating $r_i(t)$ to final demands, total outputs and the other indicated variables. Since $r_i(t)$ is defined in terms of the 1947 input-output coefficients use is made of cross-section data for 1947, but such detailed data as would be required to compute similar input-output coefficients for other years are not needed. If the variables thus introduced have statistically significant effects, then the hypothesis of temporal invariance is rejected; on the other hand, if rejection occurs, then a way of explaining the variation of input-output coefficients over time may have been found and to that extent this shortcoming of the input-output model has been repaired.

Unfortunately, this test of the hypothesis is not as easy to carry out as it might appear. When a linear form of the relation between deviations of the input-output coefficients from their 1947 values and other variables is assumed,

⁴ In fact $r_i(t) = u_i(t)$ as defined in footnote 1.

the functions g_i are found to contain no variables which can be regarded as exogenous or predetermined. Hence, it is necessary to find sufficient predetermined variables, excluded from each equation of (5), in order to identify each one individually. The authors choose a set of 19 such variables but find them so highly intercorrelated that only 6 or 7 are really usable. Twelve are actually used in estimation due to the fact that the discovery that only a lesser number should be used occurred after extensive calculations had already been made. The estimating techniques used are closely related to those recently suggested by Theil.⁵ Although the hypothesis that the $r_i(t)$ are random is rejected in every one of the four balance equations actually estimated in this fashion,⁶ the explanation of the temporal variation is generally unsatisfactory; in particular, the hypothesis that the relations are actually identified is rejected for two of the four relations estimated and a number of the estimated input-output ratios turn out to be either implausibly large or negative at one or more points in time. The authors conclude (p. 132) that "... there is little reason to put much confidence in any relations based on the model," despite the fact that they reject the hypothesis of temporal invariance. The two conclusions might appear to be inconsistent; but in fact they are not, since any evidence of significant nonrandomness in the $r_i(t)$ is sufficient to reject the hypothesis of temporal invariance.

Besides estimating equations (5) for four sectors using additional predetermined variables, Arrow and Hoffenberg also estimate them individually by a procedure which minimizes the sum of the absolute values of the residuals. However, in this case they impose a priori restrictions on the signs and upper bounds of the input-output ratios over time. An ingenious linear programming estimation technique is developed in order to handle these restrictions which take the form of inequalities. The authors, however, recognize two difficulties with the procedure: First, it is not really possible to apply the proper sort of significance test to the results in order to accept or reject the hypothesis of temporal invariance. Second, despite the fact that the estimated input-output ratios do not and cannot take on implausible values at any time, their values are suspect due to the simultaneous nature of the relations involved.

Although formidable computational difficulties might be encountered, it would appear worthwhile to impose similar a priori restrictions on the simultaneous-equations approach which Arrow and Hoffenberg first used to estimate their model.⁷ The reason for this is simply that there are many ways of

⁵ H. Theil, *Economic Forecasts and Policy*, Amsterdam 1958, pp. 334-61. Similar techniques were developed independently by R. Basman, "A Generalized Classical Method of Linear Estimation of Coefficients in a Structural Equation," *Econometrica*, Jan. 1957, 25, 77-83.

⁶ Lumber and wood products, crude petroleum, petroleum products, and rubber products. For various reasons the temporal invariance hypothesis was not tested for the remaining 66 sectors included in the study.

⁷ If Theil's two-stage least-squares method were used, this might not prove as difficult as Arrow and Hoffenberg indicate. This is especially likely in view of the fact that practical methods of quadratic programming are now available, as they were not when Arrow and Hoffenberg did the bulk of their study.

achieving identification of economic relations other than the standard method of excluding predetermined variables from the equation to be estimated, e.g., taking account of nonlinearities or other types of constraints. The difficulties which the authors encountered in estimation were due essentially to the fact that they did not find a list of predetermined variables sufficiently independent of one another to identify the relations estimated. If the a priori restrictions had been imposed, it is conceivable that the relations would have been identified despite the smaller number of excluded variables; furthermore, the input-output ratios clearly could not be implausible at any time. In any case, the authors' negative conclusion with respect to the positive contribution of their study does not appear to be entirely justified on the basis of the evidence presented.

Econometric studies are easy to criticize on the grounds that all possibilities were not explored. This study is no exception. However, as great as the authors' resources evidently were, they were not infinite; hence, such lacunae as exist are excusable. Furthermore, more than half the volume is devoted to a detailed presentation of the basic data and the methods by which these were obtained; hence, it should be possible for others to build on their work with relative ease.

This is an important and valuable book. It is noteworthy for its useful insights into econometric techniques and the process of empirical research in economics, and this in a field, the analysis of interindustry demand, in which application of statistical inference has been relatively neglected. Although the exposition is not as transparent as the material might allow, the volume should prove a useful reference in courses on econometrics and an excellent source of further investigations in the area of interindustry economics.

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Individual Choice Behavior: A Theoretical Analysis. By R. DUNCAN LUCE.
New York: John Wiley and Sons, 1959. Pp. xii, 153. \$5.95.

Professor Luce bases the theory of behavior that he presents in this book on the following axiom. Consider a subject faced with the set U of possible alternatives. Let T be a finite subset of U from which the subject must choose an element and denote by $P_T(S)$ the probability that the element that he selects belongs to the subset S of T . Introduce also, for simplicity, the notation $P(x, y)$ for $P_{[x, y]}(x)$ when $x \neq y$, agreeing that $P(x, x) = \frac{1}{2}$.

Axiom. Let T be a finite subset of U such that, for every $S \subset T$, P_S is defined.

- (i) If $P(x, y) \neq 0, 1$ for all $x, y \in T$, then for $R \subset S \subset T$

$$P_T(R) = P_S(R)P_T(S);$$
- (ii) If $P(x, y) = 0$ for some $x, y \in T$, then for every $S \subset T$

$$P_T(S) = P_{T-[x]}(S - \{x\}).$$

In Chapter 1 the author discusses the meaning of this axiom, alternative formulations, immediate consequences, relationship to previous work and problems of empirical verification. The remaining chapters are devoted to

applications: Chapter 2 to psychophysics, Chapter 3 to utility theory, and Chapter 4 to learning theory. This review, written by an economist for economists, will concentrate on the contents of the first and third chapters. But it must be recognized that this procedure is unfair to Luce whose primary concern is probably for the applications of his axiom to psychophysics and to learning.

If one excludes cases of perfect discrimination, of which part (ii) of the axiom takes care, the axiom is equivalent to

$$P_S(R) = P_T(R|S) \quad \text{for } R \subset S \subset T,$$

where $P_T(R|S)$ denotes, as usual, the conditional probability of R given S relative to the probability distribution P_T . Since this axiom is sometimes misunderstood, it may be worth emphasizing that it can in no way be derived from the axioms of probability theory. One can then prove the important consequence of the axiom: under certain conditions which will not be specified here, there is on U a positive real-valued function v such that

$$P_T(x) = v(x) \left[\sum_{y \in T} v(y) \right]^{-1}$$

for every subset T of U for which (i) holds. A natural result, whose proof however requires some care in the context chosen by the author. This result, in turn, implies the existence of a Fechnerian scale on U , i.e., a real-valued function u on U such that, for $P(x, y) \neq 0$ or 1 , $P(x, y)$ is a function of $u(x) - u(y)$ only. It suffices to take $u = a \log v + b$. Aside from this, the most interesting fact, among those listed by Luce in the applications to psychophysics, may be the following for the readers of this review. Imagine that, to rank the elements of a finite set U , the subject decides first which of all the alternatives is superior, secondly which of the remaining alternatives is superior, etc. One obtains in this way a probability distribution over all the possible rankings. Imagine now that the subject selects in a set T of alternatives the *inferior* element, that those choices can be described by probability distributions P_T^* satisfying the axiom, and that to rank the elements of the same set U he decides first which of all the alternatives is inferior, second which of the remaining alternatives is inferior, etc. Assume moreover that the two families of distributions $\{P_T\}$ and $\{P_T^*\}$ are related by equalities such as $P(x, y) = P^*(y, x)$. The probability distribution of rankings obtained in the second way is generally *different* from the distribution obtained in the first. In this light, the principle that a subject ranks the elements of a finite set in the same fashion whether he considers them from superior to inferior or from inferior to superior is inconsistent with the axiom.

Chapter 3 on utility theory is a study of decomposable preference structures. Let A be the set of pure alternatives and E the set of chance events. The symbol $a\rho b$, where $a, b \in A$ and $\rho \in E$, is the uncertain alternative where a is the outcome if ρ occurs and b is the outcome if ρ does not occur. In addition to the probability measures P_T introduced so far for the sets T of uncertain or pure alternatives, probability measures Q_D are introduced now for the sub-

sets D of E to describe the probability that an element of D is thought by the subject to be the most likely to occur. The axiom is assumed to hold for the family $\{P_T\}$ and for the family $\{Q_D\}$. Finally the preference structure is said to be decomposable if:

$$P(apb, a\sigma b) = P(a, b)Q(\rho, \sigma) + P(b, a)Q(\sigma, \rho), \text{ for } a, b \in A \text{ and } \rho, \sigma \in E.$$

The rationalization is that apb will be preferred to $a\sigma b$ if and only if (1) a is preferred to b and ρ is considered more likely than σ , or (2) b is preferred to a and σ is considered more likely than ρ . The main result concerns the equivalence relation \sim defined on E by " $\rho \sim \sigma$ if and only if $Q(\rho, \sigma) = 1/2$ "; it states that if there exist $a, b \in A$ such that $P(a, b) \neq 0, 1/2$ or 1 (i.e., if discrimination among pure alternatives is not perfect), then the relation \sim partitions E into, at most, three equivalence classes.

These considerations suffice to show the strong consequences that can be derived from Luce's axiom. I may add an example to illustrate another difficulty which seems to severely restrict the applicability of that axiom. Let the set U have the following three elements:

D_O , a recording of the Debussy quartet by the C quartet,

B_F , a recording of the eighth symphony of Beethoven by the B orchestra conducted by F ,

B_K , a recording of the eighth symphony of Beethoven by the B orchestra conducted by K ,

The subject will be presented with a subset of U , will be asked to choose an element in that subset, and will listen to the recording he has chosen. When presented with $\{D_O, B_F\}$ he chooses D_O with probability $3/5$. When presented with $\{B_F, B_K\}$ he chooses B_F with probability $1/2$. When presented with $\{D_O, B_K\}$ he chooses D_O with probability $3/5$. What happens if he is presented with $\{D_O, B_F, B_K\}$? According to the axiom, he must choose D_O with probability $3/7$. Thus if he can choose between D_O and B_F , he would rather have Debussy. However if he can choose between D_O, B_F , and B_K , while being indifferent between B_F and B_K , he would rather have Beethoven. To meet this difficulty one might say that the alternatives have not been properly defined. But how far can one go in the direction of redefining the alternatives to suit the axiom without transforming the latter into a useless tautology?

Yet, even if economists think that this axiom is implausible in the choice situations that they study, this book will interest them in several ways. In particular its emphasis on the conceptual and experimental difficulties associated with any theory of choice, and its constant concern for empirical verification deserve the attention of our profession.

GERARD DEBREU

Cowles Foundation for Research in Economics

The Failure of the "New Economics." By HENRY HAZLITT. Princeton: D. Van Nostrand Co., 1959. Pp. xii, 458. \$7.50.

In this volume, subtitled "An Analysis of the Keynesian Fallacies," Hazlitt has "analyzed Keynes's *General Theory* . . . theorem by theorem, chapter by

chapter, and sometimes even sentence by sentence." Such a task could have been worth while, for the *General Theory* is far from perfect. Its style is difficult, its definitions often ambiguous, statements of fact are often unsupported, and many of its observations badly integrated into the body of thought. It is, furthermore, riddled with a contempt for free markets and their results. All this is pointed out by Hazlitt, down to such minor items as the misquotation of Voltaire ("it was not Candide who was the incurable optimist, but Pangloss.")

On the whole, however, Hazlitt does little to improve matters. Witness, for example, his condemnation of Keynes's style: "It is largely on such pretentious pleonasm and circumlocutions that Keynes's reputation for profundity seems to rest." His attempt to clear up certain ambiguities in definition is no more successful. On the question whether saving equals investment, surely no great gain is achieved by taking together the definitions of the *Treatise on Money* and those of the *General Theory* as equally good indications of Keynes's thought. A clearer understanding of the latter definitions would have made it obvious that although saving equals investment, attempts to save are bad and attempts to invest are good under conditions of less than full employment. Nor is the matter clarified by Hazlitt's acceptance of Say's law in its crudest and most objectionable form, that all savings are invested.

Hazlitt properly objects that the *General Theory* is quite special, applying only to conditions of unemployment. Yet he ignores this speciality repeatedly, accusing Keynes of error and proving the error by the assumption of relatively full employment. Thus Hazlitt proves that interest rates cannot be held below the natural rate by monetary policy because rising prices would force interest rates back up. Again, he objects that government spending might only encourage unions with excessive wage rates to demand even higher wages and the new spending may even lead to a decrease in employment. (Even under full employment this last conclusion would be justified only with rather strangely shaped labor-supply curves.)

On its statements of fact, Hazlitt's book is somewhat better documented than Keynes's, but could have used much more. As a rebuttal of Keynes, however, the documentation is grossly unfair; it takes Keynes to task because his statements do not correspond to the data of the last twenty years. Said Keynes: "workers do not bargain for real wages." Says Hazlitt: "As of January, 1958, more than 4 million workers . . . had insisted on, and secured, contracts providing for automatic wage increases with increases in the cost of living." Said Keynes: "Ice company stocks are higher in summer than in winter." Says Hazlitt: "From 1932 to 1956 . . . the shares of these companies sold higher in summer than in winter only . . . about as often as a penny might come heads instead of tails." (He then admits that these are also fuel companies, so the proof is ambiguous.) When Keynes criticizes the disorderly behavior of the American stock market, Hazlitt emphasizes its orderly character, neglecting to mention the changes in government regulation and the rules of the exchange which took place in the early 'thirties.

Hazlitt opposes the use of mathematical formulations on the ground that economic relations are not sufficiently precise to permit their use. This is

hardly an answer, for good verbal economics should be precise as well. Surely the initial mathematical formulations by Keynes have had a tremendous impact upon attempts to make these relationships more precise by more sophisticated techniques. The important question is not about perfection, but whether the approximation is sufficiently accurate to add anything to our understanding.

Hazlitt's attack on aggregation is similar. Because many interesting factors depend on the details, the use of totals and aggregates is impossible. Again, the question is whether any useful contribution is made by aggregate analysis, not whether it is the only useful kind.

It is in the discussion of depressions and their cure that the biases of Keynes and Hazlitt clash head-on, with sound and fury signifying nothing. Hazlitt continually emphasizes the general equilibrium character of society, with thousands of prices and wages. Depressions are caused by "prolonged maladjustment between prices of different commodities, or between individual wage-rates, or most often between prices and wage-rates." But in answer to Keynes's suggestion of increasing demand or prices, Hazlitt answers that employment could much more easily be brought about by cutting wages. (This is in keeping with his repeated assertion that depressions are primarily attributable to unreasonable wage demands by labor unions, certainly a strange explanation for most of the history of cycles.) Hazlitt never recognizes that, although relative prices must reflect the market, the general level of all prices is essentially arbitrary. Whether maladjustments between prices and wages should be cured by raising prices or lowering wages must depend upon the number of obstacles to each of these policies and the dislocations in the relative price structure incumbent on each. Even if unions are to blame, condemning them for their intransigence does not cure the problem. In any case, invective, whether Hazlitt's or Keynes's, is no substitute for reasoning.

Although Hazlitt clears up many minor points in the *General Theory*, his analysis of the major ones is completely unsatisfactory. In short, those who agree with Hazlitt's preconceptions will find many excuses for their views; those seeking enlightenment must look elsewhere.

JOSEPH P. MCKENNA

Saint Louis University

Social Accounting and Economic Models. By RICHARD STONE and GIOVANNA CROFT-MURRAY. London: Bowes and Bowes, 1959. Pp. 88. 9s 6d.

It is the stated objective of this book "to present a brief description of social accounting, including the input-output table, together with an introduction to economic model building." This objective is emphasized in the title of the volume, with its juxtaposition of social accounting and economic models. This theme is carried out by a concise discussion of the nature of social accounts which leads to a discussion of an input-output matrix. From there the progression is easy to input-output models, the uses and limitations of which are succinctly reviewed. A final chapter discusses models of consumers' behavior with examples drawn from econometric studies of consumers' purchases of food, motor cars, etc., in the United Kingdom. The models discussed rely both

on aggregate statistics and on cross-section data. The latter are used especially in estimates of the income elasticity of demand. To the reviewer it would have seemed more appropriate to devote the final chapter to models of the type described by Klein and Goldberger, which depend on the national accounts as systems of economic reporting in a sense in which the partial models of consumer behavior do not. But it cannot be denied that the latter do make frequent use of statistics from the accounts, such as measures of disposable income and total consumers' expenditures.

The authors also state in the foreword a more general point of view, that "theorizing is a visionary activity whose aim should be to propose a mathematical order which will fit reality." They emphasize the importance both of setting up theoretical models of how the economy works and of testing them empirically. The entire volume may be regarded as illustrating what they mean by this approach. The reviewer is in full sympathy with this general position, though he suspects that many economists would not put as much stress on empirical work. He is also in full sympathy with the proposition that the connection between the national accounts and economic models deserves more attention than it has received. There should be frequent communication between the economists who develop the accounts and those who use them in economic models.

The volume under review is not an advanced treatise and does not attempt to cover new ground. The book is not intended for use as a text but might prove a useful addition to reading lists. It requires of the reader a general knowledge of economics such as might be expected of an advanced undergraduate who has had a year or so of statistics. Some use is made of matrix algebra, though a large part of the discussion can be followed without that background. Students may find the discussion of input-output analysis particularly helpful. They will also find the volume a useful introduction to the work of the group of research economists at Cambridge. There is a bibliography which would be especially helpful for this purpose.

This book is of interest for another reason. The authors had more to say than could be compressed into an article in a journal. Instead of expanding their manuscript to the usual 400 or 500 pages, they chose to write in a clear but compact style which fills a grand total of 88 pages. May other authors and publishers follow this example!

JOHN B. LANSING

University of Michigan

The Keynesian Theory of Economic Development. By KENNETH K. KURIHARA. New York: Columbia University Press, 1959. Pp. 219. \$5.50.

The impression of this book one would gain by turning the pages and reading the index is by and large correct. Extreme use is made by Professor Kurihara of definitional manipulation of essentially an $MV = PT$ sort; an exploration of logical possibilities appears to obviate, in Kurihara's estimation, the need for data; the variables that are presumed to be potentially under the control of government are many in number and unusually wide in range. Roughly one-third of the text consists of articles and notes Kurihara has

published previously elsewhere. Economists who are mentioned in text or footnote at least as often as Kurihara himself are Domar, Harrod, Keynes, and Joan Robinson; in contrast, economists such as Chenery, Friedman, Samuelson, Solow, and Tinbergen are not referred to at all in the text.

Many students will nevertheless, find this book useful because several variants and extensions of simple Harrod-Domar type growth models are spelled out. Other students will find appealing the somewhat bland assertion that "models of *laissez-faire* growth have the negative virtue of demonstrating how precarious and unfruitful it is to leave the secular growth of an economy to the vagaries of private saving and investment, to the accident of profit-motivated inventions and innovations, and to the working of unguided market forces" (p. 185). Most economists, however, will be disappointed to find that Kurihara has devoted his energies to, and that a university press has spent resources on, what is at best a narrow and oversimplified treatment of growth processes. Of this sort of analysis we already have a great deal, while growth models which are empirically relevant or which incorporate political variables are in short supply.

As for organization and content, after three introductory chapters (one describing briefly some older theories; one describing some important characteristics of underdeveloped countries; one stating some requirements that a "desirable" growth pattern might satisfy), Kurihara deals in turn with capital accumulation, technological change, unemployment, income redistribution, monetary and fiscal manipulation, and foreign trade, all in the setting of an underdeveloped economy. The first of these later chapters reproduces and comments on models initially spelled out by Harrod, Domar and Joan Robinson while the rest consist largely of articles previously published by Kurihara. As already suggested, Kurihara is convinced that underdeveloped economies must and should marshal the power of central government to control consumption, investment, foreign trade, and possibly population. In support of this view he has collected (and modified) stock arguments against the use of *laissez-faire* control devices; e.g., he points to infant industries, external economies, demonstration effects, redundant labor. He is aware that Harrod-Domar-type models depend critically on special technological assumptions but, in spite of this, policy prescriptions are couched entirely in aggregate terms. Presumably, Kurihara does not wish to indulge "in the luxury of disaggregation" or "apply rather mechanically the classical allocation principle to a situation that calls for economic organization and planning along macroeconomic lines" (p. 200).

Kurihara's writing style is reasonably clear (exception: "an economy may be underdeveloped, not necessarily because its propensity to procreate is too strong relative to its capacity to produce, but possibly because its capacity to produce is too weak relative to its propensity to procreate" p. 27), but he often elaborates the obvious while imposing a burden on the reader to remember definitions. For example, "denoting $\Delta Y/Y$ by G_n , equation (4) can be reformulated as

$$G_n = \frac{\bar{v}}{a_0/(1 + g_a)^t}."$$

Now N is the labor force and Y is the (maximum) associated output while v is $\Delta N/Y$, a_0 is the initial value of $\Delta N/\Delta Y$, and g_a is a constant. Hence, Kurihara has taken a definition, added the assumptions that v is a constant while a decreases over time, and thus demonstrated "that the rate of growth of full-employment output is capable of increasing in inverse proportion as the labor-output ratio decreases over time" (p. 84).

JOHN BUTTRICK

University of Minnesota

Probleme und Ansätze einer kapazitätsorientierten Investitionspolitik. By DETLEF LORENZ. Berlin: Duncker & Humblot, 1958. Pp. 158. DM 13.80.

The main body of this study is devoted to a clarifying survey of the German and English literature on anticyclical and noncyclical investment theory. The first part deals with micro- and macro-investment theories in general; the second part examines special problems of a capacity—rather than income—oriented investment theory; the third part infers political conclusions from the theories evaluated.

In regard to public investment, the author insists that we should analyze all forms of investment by governments. Yet his classification into anticyclical, economic and fiscal as well as political and military is hardly fruitful. The thesis that anticyclical decisions are demand-oriented, while "economic" investments depend upon the supply of available capital, is not developed. There is especially no explanation as to why the supply of public capacity usually lags behind the demand for public facilities. The criteria for "political" investments are examined in terms of the theory of cumulative interventionism, producing a value judgment rather than a theory. One surmises that the attempted comprehensive theory of public investments would have been more successful if the analysis had aimed at a theory of the public sector of the economy.

In his examination of the various theories, the author sides with those who rejects the thesis of a *general* tendency towards underinvestment. Industries are divided into four groups according to their investment criteria. Immediate consumption demand rises with income, which increase provides an effective criterion for private investment. Industries with a rising demand due to population increase experience a lag in the provision of additional capacity. Industries affected by technological progress benefit from a neutral capacity effect since capacity increases simultaneously with demand because of price reductions. It is only in the strictly durable goods industries that there are cyclical fluctuations in demand and supply of capacity. Rather than having to commit itself to a general anticyclical investment policy, the government does not need to concern itself with the demand for immediate consumer goods. In cases of technological improvement, the government should urge producers to reduce prices promptly and sufficiently. Industries directly affected by population increases could be assisted in providing reserve capacity. Only in durable goods industries is there a possible place for anticyclical policy of public investment.

Yet the author seems almost convinced that even for the durables there

are efficient compensatory factors originating in private concerns. With a few qualifications he accepts the recently developed thesis that industrial empires have the capacity to engage in a noncyclical investment policy, thereby drastically reducing the amplitude of cyclical fluctuations. The function of the government is then limited to providing statistical information indicating the socially desirable capacity. In most cases demand and supply of capacity would thus be equated through internal business planning and external counseling of private decision-makers by the government.

This happy solution hinges upon an idealization of big business. One can agree with the author that the activities of industrial empires cannot be explained in terms of the traditional theory of restrictive monopoly. Rather than calling the massive firms competitive, we have to place them in their category as a concentrated property structure: concerns that receive interest-free capital through retained profits, benefit from preferential treatment in capital markets, own a multitude of plants, produce an endless number of products, enjoy special access to innovations, monopolize and divide investment opportunities among themselves. The very size of assets leads to the building up of exclusive, expansive or cooperative monopolies. A theory of these monopolies alone can ascertain why and how the monopoly effect leads to an insufficient capacity, or the acquisitive drive for more assets to an excess of capacity supplied by industrial empires.

ARTHUR SCHWEITZER

Indiana University

Der Lagerzyklus—Lagerbewegung und Konjunkturverlauf in empirischer Sicht. By HARRY SCHIMMLER. Berlin: Duncker & Humbolt, 1958. Pp. 113. DM 12.—.

The author examined the quarterly inventory series for Western Germany from the middle of 1949 to the middle of 1955 with a view to explaining the time-path of inventories as the sum of a desired and an undesired component. The magnitude analyzed is the inventory ratio B/N , where B denotes inventories and N the "total effective demand," which is largely identical with our GNP series. The desired *change* in inventories (L^*) is considered approximately proportional to N , except for the price influences:

$$L^*/N = \gamma + P''$$

By some involved consideration to which we shall return presently, the change of the undesired inventories U is given by the equation:

$$(1) \quad \Delta(\Delta U) = -\lambda U - \epsilon \Delta U$$

where λ and ϵ are reaction coefficients. They are both found equal .25; γ slowly varies from .3109 in the second quarter 1950 to .3296 in the last quarter 1953, and then falls to .3248 in the second quarter 1955; the price effect is very small.

The behavior functions governing the change in U are formulated as fol-

lows (p. 43): (a) The desired inventory change equals the change in the desired inventory minus the undesired inventory: $L^* = \Delta G - U$ (as consequence of a misprint we read on p. 43 L in place of L^*). (b) The firms want to compensate the undesired change (R) of the inventory [which has been shown, on the basis of (1) to equal $U + \Delta U$] or, in a sufficiently short period, a fraction of it, yielding equation (1) right side, above.

I am unable to follow this argument as to (b). First, it is not clear, a priori, why firms should orient their production decisions to the undesired *change* in inventory and not exclusively toward the *level* of the undesired inventory; nor is Schimmler's econometric method adequate statistically to confirm this hypothesis. Even more important is the point that the two assumptions (a) and (b) are not consistent: According to (a) the firm, during period $t + 1$ tries to eliminate U in full; in (b) a different attitude is assumed. It is only by cumulating these inconsistent assumptions that Schimmler is able to derive equation (1)—a difference equation of second order in U ; is it reasonable to assume that the time-path of undesired inventories is independent of the path of the desired ones, which are governed by the total effective demand? In the special chapter contributed by Klement a complete, though still simplified, model is developed covering also consumption and induced investment, and leading to a differential equation of the third order in *income*. On the basis of the available material the evaluation of the coefficients in Klement's model is not possible, and this may have induced Schimmler to look for a simpler approach.

However, it is important to notice that Schimmler's results for the desired inventories (the coefficient γ and the absolute size of the desired and undesired inventories) are acceptable even if his approach concerning the coefficients γ and ϵ is rejected. His coefficient γ (in the estimate of which the data for 1949 were disregarded) fluctuates very little, and the fluctuations in γ are not indicative of the type of nonlinear relation between inventories and sales which Whitin and others have suggested and to which Schimmler himself refers in one place (pp. 55-56). If it is agreed that the desired inventories are approximately proportionate to the total effective demand, then the usual correlation analysis would also have yielded a value approximately equal to .32.

Schimmler also presents a comprehensive model of the economy which covers consumption, investment and autonomous factors. It is of an econometric nature, i.e. it is not reducible to a difference equation in one variable. Schimmler examines briefly the other coefficients of this model as they appear retrospectively if the available data for the variables are inserted period by period. I cannot agree with him that the "structure of demand" n (which equals $1/(1 - \alpha)$, where α is the average propensity to consume in the widest sense including depreciation, etc.) has proved rather stable for 1949-55 or 1950-55; in 1950 it was 2.32 and in 1956 equal to 1.91. Of greater interest are his attempts to estimate for this period the "capital coefficient" β (the Hicksian v) and the "capacity coefficient" which indicates the influence of existing unutilized capacity on the willingness to invest. It would take considerable

space to describe the estimation procedure; suffice it to say that in spite of the inevitable crudeness of method Schimmler has in the reviewer's opinion here obtained useful results.

HANS NEISSER

New School for Social Research

The Economic Mind in American Civilization 1918-1933. By JOSEPH DORFMAN. Volumes 4 and 5. New York: Viking Press, 1959. Pp. xxxiv, 398; liv, 375. \$12.50.

With these two volumes, Professor Dorfman brings to a close his massive work on the development of economic ideas in this country. Volumes 1 and 2 covered the period from colonial times to the Civil War; Volume 3 brought the story up through the first world war, and the present volumes span the years 1918-1933. Here it ends, partly because the subsequent years are too close for the historian's perspective and because, as the author sees it, the main ideas of the New Deal era are worked out in the period covered by these two volumes.

The five volumes taken together are the product of prodigious scholarship, and they illuminate a neglected aspect of the history of economic thought. In general the standard histories give scant attention to U.S. economic thought, although both Schumpeter and Hutchison analyze in detail a few U.S. economists. Those few works that are devoted to U.S. economic thought, such as E. A. J. Johnson's, E. Teilhac's, and A. G. Gruchy's, have limited coverage, namely the 17th century, the 19th century, and Institutionalism. Both Teilhac and Gruchy give a detailed account of relatively few authors. O'Connor's study of the academic origins of the subject is largely concerned with teaching materials, not doctrinal developments. For breadth and coverage, *The Economic Mind in American Civilization* has no rival.

Following the pattern of the earlier volumes, Dorfman goes well beyond the confines of academic economics for the period 1918-1933. This is a study of economic ideas, quite broadly interpreted, as they are influenced by developments in sociology, philosophy, psychology, industrial management, and public policy. The sources cited indicate the "public" character of the ideas reviewed—congressional hearings, liberal magazines such as the *New Republic*, messages and speeches of public figures, reports and pamphlets, and popular books on current issues. These sources liberally supplement the professional journals, texts, and other works of the academic economists. The spectrum of ideas and interests is wide; analytical economics occupies a rather narrow band.

The material in these volumes is organized in two parts: *The Era of Adjustment and Progress, 1918-1929*, and *The Great Depression 1929-1933*. Three-fourths of the combined two volumes concerns adjustment and progress following the first world war. Adjustment embraces the period of reconversion and the depression of 1920-21, and progress covers the years from 1922 to the crash of 1929. Dorfman meticulously reviews the ideas of both amateurs and professionals on the innumerable practical problems of readjustment and the subsequent prosperity. Unemployment, the planning of public works

(with the germ of the multiplier), social insurance, industrial relations, monopoly, agricultural "adjustment," Federal Reserve policy and stabilization, the business cycle, banking reform, stock market speculation, and economic planning are among the problems extensively covered.

The author gives every man his day in court; as a consequence the reader finds himself going through the same ideas over and over again. Such repetition is unavoidable, given the person-by-person treatment in this work. However, the inclusion of some of the persons and the distribution of emphasis puzzle this reviewer. Three pages are given to the socialist views of Steinmetz, the electrical engineer, but there is only passing reference to Norman Thomas. E. H. Downey gets five pages on his workmen's compensation ideas, and Rubinow has seven pages on social insurance. These ideas recur again and again. There are too many quotations from too many writers on too many diverse subjects. But occasionally a quotation is prophetic and succinct, as this one from a Coolidge message vetoing the McNary-Haugen bill: "Government price-fixing, once started, has alike no justice and no end."

Dorfman, of course, devotes most of this work to the professional economists. Analytical contributions are subordinated to policy views and to the positions taken on the scope and method of economics. This is in keeping with his purpose: to portray the "economic mind," or the outlook on socio-economic problems and policy, rather than techniques. There are excellent briefs on such figures as J. M. Clark, W. C. Mitchell, Walton Hamilton, F. H. Knight, and Jacob Viner, and shorter accounts of Copeland, Douglas, Tugwell, Slichter, and many others. The positions of the elders of the period—Taussig, Ely, Young, Bullock, J. B. Clark, Fisher, Seligman—are given sympathetic discussion. Brief references are made to the influence of foreign economists, particularly Keynes and briefly Marshall. Strangely, to Cassel is attributed a "substantial role in stimulating interest in the study of price determination and in neoclassical economics as a whole" (p. 165).

Dorfman seeks to show the profound changes in the outlook of economics brought about by the "new" generation of economists. J. M. Clark is in the forefront of this broadening and humanizing movement. Industrialization, the concentration of economic power, the social problems of an urban civilization, labor relations, the plight of agriculture, and recurring depressions cause problems and bring to the fore the need for social control, group responsibility, and the preservation of individualism. The emphasis is on "problems," and economists increasingly move towards specialization in efforts to find solutions to these problems. As Dorfman sees it, the main trend is in the spirit of institutionalism and reform. The author skillfully arrays his materials to bring out the main currents, and through copious quotations gives his subjects ample opportunity to make their points.

Looking back on the era of adjustment and progress, Dorfman seems satisfied with the results. Progress had been made in the delineation of problems, in pointing to solutions, in the gathering of statistical materials, and in the research directions taken. Economics had gained enormously from its assimilation of ideas and approaches from psychology, philosophy, sociology, and administration. Economists were in the thick of things, advising govern-

ment, business, labor unions, and the public at large. In his chapter "The Legacy of the 'Twenties" he said "As the period closed, the economics profession showed a deep faith in the ability of American capitalism to achieve material and social progress." Yet they showed little awareness of what was to come. The "outsiders," Foster and Catchings, were more perspicacious.

Part II, the Great Depression, is discouraging. Dorfman marshalls the best thought of the period in these fascinating chapters. Academic economists fare badly in this review—confused, bewildered, uncertain, and quarrelsome. In this context, policy prescriptions come largely from the amateurs. Dorfman suggests a reason: "In the vast and complex American economic order, with its diversity of interests, one policy could hardly have been expected to be adequate and acceptable to all. The statesmanship required was not novel; it was essentially the ability to assess a multiplicity of diverse remedial measures and to weave them together into a workable over-all program. Why, then, was this so difficult to do? A substantial part of the answer lies in an undue lag, deriving from the 1920's, between the needs of a dynamic economy and public policy . . .; and closely related, the failure to define clearly and to re-adjust the relationships between government and the economic system." In the failure of the professional economists to assess and to weave together the widely proposed remedial measures lies their inadequacy in this period.

Dorfman generously ascribes this failure to the complexity of the problem. But to this reviewer there is a more basic reason. Specialization, the preoccupation with particular "problems" and their reform, and the emphasis on social philosophy, method, and approach diverted the attention of economists from the economy as a whole and its basic processes and functions. In the early 1930's they wrangled over the bits and pieces, because this is what they had been doing throughout the 1920's—as this monumental work of Dorfman's so adequately brings out.

ARTHUR E. BURNS

George Washington University

A History of Economic Ideas. By ROBERT LEKACHMAN. New York: Harper and Brothers, 1959. Pp. xiii, 427. \$5.00.

Of the seventeen chapters in this text, six are devoted to a description of the classical school (Part II). In Part III, the marginalists are allocated two chapters followed by a brief chapter on the Methodenstreit and a chapter on institutional economics, the latter being essentially a quick run-down on Veblen's criticism of marginalism. Part IV takes up the modern period, with individual chapters on Keynes, business cycle theory, price theory of the Chamberlin-Hicks variety, and a hodgepodge titled "The Analysis of Capitalism." The latter runs all over the lot, from an outline of Schumpeter's prognosis, to Pigou's welfare analysis, to the Lange-Lerner-Dobb debates of the 1930's. Greek, Roman and medieval theory, as well as the mercantilists, are treated in the first three chapters (Part I). Nothing is discussed in any detail and the reader has the feeling that the author is ticking off the great names of economics without really giving any of them a chance to explain his position.

✓ Smith, Bentham, Malthus, Ricardo, J. S. Mill, Marx, and Keynes each receive the dignity of a whole chapter, while all other writers receive a "Who's Who" treatment. The reviewer noted a total of 53 economists who were given separate billing, with anything from a chapter to a few lines summing up their contributions. The heavy emphasis upon the classical school is explained by the author thusly, "Economics does its job best and commands most respect when economists concern themselves with the problems which afflict ordinary human beings." Since, in his opinion, the classical writers dealt with real issues, as did Keynes, his preference and prejudices led him to stress their contributions, rather than those of writers who have merely engaged in "intellectual gamesmanship" (p. xii).

While few can deny that the economics profession harbors many writers who follow the tenets of Stephen Potter, it would seem that Lekachman should have supplied the reader with some criterion by which to judge when economists are engaged in gamesmanship and when their endeavors are worth while. For example, was the development of the concept of elasticity, or of consumers surplus, merely a Marshallian ploy? Lekachman's implicit criticism of a good deal of formal economics is testimony to the fact that he really has no sympathy for the essential problem of theory *qua* theory. What this means is that the author really is not interested in discussing the subject matter of economics. Even the influence of the sage, Adam Smith, was dependent upon his conceptualization of the problems of 18th century England, rather than his recognition that they existed. Lekachman does not seem to realize that what marks the contribution of an economist is not only the issue he chooses to discuss, but the theoretical analysis he develops to deal with the issue.

As might be expected, Lekachman's approach is one which stresses the historical background that gave rise to such ideas as Ricardo's profit, and Malthus' *Essay on Population*. This approach is old hat, and the discussion does not throw any new light on the relation between the development of theory and its historical origins. Lekachman also devotes considerable space to the private lives of the great, and while undergraduates perhaps appreciate a peek into these corners, it seems that the space devoted to discussing the domestic affairs of, say, Marx and Ricardo could be better allocated to more detail on their respective systems of analysis.

In large part this volume reflects the dilemma of a shrinking, but courageous, corps of economists who cling to the idea that it is desirable to offer an undergraduate course in the history of economic thought. The reviewer hastens to add that he includes himself as a member of this minority. However, with expanding curricula not many institutions can devote more than a semester to an undergraduate course in the history of thought. Lekachman's volume, apparently, adheres to the idea that under these restraints it is best to cultivate extensively and to cover as much territory in the history of ideas as possible. His volume, for example, runs the gauntlet, in 400 pages, from Plato to Hicks' *Value and Capital*. In contrast, it took Schumpeter 1200 pages to go over the same area, and his work was published on large sheets with small print!

Lekachman's volume raises serious question as to the usefulness of a canvas which has been painted with such large and uneven strokes. The major criticism, therefore, rests on the fact that Lekachman, in applying the law of scarcity, has sacrificed depth for breadth. For the undergraduate, it would seem that Jevons, Menger and Walras are near perfect substitutes for one another, and that Edgeworth, Fisher and Xenophon could easily be classed as inferior goods. Is it not more desirable to allocate the scarce teaching time, as well as the printed page, to dealing with a few representative writers from whom the student can gain some appreciation for the difficulties in the growth of economic theory, and for the relation of theory to the issues which gave rise to it? Textbooks like the one under review, wherein the discussion changes the subject every page, only tend to give the student a "quiz program" familiarity with the great minds in economic thought; and it is to be hoped that education in this country has advanced beyond this pitiful stage.

JOHN P. HENDERSON

Michigan State University

Economics of the Business Firm—Economics of Decision Making in the Business Enterprise. By JOSEPH D. COPPOCK. New York: McGraw-Hill, 1959. Pp. xiii, 366. \$6.95.

It is sometimes argued that there is a wide gulf between economics and business, that while economic analysis offers much by way of explanation of how our economic system operates, it has little practical value for actual or potential business administrators. Coppock feels differently. He believes that the tools of marginal analysis as presented at the intermediate level are much more than a logical exercise or a guide for public policy makers. To him they are indispensable theories for "the businessman who seeks to make profitable decisions."

Since the book is written primarily for business school students, one might think it would be a popularized Joel Dean. It is not. Instead it is almost entirely a series of lessons in formal logic, very carefully written to demonstrate step by step the abstract mechanics of profit maximization within the mathematical limits of two- or three-dimensional geometry. In fact, one gets more than half way through the volume before finding anything beyond formal models of maximum, minimum, and equilibrium situations encountered by the firm. Even the price discrimination cases are presented in the abstract without enrichment by illustrations from real life. This approach may appeal to some teachers, particularly those who like to keep their illustrative material up to date by using current articles, reports, and documents. Others may find it too dry and lacking in student interest.

It is not until Coppock takes up his nine models of commodity markets (based on combinations of one, few, and many buyers or sellers) that he lets himself relax enough to make a few comments on such things as the collusive ways of oligopolists and the problems of polyopsonist consumers. In a later chapter he uses a similar ninefold path to the analysis of the markets for factor services and again turns slightly institutionalist. A concluding

chapter of only eight pages is devoted to the applicability of marginal analysis to the real problems of business firms. In it he briefly refutes the usual objections to the applicability of the analysis because of the existence of the following complications: multiple products and multiple inputs, inadequate knowledge of demand and cost functions, the lumpiness of certain inputs, uncertainty, variations in market structure, and the operation of nonprofit motives.

Like most writers of texts, the author does not strive to contribute new ideas as much as improved presentations. Among the latter the reviewer was impressed by the consistent symmetry of approach in treating a large number of demand and supply concepts for both commodity and factor markets, though with some skimping on the latter. Coppock also stresses the need to consider the state of the other side of the market in explaining how one arrives at a true supply curve or demand curve. For instance, only when the opposing demand curve is horizontal do the marginal cost curve of a firm or the "marginal withholding curve" of a factor-owner become true supply curves. Similarly, only when the opposing supply curve is horizontal do the marginal factor revenue curve of the firm as demander and the marginal benefit curve of the consumer become true demand curves. The reviewer would also commend the insights of Chapter 17 which is devoted to methods and problems of estimating demand. For those who like a text with end-of-chapter questions and references, Coppock offers an abundant supply of the former, both memory and thought types. The supplementary readings are more scanty, confined to books, and generally without annotation. The numerous diagrams are usually clear and well integrated with the argument.

The author expresses few value judgments and when he does, is usually careful to define the standard of judgment being used. In several places, however, he seems to suggest to potential business administrators that if and when they find themselves in "oligo" positions, they give serious thought to the advantages of "cautious collusion" and combination. (Cf. pp. 222, 229, 240, 320.) Here "stability" seems to become at least the equal of the profit-maximizing motive in determining appropriate business conduct. Yet in one of the noticeably few paragraphs devoted to the social implications of business decisions, he says, "Businessmen have to be reminded occasionally of these [freedom] aspects of the system under which they operate, since they devote so much of their attention to the creation of monopoly positions, which, if securely achieved, would destroy the capitalist system, or strip it of its virtues" (p. 244).

ROBERT B. PETTENGILL

American University of Beirut

Economic History; Economic Development; National Economies
Patterns of Trade and Development. By RAGNAR NURKSE. Wicksell Lectures 1959. Stockholm: Almqvist and Wiksell, 1959. Pp. 62. 3.50 SKr.

The Wicksell Lectures at the Stockholm School of Economics were inaugurated in 1958 by Erik Lindahl. For the second year, Ragnar Nurkse

was selected for this distinguished role, which he performed in two lectures on April 7 and 10, 1959, only a month before his untimely death in Geneva. In a brief introduction to the published lectures, Erik Lundberg expressed the sentiment that publication of the essays was the best way in which he and his colleagues could honor the memory of Ragnar Nurkse.

The first lecture deals with "Contrasting Trends in 19th and 20th Century World Trade." During the century from 1815 to 1914, international trade was, to a degree not subsequently realized, an engine of transmission of economic growth. Britain's income increased tenfold but her imports increased twentyfold. It was principally through this great demand for primary products that the "regions of recent settlement" in the world's temperate latitudes—Canada, Argentina, Uruguay, South Africa, Australia, New Zealand—were provided with the drive toward economic development. But scarcely less important was the fact that the United Kingdom complemented its imports from these regions with a heavy flow of capital, which grew from one-third to two-thirds of British capital exports from 1870 to 1913. Such regions as China, India, tropical Africa, and Central America were not blessed by either a demand for their exports on any comparable scale nor by a comparable flow of capital, and they failed to develop in any such degree as the "regions of recent settlement." In these more fortunate regions, the great markets for primary products in the developed economies and the inflow of capital from that quarter built up not only the export sectors but also the overhead facilities for domestic activities as well. The whole evolution belied the Marxian diagnosis of capitalist foreign investment as aiming at the control of colonial markets and the dumping of excess supplies.

The twentieth century has witnessed a marked slackening of the growth of international trade. During the three decades from 1928 to 1958, the rate of expansion was only one-fifth as large as it was a century earlier. Furthermore 43 per cent of international trade takes place among industrial countries, and only 9 per cent among nonindustrial countries (the remainder being between these categories). Nurkse points out six causes which have contributed to the relative decline in demand for primary products and to the shifting of the importance of international trade as a growth mechanism into low gear.

In the second lecture, he examines industrialization, first for export markets and secondly for home markets, as a means of economic development. Industrialization implies neither the abandonment nor the contraction of exports of primary commodities, but a desirable complement to primary production in the setting of the twentieth century. Industrialization for export is particularly important to densely populated regions as a means of commanding an adequate food supply. But such a policy may encounter difficulties. On the side of the supplying of industrial products, the unskilled labor of densely populated areas may not be really cheap, and there may be a wide gap in the comparative costs of primary as compared with manufactured exports. On the demand side, the older industrial centers may have pronounced comparative advantages; and if not, they may adopt protective devices.

With potential limits on industrialization for export, the less developed countries may turn to industrialization for sales to their own markets. First it has to be realized that, to sustain the new industrial workers, food supply must be increased *pari passu* with manufacturing. Here lies a rub, however: agriculture is often tradition-bound, and the tropics do not seem to hold out as much promise physically as did the regions of recent settlement. But most observers believe that there is ample room for improvement and increased productivity.

Just as agriculture must complement the growth of manufacturing, so the various segments of manufacturing must grow together. Balanced growth accelerates the general rate of output growth. More technically expressed: output must be diversified according to domestic income elasticities of demand. With the growth of industrialization for the home market, the less advanced economies can begin to export manufactured goods to the wealthier countries. But for some time to come it may be expected that the younger industrial countries will have to import capital goods. One of the most conspicuous features of twentieth century trade is the growth of capital-goods imports by the less developed economies. Nevertheless, in the current scene, it is production for the home market, rather than international trade, which holds the key to development in most of the low-income countries.

It is characteristic of Nurkse's intellectual vigor, integrity, and flexibility that he should have proclaimed these conclusions which somewhat reduce the importance of international trade, despite a lifetime devoted chiefly to this subject. No doubt his greatest systematic works are *Internationale Kapitalbewegungen* (1935) and *International Currency Experience* (1944). But on the theme of economic development, to which his contributions took the form of lectures, as for example *Problems of Capital Formation in Underdeveloped Countries* (1952), and numerous symposia and articles, we have the greatest accomplishment of his mature years. The present lectures carry forward this distinguished literature. The scholarly world has lost immensely through the death of one of the most productive, knowledgeable, and wise economists of our day.

HOWARD S. ELLIS

University of California, Berkeley

Business Enterprise in its Social Setting. By ARTHUR H. COLE. Cambridge: Harvard University Press, 1959. Pp. xiii, 286. \$5.50.

The Harvard Research Center in Entrepreneurial History, which operated for many years under the leadership of Professor Cole, conducted numerous studies of entrepreneurs and business firms covering many centuries and several countries. The Center, taking its inspiration from Schumpeter's theory of economic development, was concerned primarily with the role of the entrepreneur in economic change.

In this book, Cole has attempted to draw together the implications and generalizations of many separate studies done at the Center and elsewhere. The book is a report on a massive research effort extending over many years. It is also an example, rarely encountered in modern economics, of the inductive

method based on historical research. Cole has attempted, he says, "to extend a bridge between history and theory" (p. xi), "to restore economics to the rubric of a 'social' science" (p. xii), and to bring "all relevant factors into the models" (p. xiii). His method is to proceed "by alternate steps of empirical inquiry and tentative generalization" (p. 137). Entrepreneurial research, he says, is an interdisciplinary inquiry involving economics, history, technology, and all the behavioral sciences. It seeks to explain, and possibly forecast, the great sweep of economic change over long periods of time. Cole points out that most economics is concerned with a time period shorter than the life of an oak tree. He wishes his generalization to cover periods comparable at least to the span of a middle-aged redwood.

His basic postulate is that the entrepreneur is the central figure in economic development and the principal instrument of economic change. He struggles with the definition of the entrepreneur, a concept which does not fit easily into the facts of modern corporate life. While he sometimes refers to the "entrepreneurial team," the researches of the Center seem to have been concerned mostly with the creative individual business leader, and much of the book is devoted to vignettes of various kinds of entrepreneurs, mostly regarded as individual persons.

In the postulate, Cole attempts to formulate a theory of economic change. I found few important new insights beyond those elaborated by Schumpeter a generation ago, and little that is comparable in suggestiveness to the work of Weber or Tawney. The basic theme is that the total culture, as expressed in values and attitudes toward business or production, is an important determinant of entrepreneurial behavior.

Examples of his generalizations are these: that there are observable national differences in entrepreneurship (p. 147), that talent for entrepreneurship is found in all social classes (p. 156), that entrepreneurship can be exercised by public officials as well as by private operators (p. 213), that latent entrepreneurial talent will appear wherever the social structure provides the necessary flexibility (p. 156), that innovation can take the form of new systems of organization as well as new technology (p. 180), that facilities for communication and tendencies to secretiveness or monopoly affect the speed of transmission of innovation (p. 245).

Interesting and valid though observations of this type may be, they do not carry us very far toward a theory of economic change. In this comment, however, I do not mean to be captious. My impression is that the historical method, as the Germans of the 19th century discovered, does not readily yield sharply focused generalizations. In fairness it must be observed that the method of pure theoretical analysis also does not carry us very far in the study of economic change. One must conclude that the progress of economics in the analysis of long-term change must depend on the simultaneous use of inductive and deductive methods, and that there is much work to be done. Those on either side can scarcely afford to throw stones at those who labor on the other.

As a summary of a major research effort, I found the book valuable. The vignettes, which are summaries of research on entrepreneurship in particular

times and places, are gracefully written and suggestive. An enormous amount of history has been usefully compressed for those who may not have the time and interest to review the thousands of pages of historical writing on which they are based. The book concludes with an excellent bibliography.

HOWARD R. BOWEN

Grinnell College

Études économie humaniste—moyen âge et capitalisme mercantiliste. By ÉTIENNE ANTONELLI. Paris: Sirey, 1958. Pp. 406. 2.200 fr.

Nouvelles études d'économie humaniste—le capitalisme du XIX siècle de 1814 à 1914 et le monde économique présent de 1914 à 1957. By ÉTIENNE ANTONELLI. Montpellier: J. Reschly, 1959. Pp. 470. 2.000 fr.

We seem to have taken little notice of the emergence of a body of thought and doctrine which presents itself as still another of the many "third" solutions between the extremes of the theoretical model of the market economy and the planned economy. When Fidel Castro of Cuba described his social and economic program to his North American audiences as motivated by principles of economic humanism very few of his listeners understood what he meant. Economic humanism and humanist economy have remained terms which as yet lack any precise definition.

Professor Antonelli attempts not only to provide us with a general definition of the concept of humanist economy but also to trace the evolution and gradual extension of economic humanism within the economic life and general stream of thought of Western Europe, with occasional side-glances at the American scene. The general uncertainty which seems to surround the key concepts of man, humanism and economy in the writings of contemporary philosophers and economists leads the author first to a general epistemological examination of the two components of the composite term *économie humaniste*. According to Antonelli Greek philosophy knew man; but it knew neither humanity nor the concept of humanist economy. Plato, Xenophon and Aristotle did not reflect but actually reacted against the new and progressive forces of their time. Belonging to the conservative elements of their society who aimed at the preservation of the past, the Greek philosophers shared the general contempt for manual work and accepted the principle of a natural hierarchy of classes (slavery). On the other hand, modern Western thought gives expression to a state of social and economic development in the course of which the principles of hierarchy, slavery and caste have been replaced, however imperfectly, by the universalistic notions of human equality, common human traits and requirements regardless of race, creed, social role or status. Antonelli credits early Christianity which for more than 200 years was preached and practiced in the catacombs and never reached the ruling classes of Rome, with the elaboration of this radical and universalistic humanism. For him the term humanist economy stands for an economic order which is dominated by a concern for man and human life which, if lost sight of in the economic sphere, can only lead to a dehumanization of work, human existence and society. In a sense, Antonelli comes close to the thesis of Karl Polanyi's *The Great Transformation* without using the latter's conceptual framework.

It is against this broad background and with the aid of his rather vague definition of humanist economy that Antonelli elaborates four basic theses: (1) Economic humanism found its most perfect expression during the flowering of the Middle Ages (e.g., production of essentials "for use" and local needs rather than markets; pricing and distribution by custom rather than competition; the insistence that the economy must be a moral order; employment and development policies carried out by monasteries rather than manorial estates; protection of the weak, religious fraternities, charity, hospitality, municipal regulations against commercialization, and control of luxuries and conspicuous consumption). (2) Elements of a new economic humanism manifested themselves after the decline of the Middle Ages within market capitalism—the negation of the medieval economy (e.g., the insistence on economic individualism, private initiative, freedom of movement and freedom of contract). (3) This new economic humanism, reflected in liberalism and the model of the market economy, soon lost its substantive content and concern for "living" or "real" human beings due to the fact that economy and society were increasingly subjected to the impersonal automatism of competitive institutions and to what Jevons called the mechanics of utility and self-interest (e.g., widespread neglect of social and human costs of various kinds, depersonalization of industrial relations, control of output and "administered" prices). (4) Contemporary "mixed" economies, with their emphasis on protective labor legislation, social security, price control, subsidies, stabilization programs, antitrust legislation and the manifestation of countervailing power, represent a negation of the capitalist competitive market economy and thus, "as a negation of the negation" of medieval humanism, relate us once more with the earlier economic humanism of the Middle Ages.

The major part of the two-volume work is devoted to a historical account of economic developments and the evolution of economic thought in an effort to illustrate, elaborate and substantiate the four theses listed above. The reviewer has the impression that much of this material, while doubtless related to the author's major concern, remains insufficiently integrated with the specific points raised. Greater economy in exposition and more rigorous concentration on essentials would have improved and strengthened the book. Nevertheless the reader who has the patience to go through these long historical excursions of uneven quality will be rewarded here and there by fresh points of view, references to an encyclopedic literature and a general lucidity of presentation.

Antonelli distinguishes three major strands of economic humanism in present-day thought and action: (1) the religious or Christian current; (2) the Marxian brand with its criticism of "self-alienation"; and (3) a scientific humanism which derives its concept of man, human nature and human needs from a science of man based upon the data and results of such empirical disciplines as anthropology, biology, psychology and paleontology.

Admittedly the subject matter of the book is qualitative rather than quantitative and does not permit short-cuts or mathematical treatment. However, if the mathematical economist has an obligation not to withdraw completely into the phantasies of his constructs, symbols and models, i.e., unless he

wishes to remove himself and his work from the common world of intellect and inquiry, the literary economist including the historian of ideas owes it to our supposedly common scientific enterprise to make his definitions as precise and "operational" as possible and not to overelaborate his evidence by historical excursions without obvious relevance to the matter under discussion. Doubtless economic humanism points to important issues in a world which has as yet to find answers to the questions raised by rapid technological progress, automation, radio-active fall-out, high pressure salesmanship, deceptive advertising and lack of social balance in an affluent society, to name only a few of our current problems.. While it may be true that economic humanism has entered, modified and "disarticulated" the 19th century market economy and while it is doubtless correct that the really important problems are dynamic in character where perfection (in a static sense of optimum allocation and maximum efficiency) eludes us, economic humanism is in need of objective and substantive criteria of human well-being and social welfare. Such criteria can be derived, if at all, only from a science of man which would lend objective support to the elaboration of a theory of essential human needs (as distinguished from Veblen's "superfluities") and of minimum requirements of physical and mental health, housing, education and similar collective needs. In short, economic humanism can prove its status as a new approach to economics only by moving resolutely in the direction of a *scientific* humanism and a truly interdisciplinary approach to the science of man and society. Antonelli hints at this himself but in the end fails to elaborate the full implications of this position.

The book will be of interest to all those who want to be brought up to date on the discussion of economic humanism in France and to economists who question the contemporary withdrawal into model building for analytical and predictive purposes without defining the actual social space and time to which either the model or the prediction may have reference. As such it can serve as a timely reminder that social institutions and arrangements need to be evaluated in terms of criteria that are related to man and human life and that such an evaluation is a legitimate scientific concern also for the economist.

K. WILLIAM KAPP

Brooklyn College

The State and Economic Growth. Edited by HUGH G. J. AITKEN. New York: Social Science Research Council, 1959. Pp. x, 389. \$3.75.

This is a collection of papers given at the Conference of the State and Economic Growth held in October 1956 under the auspices of the Committee on Economic Growth. It is the seventh of such conferences, the first being in 1951, and is the fourth to have its proceedings published. This new branch of economics deals chiefly with methods used by nations to industrialize, whether they be deliberately designed, as in Russia today, or unconsciously adopted, as in the United States a century ago. The economist believes that he can make important contributions to this branch of economic history by applying his tools of analysis and by comparing the experiences of many nations, something increasingly needed in our fast shrinking world. Implied too is the idea that

national leaders everywhere must find ways to reduce the great discrepancies in living standards between countries. This book of essays by 13 authors analyzes the economic status of various nations when they began to industrialize, their methods of industrializing, and their degree of success.

The conference took as its theme a "model" by Bert Hoselitz with which countries may be analyzed simultaneously in three ways: First, did the nation grow by *expanding* its frontiers or by *intrinsic* (internal) development? Second, was it a *dominant* (economically self-contained) or *nondominant* nation? Third, were the decisions allocating resources *autonomous* (nongovernmental) or *induced* (governmental)? There are eight possible combinations of these six types of situation. For example, the United States was rated expansionist, dominant and autonomous, whereas the "people's democracies" of Eastern Europe are today the opposite. Each paper deals with a country, or a group of countries, representing one of the possibilities. Among the countries are the United States, 1820-90, discussed by Henry Broudé; Australia, 1860-1900, by Noel Butlin; Canada, by Hugh Aitken; Russia, 1890-1939, by George Carons, Jr.; Manchuria, 1860-1940, by Edwin Reubens; Germany, by Norman Pounds; French and German Mining, by William Parker; Switzerland, by Alfred Bürgin; Turkey, 1933-50, by Robert Kerwin, and Eastern Europe's satellites, by Nicolas Spulber. In addition, there are concluding essays by Richard Hartshorne and by Bert Hoselitz, and a summary of all the papers of the conference's conclusions by Joseph Spengler. The papers vary in clarity and in the extent to which the authors keep their assignments in mind. Of the country papers those by Butlin on Australia and by Parker on French and German Mining were particularly impressive. Richard Hartshorne's "The Role of the State in Economic Growth" was especially rewarding. In it he offers a table measuring the attainment of various nations (pp. 292-94) and he also analyzes the eight factors most essential to economic growth. The most advanced nations have had ample power resources within them and have been able to generate a spirit of entrepreneurship.

In his summary paper Hoselitz produces an evolutionary model. A nation tends to pass through three phases: (1) achieving solidarity (a national unity); (2) attaining its goals (government ownership or regulation of industry), and (3) adapting itself to the world it has created. Then in three pages he compresses some 2000 years of Western civilization's history into this framework. Further on he explains what produced the few great waves of entrepreneurial vigor in European economic history: entrepreneurs appeared when they had freedom to work out their own solutions.

In a review of all the papers and conclusions Joseph Spengler, ever provocative, says that Hoselitz's first model predicts nothing and so has limited value. He also asks whether the experiences of the Western world can point the way for underdeveloped Eastern nations whose cultures may cause them to react differently.

Only rarely do the writers ask to what degree the tyranny of the state has eventually improved the standards of living. This would seem to be the fundamental measure of success. The book also suggests that an autonomous economy, such as ours, is the exception and the induced one is more normal; too,

that an induced economy may be needed to put a nation into a condition to grow, but it takes autonomy to produce much growth.

This book has exciting ideas, once one penetrates the abstractions and learns the jargon; but the historical support for many of its theories is often too meagre to convince the historian. It is nevertheless a very worth-while pioneering project, and deserves a better index.

DONALD L. KEMMERER

University of Illinois

Formação econômica do Brasil. By CELSO FURTADO. Rio de Janeiro: Fundo de Cultura Editora, S.A., 1959. Pp. 291. Cr\$220,00.

This study reviews the economic growth and processes of Brazil as an underdeveloped country from the colonial era to 1958 to provide the Brazilian lay reader with an easily readable economic history. Trends of recent years are related back to early roots in Brazil's traditions and institutions involving her coffee-sugar-cotton-cacao-mining-livestock-subsistence economies. For the English-speaking economist the book's main interest probably lies in its assessment of the influence on Brazil's economic growth and capital formation of various factors: the political, social, geographical, fiscal, monetary and others.

The first 7 chapters treat the colonial era; 8 through 15, the 16th and 17th century slave-based agricultural system of the tropical area, and the 18th century slave-based mining economy of the central part of the country; and chapters 16 through 29, 19th century conversion to a wage economy. Chapters 30 through 36 analyze the emergence of industrialization in the 20th century and venture some forecasts. Most of these last seven are based on the author's *A Economia Brasileira*, published in 1954.

A recurring theme of the book is the export-agricultural interests' almost uninterrupted control, since Brazil's independence and well into the first half of this century, of economic policy whenever they desired to influence it. Even with a shift of governmental power to Brazil's subsistence South, in the third decade, economic regulations continued favorable to the export-agricultural interests.

The author implies that capital formation in the country was handicapped, after independence, in comparison with that in the United States, by Brazilian governments' persistent deficit-financing without the accompanying purchase of government securities by private investors, whereas our own investors' willingness to purchase U.S. Government securities was an important factor in developing our own capital flows. Nevertheless he supports the further dose of inflation in the first half of the 20th century embodied in the coffee valorization of the 'thirties. He admits that both this, and the later selective exchange controls, followed the historical pattern of shifting onto the general public financial disadvantages which would otherwise have accrued to the historically privileged export-agriculture group and its accumulated Brazilian capital. However, he concludes that formation of Brazil's present private industrial capital was made possible by (1) the stimulus of coffee valorization, which through its multiplier effects brought Brazil out of the depression well before other countries and, especially, before the undeveloped ones which followed

orthodox policies; and (2) the preferential exchange rates granted in the 'forties for machinery and other producers' goods, making investment in industrial capacity doubly attractive as a result of (a) the prohibitive pricing of imported consumer goods and (b) the presence of purchasing power for domestic products in the hands of a wage-earning class. Thus, a timely application of further inflation is shown as one of the two main factors contributing to the growth of Brazil's current industrial capital. He does add that prolonged inflation, continuously adding to the entrepreneur's share of income, would eventually result in disinvestment.

Probably the prime aspect of his treatise meriting careful consideration by economists of the more developed countries is its reflection of the tremendous politico-economic insistence of undeveloped countries in these decades on (a) seeking rapid growth regardless of cost, and (b) using to this end methods considered unorthodox. As Chief of the Development Division of CEPAL (Economic Commission for Latin America) and in other international posts, Furtado has had important sounding-boards from which to advance his views. He is obviously well versed in the new economics, within the framework of which his analyses have evolved. For the first three decades of this century, he says, Brazil's unchanging confidence in the monetary rules followed in Europe resulted in applying to an underdeveloped country principles divorced from reality.

The reviewer is impressed by the author's ingenuity in making statistical estimates from existing data with their typical lacunae for the earlier years especially. The estimates are conscientiously explained, even at some sacrifice of readability. The detailing of sources in footnotes adds to the impression of care. In footnote 49, page 57, giving Simonsen one of the many acknowledgments for use of data, the author states he does not always follow Simonsen's practice of using the more conservative of the data available. He shows moderation in considering possible restraints on Brazilian industrialization under the early 19th century treaty with Britain, when he indicates that restraining effects of low Brazilian tariffs stipulated by the treaty were much more than offset by prompt Brazilian exchange depreciation.

Whether an economist leans towards orthodox or newer economic theories, this study can greatly add to his understanding of the forces at work in our historically friendly neighbor to the far south, so territorially and culturally ample that the noise of expanding industry is in no danger of detracting from the cherished cadences of the song of the *sabiá*.

ALLEN H. LESTER

New York City

Die gegenwärtige Aussenhandelsverflechtung der Sowjetischen Besatzungszone Deutschlands. By ERICH KLINKMÜLLER. Osteuropa Institut, Freien Universität Berlin. Berlin: Duncker & Humblot, 1959. Pp. ix, 196. DM 25.80.

The darkness that covers some of the principal economic facts of East Germany results in part from the willful veiling of price, income, trade and payments data by the local regime. But it is also due to a lack of incisive efforts

of Western economic analysts close to the scene to study what can be learned from official East German and Soviet bloc sources or from direct observation. Considering the strong concern of West Germans with the division of the country by the iron curtain, and the impact of the Soviet-sponsored "D.D.R." (the so-called German Democratic Republic) on their lives, one might expect that more West German economists would bother to analyze that economy systematically.

Erich Klinkmüller, a young economist at the Osteuropa Institut of the Free University of Berlin, has made the first comprehensive analysis of the foreign-trade system of Soviet-occupied Germany. The book offers a statistical study of East German foreign economic relations, particularly with the Soviet Union and the Soviet bloc, and it discusses various theoretical and institutional aspects of these relations, such as the problems of foreign trade planning, the dichotomy of internal and foreign trade prices, the dominant role of the Soviet Union in East German economic policy, bilateralism and economic coordination within the bloc. The empirical work is on the whole competently done, and the theoretical discussion, while sometimes hesitant and a little blurred, is intelligent and well linked to the international literature. This monograph is one of the best studies of the East German economy that has appeared.

Klinkmüller recognizes in East Germany's external economic relations the interplay of several partly cooperating, partly conflicting factors: the resource endowment and structure which call for intensive foreign trade in manufactured goods (exports) and basic materials (imports); the dominant role of the USSR in rerouting the area's trade and regulating its composition and procedures; the imposition of a command economy with its hankering for total control and its tendency to autarky. He shows how these factors combine to create problems of rationality in the conduct of foreign trade, such as uncertainty as to comparative costs (due to arbitrary domestic prices in East Germany and communist partner countries), and manipulation of prices and financial arrangements to the advantage of the USSR. Klinkmüller also describes some of the peculiar instabilities of the planned trade of the Soviet Zone, e.g., the "planning seasonal" in industry and trade, with its typical fourth-quarter peak and first-quarter trough, which reflects the all-out production and delivery campaigns toward the end, and the frictions at the beginning, of the annual plan periods. He also discusses the rigidities of trade among the communist countries and points to the possible contradictions between the efforts to coordinate industries in several bloc countries on a product-by-product basis—under currency and price conditions that make it hard to test the rationality of the division of labor—and the consequences of closed national economies trading among themselves with a minimum of multi-lateral settlement facilities. These contradictions may give rise to balance-of-payments problems.

East Germany, like the other bloc countries, prefers to keep its balance of payments a closely guarded state secret, so much so that some financial transactions with the Soviet Union may be known only at the apex of the party hierarchy. Klinkmüller makes an attempt to estimate the balance of payments

for recent years, but the result is at best a rough first approximation. The meaning of the officially published commodity trade values is obscure for a number of reasons, among them the changing treatment of uranium exports and the veiling of armament imports; data on the service and capital accounts are fragmentary; the transactions with the Soviet forces in Germany are left in the dark. The author seems to have been unaware of the fact that Soviet-East German commodity trade, according to the latest Soviet sources, totals up quite differently, for 1955 through 1957, from the figures published in East German sources. For 1957 especially the differences are sizable. The detailed Soviet publication shows a Soviet export surplus of 400 million rubles, the East German Yearbook an East German export surplus of 300 million rubles. Both figures are computed on an f.o.b.-frontier-of-exporting-country basis. A good deal remains to be explained before the balance of payments of this area can be understood, and the full story is likely to be a fascinating one.

The "price equalization" system, by which East Germany covers the discrepancy between its trade balance at internal and external prices, is well described, although perhaps not in the detail necessary to understand the operation fully. The author points out that the net equalization payments out of the state budget to domestic enterprises represent a charge (downward correction) on the available national product. The statistical exercise, however, that seeks to express this process numerically, suffers from at least one major blemish. The estimates of foreign trade at domestic prices (Table 6) command little confidence for the later years; the assumption that the average price equalization factor on the import side has remained constant since 1951 seems quite arbitrary, and the variations of the factor on the export side are given without any explanation. (The first two lines in Table 6, incidentally, seem to show these factors in the wrong places.) Also, the net price equalization payments for 1955 and 1956, based on figures published in the West German Statistical Yearbook, run much higher than the figures shown in the ECE *Economic Survey of Europe, 1957* (Ch. 6, p. 28). A reconciliation of the estimates would be desirable. Klinkmüller may also be criticized for reproducing somewhat lightheartedly the East German official national product figures. These figures are governed by a variety of peculiar definitions and practices and hardly deserve being shown, without adjustments, side by side with the West German national product statistics.

The uncertainty of the price equalization figures, taken together with the lack of allowance for the changing treatment of uranium in the foreign trade statistics, cast some doubt on Klinkmüller's conclusion (p. 41) with regard to the development of the terms of trade of the area from 1955 to 1957. The terms of trade of East Germany may indeed have improved significantly in that period simply because the Soviet Union agreed to pay for uranium shipments. An analysis of Soviet foreign trade statistics (*Rev. Econ. Stat.*, May 1959) leads the present reviewer to suspect that the terms governing other East German trade with the Soviet Union also improved somewhat during that period but that price disadvantages on imports from the Soviet Union were not entirely removed. The main import commodities, incidentally, for which these price disadvantages can be found (grains, cotton, coal and fats), were

never affected by the Western embargo on strategic exports to the bloc. One may therefore doubt that the embargo added significantly to the bargaining weakness of the occupied area vis-à-vis the Soviet Union in the early 1950's, as Klinkmüller seems to think.

The breakdowns of East German foreign trade by commodities and countries and the matrixes of intra-bloc trade presented in the monograph may be of interest to students of international trade and Soviet economics. They bring together most of the available statistics on East Germany's considerable trade with the Soviet realm, with the exception of the detailed data on Soviet-East German trade published by the Soviet Union in 1958. Trade with Western countries other than West Germany is covered less exhaustively. In a final section, the author adds East and West German foreign trade together and looks at the distribution of the fictitious aggregate over the various countries. He finds that, compared with before the second world war, East Germany's share in "total" German foreign trade has fallen, and that the share of Eastern Europe in the "total" is about the same as in 1936.

One may hope that this informative and straightforward monograph will stimulate further studies of the East German economy.

HORST MENDERSHAUSEN

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The Growth Rate of the Japanese Economy since 1878. By KAZUSHI OHKAWA AND ASSOCIATES. Tokyo: Kinokuniya Bookstore Co., 1957. Pp. xvii, 250. \$6.00

This small book, presenting and discussing in detail new long-term national income figures for Japan, is a translation, revised and expanded, of a 1955 report which won the Mainichi newspaper prize. The authors modestly regard the present study as only an interim statement in their planned revision of Japanese historical statistics.

A few noteworthy predecessors in Japanese national income research are: Nakamura's 1902 estimate for the year 1900; the first government (CBS) estimate in 1928 for the year 1925, extended back to 1887; Hijikata's publication in 1933 of a series for 1919-1930, extrapolating 1919 figures back to 1900; and Yamada's book (1st ed., 1951) giving an 1875 to 1942 or later, series. Since the second world war there has been an official (EPB) yearly estimate, which has been run back to the 1930's. The present study's national-income-produced series (for 1878-1942) puts a fair amount of emphasis procedurally on the period from 1878 to 1918-22, since after 1922 or so "the differences found among various estimates are rather slight" (p. 35).

The national income estimates made by CBS (1928) for the period before and following 1900 were very low and hence the rise was steep thereafter. Hijikata in 1933 lifted the estimates for the first decade of this century somewhat and although this lessened the rise, he still left it a remarkable one. One effect of Yamada's decisions in his 1951 work was to jump the pre-1922 figures well above practically all previous estimates, sharply cutting the growth rate indicated. What Ohkawa's group does now is to lower Yamada's

pre-1922 figures part way toward the old Hijikata series, restoring a little of the steepness to the rise and leaving a smoother slope. In effect then, Ohkawa makes a conservative restatement of Yamada. Growth rates for pre-1922 still remain very high, even if considerably less sensational than in the CBS series of 1928.

The present study suggests that a "main part of the difference between" its aggregate income series and that of Yamada lies in different estimates for the tertiary sector (p. 115). Estimates are made for the primary sector (agriculture, etc.), the secondary (manufacturing, etc.), and the tertiary (a residual category covering not only communication, transportation, commerce and government, but out of statistical necessity, also government manufacturing, and private building and construction).

Especially for the tertiary sector and for that sector before 1918, Ohkawa's estimates are necessarily indirect and somewhat arbitrary. A few details will show this. Ohkawa gets figures for the pre-1918 tertiary sector by assuming such figures to be a fixed proportion of similar figures in the goods-producing sector (that is, primary plus secondary). He finds his proportion in the 1921-1929 period as a base. Since he has 1878-1918 yearly estimates for the goods-producing sector on per capita income and on wages, he can use his fixed 1921-29 proportion to work out a yearly per capita figure for the tertiary sector, 1878-1918. Then to get tertiary yearly aggregates, he multiplies the tertiary per capita figures by the number of gainfully occupied in the tertiary sector each year, 1878-1918.

Although widely used in this study, "gainfully occupied" is, for Japan before 1950, a statistical concept open to serious criticism, as the authors realize (pp. 142 ff., p. 34 fn.). The concept reflects, for example, usual status not actual employment.

The paragraphs above are intended to convey something of the impression acquired by this reader that Ohkawa's improvements on the Yamada series cannot be completely separated from the preconceived ideas of the Ohkawa group on what would be a reasonable result.

Part I of the book gives the new national income series for 1878-1942, deflated by a somewhat new wholesale price index, and broken down into three sectors. For the aggregate product (1878-1942) the modal growth rate of real income per gainfully occupied person is said to be a little more than 3 per cent (p. 23). An appendix on "Economic Growth and Capital Formation in the Post-War Period" indicates that the rate for 1952-55 is 3.3 per cent (p. 239).

Part I also has a brief, interesting section on the relation of previous estimates to the present series. Part II gives in detail the procedures followed in measuring each sector, discusses various deflators considered before choosing the one used, and surveys Japanese statistics on population.

Part III treats of the measurement of capital formation. It consists of three papers independent of the rest of the book. Ito works out an approach employing national wealth data. Shinohara uses the commodity-flow method, after Kuznets; and there is a report on the possibilities of using capital coefficients by individual industries.

On page 122 in Table 2, showing the price index used, column 2 seems to be intended as a simple five-year average, compiled from Table 6 (p. 130). But, if so, the first two figures, 41.7 and 31.6, should be 46.3 and 33.8. This error also affects the first two figures given for Yamada, Table 2 (p. 36). Similarly, in Table 2 (p. 122), "our" index for 1918-22 should be 158.4 not 150.4. This error also affects the Yamada, Hijikata, and CBS figures for 1918-22, Table 2 (p. 36).

MICHAEL O'CONNOR

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The Agrarian Origins of Modern Japan. By THOMAS C. SMITH. Stanford: Stanford University Press, 1959. Pp. xiv, 250. \$5.00.

Unraveling the agrarian origins of modern Japan is a monumental task which would occupy more than a lifetime of any single scholar in the field. The immensity of the task is due partly to the varieties of strains observed in the Japanese feudal society and partly to the paucity of documentary materials of the pre-Restoration (1868) period. T. C. Smith has addressed himself to this task apparently with full realization of the pioneering character of the research involved and has come out, in a relatively short time, with a commendable result, neatly organizing the thus-far available studies by Japanese scholars, in particular those of Ariga and Furushima. Smith's interest seems to have lain especially in tracing the intricate course of change in Japan which produced the present-day landlord-tenant relations from the social relations of owner-cultivated farm units of the feudal period. For this purpose he first analyzes empirically the farm organization (the land system, labor services, the organization of political power, etc.) of the traditional village. What is characteristic here is a system of extended family which permits a fairly large-scale unit-operation on a cooperative basis and which might be visualized as composed of three concentric circles of the nuclear family, affiliated relatives, and a servile group. The hierarchy of authority based upon such a system of socio-economic organization is said to have given the traditional Japanese village "its fierce and pervasive sense of solidarity." What shattered, albeit gradually, this pastoral tranquility was, as usual, (1) the growth of the market, with its impersonal logic of money economy, and (2) the development of agricultural techniques which, paradoxically enough, acted as a stimulus to the smaller farming unit. These two factors together brought about the disintegration of the cooperative farming of the extended family and gradually helped crystallize a new class relation with which tenants in the modern sense of the term emerged.

This, in broad outline, is the picture which Smith depicts with unusual clarity, drawing in the process upon extensive empirical research done by Japanese scholars as well as upon his own original research on the particular problem of political conflict in the village in transition. His interest in the problem of modern Japan (1868-), it may be surmised, must have led him inevitably to its agrarian origins in the Tokugawa period (1603-1867). But such "origins" have in turn their origins in the antecedent period; and if at all, Smith's weakness appears to lie in leaving some retrospective loose ends in

his analysis. For example, the dominant characteristic of the class relations of the Tokugawa period was that the warrior class lived on the surplus of the agricultural-producing class *without* any intermediary group of persons. Exceptions to this rule were observed notably in the *Kinai* district where the old manorial system (*shō-en*) lingered on the longest and where manorial masters, instead of transforming themselves into a warrior class, continued as large-scale landowners appropriating a part of the surplus to themselves. Smith draws a large part of his empirical evidence from the Tokugawa record of this *Kinai* district and often goes on to reason as if that district were typical.

In general, his method of marshalling historical facts for his case tends to be somewhat "particularistic" in the sense that each of the historical elements in the situation is studied separately. Thus, the facts of even recent date are brought in to support a certain point essentially germane to an earlier period (see pp. 41, 47, 57, 156, 169). Also, it appears that not enough attention is paid to the integral institutional phenomena of the Japanese feudal society, such as the *sankin-kōtai* system (which certainly played a major part in stimulating the spread of the money economy), the "five-family group" system (*go-nin-gumi*), the development of merchant capital in the late Tokugawa period, etc. The use of Japanese terms in the text is also too bold, at least to my taste, often stretching too far a specific category to cover too wide a field, e.g., *nago*, *mutaka*, *oyakata*, etc.

In spite of these shortcomings, however, the main contention of the book is, I believe, broadly correct, and furthermore is closely argued with detailed empirical support. The author and the publisher are highly to be commended, too, for the remarkable lack of misprints of Japanese spelling except one mistake where a feminine name (Kazuko Tsurumi) is represented by a masculine pronoun (p. 64).

SHIGETO TSURU

Hitotsubashi University

Economic Policy Revolution and Industrialization in Latin America. By PEDRO C. M. TEICHERT. University, Miss.: Bureau of Business Research, University of Mississippi, 1959. Pp. xviii, 282. \$8.00.

The author of this book attempts to study the forces in Latin America that are allegedly pushing it "towards an unprecedented era of planned development and industrialization" (p. iii). Though his focus is the economic policies (protectionism and planning, for example) that are regarded as the basis of the new industrial society that he claims is in the making, he covers much historical ground in showing how past revolutions and wise, up-to-date policies have removed institutional obstacles to growth. He has a most exuberant animal faith in the prospects of Latin America that he describes as "an area that easily might become one of the economically most important and powerful regions of the world" (p. iv). This is questionable, of course, especially while Argentina and Chile continue in the doldrums and Brazil's and even Mexico's rate of growth go down perceptibly.

Professor Teichert swallows whole, and in the crudest form, many economic

notions that are the fashion today in some Latin-American circles. Everything that smacks of *laissez-faire* orthodoxy, for instance, is to be rejected, and indeed "the more underdeveloped the area, the more unorthodox would be the economic policies pursued in an attempt to change its destiny for the better" (p. 39). It is a wonderfully simple guide to economic policy that puts statistics on the comparative level of development in the place of good sense. But he is emphatic in rejecting traditional economics (p. 233):

The second point is concerned with the fact that Anglo-Saxon policies directed towards the maximization of profits in monetary terms are not always the primary goal of the Latin Americans. It is instead a broader maximization of life enjoyment the Latin Americans are after, including the satisfaction of their spiritual and intellectual wants.

The application of marginal and comparative cost analysis as developed by classical economics is, therefore, not always appropriate. . . .

This also in part explains the continuous drive to industrialize even though manufactured goods could be imported much more cheaply from abroad. But it is for instance the realization that the multiplier only works in closed economies, that made it almost mandatory for Latin America to develop basic industries, particularly steel mills, at any cost.

The author also pays due respect to "balanced development" that he interprets to mean to "move ahead simultaneously on all fronts—increasing food production, export production, industrialization and resource development to the same degree" (p. 123). Why to the same degree for each and every country is not explained.

Though light on ideas, the present study is heavy on facts, many of them given with little apparent purpose rather than to sustain theses, especially the self-designated core of the study, the "theory of the peripheral economy," that is given this omnibus definition (p. 199):

It is based on the interrelation of technical problems, savings, investments and capital formation; naturally it stresses the institutional factors involved in the development process. The theory proposes measures by which to avoid the high fluctuations to which the under-developed, mono-cultural export economies are especially subject.

To offer prescriptions is really a big jump for a theory that never was anything but a presumptuous name for the simple but resented fact of dependence of the raw material producer upon the manufacturing country.

The bulk of the book is made up of loosely connected narratives of economic history, particularly on Uruguay and Mexico, the first country being given the curious role of a leader in economic growth. The bibliographic references seem disproportionately abundant for the little light the study casts. In general, the book misses the goal of bringing onto common ground the Latin-American that is always complaining and the American that is always scolding.

THEODORE A. SUMBERG

New York, N.Y.

Ukraine and Russia: A History of the Economic Relations between Ukraine and Russia (1654-1917). By KONSTANTYN KONONENKO. Marquette Slavic Studies, IV. Milwaukee: Marquette University Press, 1958. Pp. xv, 274. \$7.50.

Numerous studies treat the prerevolutionary Russian state as one economic entity. Such an approach blurs the peculiarities of the economic past of areas into which Russia has expanded in modern times, and leaves no room for an analysis of the interaction between Russia proper and the newly acquired territories. Professor Kononenko's work, which interprets the economic relations between Ukraine and Russia, is therefore an important contribution.

The thesis of the book is clear: Having fallen under the political control of Russia in the late seventeenth century, Ukraine's economy became subject to Russian interests. Russia succeeded in transferring considerable "surpluses of wealth" from Ukraine by imposing tariffs discriminatory to Ukrainian industry, shipping, and commerce; by forcing a market dependence on Russia through direct control of industry, building a Moscow-oriented communications system, etc.; by extracting wealth from Ukraine through taxation and control of capital, yet spending much less in Ukraine itself.

This is by no means a new interpretation, and it will even be found in Soviet publications, though in an inhibited form. Kononenko's book, however, treats the topic intensively, and endeavors to support the thesis by statistical data on diverse phases of economic life. He is aware of the pitfalls of a narrow economic interpretation of the causes and nature of imperialism, and is therefore satisfied with presenting the evidence that the surplus production of one national economy (Ukraine) had been appropriated by another (Russia) through military and political superiority. Some of the tables, we note, refer to fragments of the economy or to short spans of time, so that inferences drawn from them are not convincing. The book as a whole is argumentative, and looks at the historical evidence from the standpoint of the thesis; this, in the opinion of the reviewer, prevents a balanced over-all analysis.

The value of the monograph to the historian would seem to be primarily that it identifies the peculiarities of Ukraine's development of commerce, agriculture, and industry in the stated period. This, together with the discussion of the economic relations of Ukraine with Russia in all phases of economic life, helps to identify the forces which shaped Ukraine into an important industrial country on the continent of Europe. The reader will find the volume a revealing introduction to understanding the position of Ukraine in the sphere of Russian domination.

V. N. BANDERA

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Economic Systems; Planning and Reform; Cooperation

A History of Socialist Thought. Vol. IV (2 parts): *Communism and Social Democracy, 1914-1931*. By G. D. H. COLE. New York: St. Martin's Press; London: Macmillan, 1958. Pp. x, 455; viii, 483. \$14.50.

Volume IV of G. D. H. Cole's *History of Socialist Thought* was completed at the end of 1957, little more than a year before his death in February 1959.

He had planned at least one more volume carrying the story down to the end of the second world war and possibly beyond, but I do not know whether he left any of this projected final volume in publishable form. Whatever may be the case in this regard, it is a pleasure to be able to report that Volume IV, which is divided into two parts, each between its own covers, brings the story to a logically satisfying stopping point. As it stands, this broadly conceived and splendidly executed history makes a fitting monument to one of the outstanding economists and social historians of our time.

I reviewed the first three volumes of *A History of Socialist Thought* in an earlier issue of this journal (December 1957) and will not repeat what was said there about the general plan and character of the work. Suffice it to say that Volume IV continues the pattern set in Volume III (which was also in two parts) of organizing the subject matter on a country-by-country basis, with a number of interspersed chapters on international aspects of the socialist movement, plus a concluding essay on the nature of the relationship between communism and social democracy in the years between the outbreak of the first world war and the depths of the great depression. Moreover, like the preceding volume the one before us is both more and less than a history of socialist *thought*. It would perhaps be more accurate to call it a history of important labor and revolutionary movements of the world during the period covered. In the case of some countries, this requires a good deal of attention to the clash and development of ideas, while in the case of others the chronicle of events is unrelieved by manifestations of intellectual originality or independence. (Unfortunately the United States falls in the latter category, so that the single chapter devoted to this country and Canada is in the main a rather dreary recital of factual quarrels, splits, and maneuvers. The interesting—and I think also important—question which Cole raised in Volume III, namely, why a rapidly growing U.S. socialist movement went into a long-term decline after 1912, is not reopened in the present volume. Judging from talks I had with him after my earlier review, I think it is quite likely that he would have returned to this problem if he had lived to complete the work.¹)

There is, of course, no possibility in a brief review of evaluating or criticizing the more or less separate national histories which comprise this volume. They are inevitably of varying quality—best and most absorbing in the case of the European countries which Cole had closely observed over the years, less satisfying in the case of other countries which he knew only or largely through library research. One thing, however, all the chapters have in common: in respect to each national movement, Cole has attempted to set down the relevant factual highlights clearly, concisely, and accurately. So far as my limited knowledge permits me to judge, he has been remarkably successful in doing so. I believe that the student who needs a reliable guide to socialist movements anywhere in the world will find what he is looking for in Cole's *History*. And even the specialist will find much of value—not least the lists of

¹ I would like to take this occasion to make a correction in the earlier review. There I said that it seemed obvious that Cole had "A great respect and liking for Marx." He was at pains to make clear that while he certainly respected Marx he very definitely had no liking for him.

socialist leaders with carefully checked birth and death dates, and the 20 pages of selected material at the end of Part II.

It is no bibliographical criticism to say that Cole's interpretations of his material—as distinct from his presentation of the facts—will not meet with general acceptance. He was himself a dedicated socialist, which explains why he cared so much for his subject and understood it so much better than an unengaged scholar possibly could have; but within the movement he stood aloof from all parties and factions, keeping his eyes steadily fixed on ultimate ends, encouraging everything he thought contributed to their attainment, and unsparingly criticizing whatever he thought incompatible with them. Such a person can command more respect than agreement, and so it was with G. D. H. Cole. Non-socialists tend to consider him a dangerous radical, communists to dismiss him as a half-way revolutionary, and social democrats (a category which includes most of the British Labour Party) to suspect him as a fellow traveler. They are all quite justified in a way, and yet the more intelligent among them must know that they are also quite wrong. At bottom Cole was that paradox on which perhaps the future of the world depends, the individualist who believes in the desirability and indeed the inevitability of collectivism. His *History of Socialist Thought* reflects the man—in its honesty, its insights, and its deficiencies.

PAUL M. SWEETZ

Cambridge, Mass.

Planning in Norway 1947-1956. By P. J. BJERVE. Amsterdam: North Holland Publishing Company. Pp. xi, 384. \$9.25.

Norway is one of the few Western countries where the preparation of national economic plans plays an important role in policy formation. The planning is detailed and is taken seriously by government administrators. Consequently, an evaluation of this country's experience could be useful indeed. To date, however, serious evaluation has been hampered by the lack of comparable *ex ante* and *ex post* data. Differences in accounting procedures and definitions, inadequate price and volume indexes, and inadequate knowledge of margins of error in the accounting figures have combined to make difficult the massive task of comparing projections and plans with actual results. Petter Jacob Bjerne is in an excellent position to give us the necessary comparable data and to undertake a comprehensive evaluation. Not only is he presently the director of the Norwegian Central Bureau of Statistics, but his role in establishing this planning system gives him an intimate knowledge of its operations. Bjerne has done a painstakingly thorough and detailed job of presenting the necessary data. His evaluation, unfortunately, although of considerable interest, is not as complete as one could wish.

To a large extent the incompleteness of the evaluation stems from Bjerne's limited purposes. As he makes clear, the book does not attempt to evaluate economic policy *per se*. It does not directly attempt to analyze the consistency of the goals, the relevance of the means, or the efficiency of the planning system. Instead, over two-thirds of the book is devoted to the twin tasks of, first,

presenting comparable *ex ante* and *ex post* figures, and second, explaining the deviations that occur between them. The explanations reflect different levels of analysis, starting with statistical observations and ending with hypotheses about administrative behavior patterns. Examples of the former include explanations of the varying degrees of inaccuracy of different projections, and recognition of the fact that preliminary accounting data underestimate changes from the previous year; examples of the latter include the hypotheses that superiors do not take into account the way subordinates actually carry out orders, and that there is a tendency to underestimate changes because of greater risks attached to overoptimistic estimates. With regard to the statistical observations, evidence is presented in great detail; with regard to the observations about behavior patterns, we must rely on Bjerve's judgment, since evidence from unpublished government records and interviews is not presented.

One of the procedures used on the statistical level of analysis seems of questionable value in the Norwegian context, although quite interesting. This is the use of a numerical model to separate errors arising because of bad forecasts of exogenous variables (e.g., export prices) from other sources of error (e.g., effects of changes in exogenous on endogenous variables). If a complete numerical model were available and if it were actually used to formulate the projections and plans, this separation of errors could be accomplished by resolving the equation system, using realized values for the exogenous variables and noting the degree of improvement in the results. The Norwegian national budgeting system does not rely upon an explicit econometric model. Rather, it relies on estimates submitted by strategically located specialists in the government administration, and the coordination of these estimates by a trial-and-error process involving liberal use of the telephone and the definitional relations of the national accounts. Since it is not possible to ask the administrators what they would have done had they known the actual values for the exogenous variables, Bjerve uses a simple model (three equations and five variables) which he hopes correctly simulates their behavior.

Actually, the variables assumed to be exogenous in the model (exports and investment) are not completely so, nor are they so treated by the planners. This makes it difficult to say which is cause and which is effect in "explaining" the deviations in one variable by reference to another. Although Bjerve admits this and cautions the reader that he must not place too much weight on results derived solely from the model, he does conclude, primarily by reference to the model, that unrealistic assumptions concerning exports and investments account for a large part of the errors in forecasting gross national product, imports and consumption. The procedure would certainly be justifiable and useful if, *inter alia*, the exogenous variables of the model were in reality treated as such, but considering the qualifications necessary in this case, its value is somewhat questionable.

The final chapter, entitled "An Appraisal," is different in character from the other chapters and does not depend heavily on the previous 200 pages of detailed analysis. Bjerve asks a series of questions—for example, "Is National Budgeting Used as an Important Tool of Economic Policy?" "Are Improve-

ments in the Present National Budgeting Methods Feasible?" "Is National Budgeting Useful in an Economy without Direct Quantitative Controls?" "Have Major Post-War Economic Goals Been Achieved?"—and answers them all in the affirmative. This chapter is interesting because it considers some non-statistical aspects of the planning system and presents the author's personal attitudes about them. It is not, however, a summary of conclusions derived primarily from the statistical material presented earlier.

The value of this book lies first and foremost in the presentation of comparable *ex ante* and *ex post* data that can be used by others to make further analyses and international comparisons. Apart from this, it is of interest because of the light thrown on such scattered facets of planning and control as the effects of publishing plans and estimates as compared with keeping them secret, the way in which licensing officials influence planners, and the value of this planning procedure for the education of public officials, all of which are suggested by Bjerve's comments on the statistics. The book is hard reading, but for a statistical record of one country's experience in attempting to forecast and plan, and for discerning comments on the nature of the planning system and the behavior of its planners, it is worth the struggle.

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Business Fluctuations

Business Conditions Analysis. By JOHN P. LEWIS. New York: McGraw-Hill, 1959. Pp. xii, 602. \$8.25.

Business Conditions Analysis covers five general subject areas. Part I is a straightforward, short and informed introduction to national income accounting. Part II is a more extended treatment of the theory of aggregate income, including a discussion of the role of money in the economy. Part III is a very brief account of business conditions since 1930. Part IV is an exposition of short-run business forecasting. Part V describes the complexities of long-term forecasting and the long-run outlook for pricing and pricing practices.

The task outlined in the above paragraph is ambitious, especially when directed toward an audience "whose preparation in economics does not necessarily extend beyond the usual two semester introductory sequence." The general reader will be treated to a number of revealing insights into the operation of the economic system and will be exposed, possibly prematurely, to many of the ideas, concepts and controversies in the forefront of contemporary economics. In comparison with the typical text on business forecasting, the book is pitched at a fairly high theoretical level and is written in the belief that the would-be analyst must first master the formal apparatus of macroeconomic theory before business conditions analysis becomes a very meaningful undertaking. The analysis is essentially neo-Keynesian in its orientation, even though throughout the book the author displays a deprecating attitude toward Keynesian economics.

In Part IV the author follows what have become in recent years fairly conventional methods in what he variously describes as "opportunistic model

building," "short-run outlook analysis" or more simply as business forecasting. There is a chapter on government spending with appropriate remarks on budget concepts. Projections of plant and equipment expenditures are discussed in terms of the McGraw-Hill and Commerce-SEC surveys of businessmen's intentions—alternative analytical approaches are not explored. The demand for housing is analyzed in terms of the number of households, income, finance, costs, etc. Consumer durables are discussed with the automobile market analyzed separately. Nondurable and service expenditures, on the basis of a regression analysis, are assumed to "absorb about four-fifths of disposable personal income." Inventory projections lean heavily upon inventory-sales relationships tempered by considerations associated with the general inventory cycle.

The last chapter in this section is devoted to completing the short-run forecast. An initial forecast is made of GNP. Demand and capacity estimates are compared. Suggestions are made for checking the internal consistency of the forecast and for reconciling the various projections. Finally, several pages are devoted to estimating employment, prices and profits.

There are a number of limitations: The work on national income accounting, although well done, suffers somewhat from the fact that the most recent revision of the national income accounts is not incorporated in the text. The section on aggregative economics, attempting to cover in a matter of 200 pages a very sizable portion of macroeconomic theory at a fairly sophisticated level, leaves the reader somewhat breathless. The historical section is descriptive rather than analytical. Brevity alone precludes very effective use of the historical material to support or illuminate various hypotheses and theoretical positions advanced elsewhere in the book. For the do-it-yourself forecaster the material on "short-run outlook analysis," although excellent, will require a good deal of improvisation and research before forecasting skills of a practical nature become operational. Forecasting at the industry or firm level is not discussed.

The final chapter dealing with long-run pricing problems, while written with conviction, is highly speculative. Curiously enough, the author in discussing at some length the secular trend in the price level seems less daunted by the uncertainties of the long than of the short run. He writes: "Nothing is certain in this world, but the evidence and arguments supporting the third major count in the indictment against contemporary American price practice—namely, that it has injected a persistent inflationary bias into our economy—seems to me overwhelming. . ." While this may be true, the difficulty of obtaining convincing evidence concerning the probability of occurrence of events far distant in time is overwhelming also—seeming the more so, perhaps, because of the uncertainties and lack of agreement concerning the causes and relative importance of various factors in the *current* business situation.

Just as there is no one "best" automobile for all consumers, so there is no one "best" book for business conditions analysis. The book is admirably suited for those who want to study in a single book economic forecasting, national income accounting and the formal theory of income determination. For those who have the background and the desire to penetrate deeply into the subject

matter of economic forecasting, for those who want to explore in detail the relationship between economic theory and reality and to emerge with skills which are operationally useful, the book is, as indicated by the author, an introduction.

DONALD W. PADEN

University of Illinois

**Money, Credit and Banking; Monetary Policy;
Consumer Finance; Mortgage Credit**

Zur Theorie des Konsumentencredits. (Theory of Consumer Credit.) By STEGFRIED SCHIMANSKI. Tübingen: J. C. B. Mohr (Paul Siebeck), 1958. Pp. 144. DM 8,90.

This study is concerned primarily with a systematic theoretical analysis of the relationship between consumer credit (mostly instalment credit) and the national income. The analysis culminates in a series of economic models, which take the form of a number of equations, supplemented by many tables and diagrams. These economic models show, under certain simplifying assumptions, the effect of an expansion or contraction of consumer credit on consumption, investment, and the rate of saving. The book deals with cyclical as well as structural problems, and emphasizes the importance of consumer credit as a growth factor in the economy.

Chapter 1 contains a survey of the various forms of consumer credit, with particular reference to the institutional setup in Germany. In discussing the differences between consumer and producer credit, the author arrives at the conclusion that, because of the lesser amplitude in fluctuations of consumer income than of producer income, the risk for the supplier of consumer credit is by no means greater than for the supplier of producer credit.

In Chapter 2 the author analyzes the market for consumer credit on both the supply and demand side. This chapter contains frequent references to related studies in the field of consumer credit, including—besides a number of German studies—those by G. Haberler, D. D. Humphrey, A. Kisselgoff and F. A. Lutz. The theoretical discussion of supply conditions is supplemented by statistical data on the magnitude and the sources of consumer credit in Germany (Federal Republic) during the period 1952-1957. On the demand side, the analysis is concentrated on the relative importance of the various factors which determine the demand for consumer credit, notably the income level, the income distribution, the prices of durable consumer goods, the credit terms (including down payments, credit costs, and periods of repayment), and the amount of accumulated savings of private households.

Chapter 3 is entirely devoted to the construction of various economic models. It is not possible within a brief review to describe the various steps of the analysis in detail. A few remarks on the method of analysis must, therefore, suffice. The economic models are designed to show the influences of consumer credit on the development of an economy which at the starting point (without consumer credit) is in equilibrium. The focus is first on consumption,

and later on investment. Various alternative assumptions as to consumer behavior provide the basis for different models. An increasing number of economic variables (income, credit terms, etc.) are included as the analysis progresses; the effect of foreign trade and of the budget are left out of consideration, and there are some other simplifying assumptions.

Among the conclusions are the following: Depending on the assumptions that are made with regard to consumer behavior, consumer credit may lead either to a temporary or a permanent expansion (or contraction) of the national income. By intensifying income fluctuations, consumer credit has a destabilizing effect on the economy, and credit terms should therefore be controlled. The producer goods industries are stimulated not only indirectly, but also directly, because they frequently also produce durable consumer goods which are sold on the instalment plan. In a growing economy with a high rate of saving, the expansion of consumer credit may act as a useful aid in maintaining a high level of economic activity.

The practical importance of a theoretical study of this kind is difficult to evaluate. There are, first, some conceptual problems that come to mind. Is it, for instance, correct to assume that consumer expenditure is increased fully by the amount of new consumer credit, and reduced correspondingly by the amount of repayments? Can any universally valid assumptions be made with respect to consumer behavior? Second, there are great gaps in the statistical material on consumer instalment credit (even in the United States) which are an obstacle to the practical application of such economic models. Nevertheless, Schimanski's study of consumer credit should be of interest to European economists (for whom the bibliography on pp. 139-144 will be a valuable asset), and also to U.S. economists familiar with the earlier literature and the recent comprehensive studies of the Federal Reserve Board and the National Bureau of Economic Research in the field of consumer instalment credit.

EMIL G. SPITZER

Washington, D.C.

International Economics

Balance of Payments and Economic Growth. By JOHN M. LETICHE. New York: Harper and Brothers, 1959. Pp. xiii, 378. \$6.00.

Letiche's title, unlike that of many another book, is an accurate indication of his subject matter. The first part of the book is on the theory of the balancing of international payments and the second part is on some applications of the theory to international problems arising from economic development. Thus Letiche refreshingly belongs to the somewhat old-fashioned camp of economists that regard economic theory as a guide to the real world, with the test of theory being its usefulness in analyzing and securing solutions to real policy problems. He evidently does not regard theory as a refined intellectual game the main touchstone of which is the elegance of its solutions.

The book begins with a critical review of classical and current balance-of-payments theories. Letiche shows that for many classical writers induced

movements in income and expenditure were a key factor in the balancing process; this is not something newly discovered by modern writers. He makes a number of penetrating observations that both promote an understanding of these theories and bring out their limitations. For example, in discussing the theory of the effects of a devaluation he shows the very limited usefulness of the elasticities approach by pointing out that "to apply the Marshallian concept of elasticity, which assumes the constant purchasing power of money, and which has relevance only to incremental changes *along a demand or supply schedule*, to problems dealing for the most part with *shifts* in these schedules is misleading" (p. 67, author's emphasis). In a devaluation the cost and demand structures of different industries and different firms are affected differently; commodities may now become exportable that previously were purely home goods, or even not produced at all; import substitutes now become more profitable, etc. Estimates of the elasticities of supply of exports and demand for imports correct before a devaluation may be almost useless as a guide to the impact of a devaluation on the country's balance of payments, for one may then be on a new set of curves.

Finally, in integrating the positive contributions of the earlier theorists, Letiche analyzes the effects of devaluation on the trade balance under conditions of inflationary, deflationary, and neutral fiscal and monetary policies, at home and abroad. In his balancing theory Letiche shows how, according to the state of employment of factors and the monetary and fiscal policies pursued, the balancing mechanism may operate differently—in some cases the adjustment of the balance of payments to certain actions will operate through the income effect, in others through the price effect, and in others through both effects. Consequently, by finding a place for the aspects emphasized by the classical and by the neo-Keynesian approaches, Letiche provides a unified synthesis.

In the second half of the book, Letiche applies his theory to the world as it is—the real test—and his theory passes with high marks. He discusses, for example, what would be the impact of a devaluation on a semi-industrialized country which is a primary products exporter. This, of course, covers a large class of countries. He convincingly shows that devaluation would probably not induce a larger proportion of labor resources to move into agriculture but that it would become more feasible and more profitable to devote more labor to produce previously imported manufactured products and import-substitutes.

He makes a careful examination of dollar-shortage theories and concludes that "there appear to be no *structural* forces which have been operating *abroad* during the interwar period that provide a satisfactory *general* explanation of long-term imbalance of the industrial countries of Western Europe vis-à-vis the United States" (author's emphasis). Since this must have been written when the dollar shortage was still widely believed in, his theories stand the test of application well.

If the book goes through several editions, as it deserves, there are many rough spots where editing would be helpful. Chapter 2 on the writings of Isaac Gervaise contributes little to the general theme and could be transferred to an

appendix or dropped without much loss. On the other hand, too much valuable material is in the footnotes: too often a footnote provides illumination for the main argument or makes a point that deserves a better life than the dungeon-existence below the text. But these are strictly minor defects. In sum, Letiche has written a valuable book which should be most useful to student and policy-maker alike.

A. M. KAMARCK

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Foreign Trade and Finance. Edited by WILLIAM R. ALLEN and CLARK LEE ALLEN. New York: Macmillan Co., 1959. Pp. xii, 500. \$6.00.

This is a useful addition to the growing library of readings in specialized fields of economics. In this case there are some twenty-three articles arranged in five sections under the topic headings of international trade theory and policy issues, the balance of payments and equilibrium, the adjustment process: changes in prices and income, the rate of exchange and equilibrium, multilateralism and capital movements. Each section is preceded by an introduction consisting of a brief textbook-level discussion of concepts. Students who are sufficiently advanced to handle the reprinted articles may find parts of these introductions needlessly elementary. At the end of each section the editors give us their "commentary" consisting of explorations of technical points raised in a preceding article. My personal preference would have been for a critical essay by the editors, but perhaps this merely proves that editors can not please everyone.

About half of the articles reprinted in the book have been cut. The excerpting has been skillfully done so that the argument presented is complete; thus you do not get meaningless bits cut from longer works. There are, of course, some costs because of the lack of the full range of the original presentation, but they do not seem to have been inordinate in this case.

Like the American Economic Association's *Readings in the Theory of International Trade* this volume is meant to be "useful in the instruction of senior and graduate students." The level of technical difficulty of the articles is not high. Only one article, for example, refers to community indifference curves and in only the article on stable and unstable equilibria of the foreign exchanges (by one of the editors) is elementary algebra used. The geometry of reciprocal demand curves is used and is also carefully explained. Apart from formal techniques, however, the essays generally presume a degree of knowledge of international economics that would be unusual without a year of undergraduate work in the field.

It is hardly necessary (or feasible) to comment on the individual articles. One meets mainly familiar faces here, e.g., Machlup on concepts of the balance of payments, Triffin on central banking, Bernstein on American productivity and the dollar shortage, MacDougall on the dollar shortage, Friedman on flexible exchange rates, Hilgert's case for multilateral trade. Haberler on convertibility, and Metzler on tariffs and the terms of trade. Nurkse is the star performer with three appearances.

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The weakest section of the book is that dealing with the theory of trade and its policy implications. In the first place it is the shortest section. This is not an allocation of space that is likely to coincide with most instructors' views of the relative importance of the topic. Moreover, none of the articles develops at any length the variations in the amount of alternative formulations of a theory of trade, nor are the criteria for judging the validity of trade theories discussed.

The exception to this statement is the article by Robert W. Stevens attacking the argument of Scitovsky *et al.* that individual countries may gain (move to higher community indifference curves) by imposing a tariff. Stevens' methodological point that classical and neoclassical writers were not concerned with national (as opposed to individual) advantage is, I believe, questionable. To be sure they were generally led to a theory of the harmony of national and individual interests and of harmony between the interests of diverse nations all achieved through individualistic action. In Scitovsky's model, on the other hand, each government acts against other national governments to increase its own domestic welfare. In both cases national welfare is a proper objective of policy and national welfare is conceived of in individualistic terms—perhaps more so in modern welfare theory than in the case of Marshall.

The trade theory section also includes Karl-Erik Hansson's, "A General Theory of the System of Multilateral Trade" which is quite worthy of being reprinted but does not constitute a general theory of trade. The section does not include an evaluation of the historical relevancy of comparative advantage theory, such as J. H. Williams' classic article that appeared in the *AEA Readings*—but then there are few such articles around.

As may be gathered from the list of contributors cited above, the treatment of the monetary aspects of international economics is excellent. Here the student may drink his fill of balance-of-payments disequilibria, dollar gaps, and foreign exchange markets. The coverage of topics is good (if it is assumed that the issues in the dollar shortage literature have not been settled by events). The viewpoints expressed are all within orthodox limits. There is no Balogh here prepared to argue for discrimination or the use of controls. The sole exception to this is Frisch's matrix demonstration that the volume of trade may, under some circumstances, be greater with rather than without payments discrimination.

The two articles on international investment experience of the past (Slater and Nurkse) are welcome additions of historical discussion to an otherwise largely ahistorical collection. It is too bad, in fact, that there is not a volume of readings, empirical and theoretical, on the development of the world economy to accompany this book for seminar purposes.

There is little doubt that this book will be widely adopted for use in advanced international trade courses including, in spite of some critical comments above, the reviewer's.

ROBERT R. EDMISTER

University of Utah

The American Economic Impact on Canada. By HUGH G. J. AITKEN, and others. Durham, N.C.: Duke University Press; London: Cambridge University Press for Duke University Commonwealth-Studies Center, 1959. Pp. xviii, 176. \$4.50.

The Canadian body politic, whenever temporarily freed of the pressure of more tangible economic and social problems, grows alarmed over the extent of foreign influence upon Canada, mostly that of the United States. In the prosperous 1950's this sentiment has blossomed vigorously; and while there is little chance of it ever leading to major substantive action it has already been a critical force in Canada's domestic political balance. In the economic sphere Canadians voice concern about how extensively American equity capital controls Canadian firms, how completely Canadian exports are tied to the U.S. market, how regularly Canadian employment follows the U.S. business cycle, and how extensively Canadian tastes are made and remade by American advertisers.

Canadian economists have generally lent their support to this concern, but only occasionally have they produced careful analyses of the impact on Canada's economic welfare of U.S. economic dominance. This is unfortunate because of the many types of theoretical and empirical analyses that could and should be done and because of the nearly complete lack of useful policy conclusions in recent studies of United States-Canadian relations.

The present volume, a bundle of seminar papers given at Duke University by seven well-known Canadian scholars, does only a little to improve the situation. The book will give a good introduction to a casually interested economist or the proverbial intelligent layman; but those who have followed the literature on Canadian-United States economic relations will find here little that is new. Furthermore, several major angles of U.S. economic influence are not even touched. The most useful pieces are those by C. L. Barber and Eugene Forsey, drawing together respectively information on the impact of the United States on Canadian agriculture and Canadian labor. H. G. J. Aitken surveys the public policies which Canada has adopted to increase the gains derived from natural resource extraction and exportation, concluding that these policies have made a difference only where Canadian producers in the aggregate have held some monopoly power in world markets. In an essay on the U.S. impact on French Canada, Maurice Lamontagne offers some insights into the relation between U.S. influence and the historic balance of Canadian political forces.

It is high time that persons writing on United States-Canadian relations were chastised for a lack of careful work on the economically significant questions involved. In the present volume only Irving Brecher says much about the need for more research. He gives an excellent agenda for further empirical study centering on the policies of businesses operating in both the United States and Canada. The other authors could well have offered something similar. Do legislative proceedings show Canadian social and economic legislation to be significantly influenced by U.S. practice? Is there evidence that U.S.

technology is adopted where it is inappropriate to Canadian conditions? Do Canadian-American businesses act in ways that help to stabilize the exchange rate? What pattern of Canadian agricultural production would prevail if U.S. farmers operated in an uncontrolled market? What price in economic welfare would Canadians have to pay in order to rid themselves of various aspects of U.S. influence? Only when these and similar research projects are tackled will our knowledge of the economic impact of the United States on Canada amount to much more than armchair observation and a battery of suspicion-ridden hypotheses.

RICHARD E. CAVES

University of California, Berkeley

British Investments in Latin America, 1822-1949. By J. FRED RIPPY. Minneapolis: University of Minnesota Press, 1959. Pp. xii, 249. \$5.00.

Many believe that the marginal rate of return on investment has been higher in the low per capita income countries than in the more developed parts of the world. In the absence of government restrictions on the flow of capital and political instability in the backward countries, capital is expected to flow from the developed to the underdeveloped countries. The Victorian era has usually been cited as the prototype of a "normal" pattern for international capital movements. From 1860 to 1914, Great Britain apparently invested about 4 per cent of her national income overseas. As the late Ragnar Nurkse wrote: "There is in America a feeling of nostalgia for the nineteenth-century environment that made this flow of capital possible. The question is: why can we not re-create that environment?"

J. Fred Rippy, professor emeritus of history of the University of Chicago, has written a book that should be read by those who are interested in the history of the international flow of capital. His book deals with two main themes: (1) the size, nature, and chronology of British investments in Latin America 1822-1949, and (2) the approximate rates of return therefrom. Rippy's book dispels many illusions about international investment in the past century.

First, the rates of return earned by British investors in Latin America were not high by European standards. The author calculates rates of return by dividing the income received by British investors in a given year by the face value of bonds or, in the case of equities, by the par value of the stock. He presents the prices at which Latin-American government issues were floated in England in 1822, 1824 and 1825. This enables us to determine the yield on the amount initially invested.

I have tried to summarize the basic information in Table 1. Unfortunately, there are certain gaps in the data. It is clear that rates of return were quite modest. One would expect a strong degree of uniformity in rates of return yielded on government bonds, given the putative financial integrity of governments. The prices of government bonds to the public (in 1822-25) ranged from 60 to 90. Hence, government bonds hardly ever yielded more than 8 per cent on the price paid. The debt record of the Latin-American governments was quite bad. At the end of 1880, £71 million out of a total of £179 million of government bonds was in default.

TABLE I

	1880	1890	1913	1928	1939	1949
1. Total nominal investment (10%)	£179	£426	£999	£1,211	£1,127	£560
(a) Per cent in government bonds	69%	46%	32%	28%	29%	31%
(b) Per cent in railroads	19	34	46	40	42	29
(c) Per cent in other business enterprises	12	20	23	31	29	40
2. Average return		4.5%	4.7%	4.4%	1.7%	2.5%
(a) Government bonds	2% on face	5	4.4	4.2		
(b) Direct investments	6% on par					
(i) Railroads			4.2	3.9		
(ii) Banking			13.4	} 6.1	2.2	6.0
(iii) Shipping			6.2		4.6	6.9
(iv) Miscellaneous			5.5			

Rates of return received by investors in business enterprises are more accurate reflections of the productivity of capital than are interest rates on foreign bonds. Bonanzas were made; but so were losses and moderate returns. The average annual income from the entire British investment in nearly a thousand economic enterprises in Latin America rarely exceeded 5 or 6 per cent of the par value of the investment. Capital appreciation could compensate for low earnings; but Rippey presents no data on the capital gains or losses made. The rates of return on investment in Latin-American business enterprises were not higher (on the average) than United States stock yields during the past century.

The second illusion dispelled by this book concerns the nature of the British investment in Latin America. Many have entertained the notion that foreign investment in backward countries was of the "colonial type," i.e., investment in extractive industries and plantations which produce products for export to the developed countries. According to the summary table above, this was hardly the case in Latin America. The major portion of British investment was in government bonds and in railroads. If we add the amounts invested in public utilities, then government bonds and "social overhead investments" were the major recipients of British capital. In retrospect, there was good reason to invest in extractive industries rather than in government bonds or in "social overhead capital." Rates of return on investment, on the average, were higher in mining, nitrates and ranches than in railroads and public utilities. Moreover, bonanzas were made in the "colonial types of investment."

Economists should examine the directions and *ex post* rates of return earned by foreign capital during the Victorian era. In retrospect, was foreign investment more profitable than domestic investment? Rippey has made a good start. His work should be extended to other areas; and the analytical tools of economists should be applied to such studies.

J. L. STEIN

Brown University

Der induzierte Kapitalexport. By KLAUS JACOBI. Zürich: Polygraphischer Verlag AG, 1959. Pp. viii, 106. 12.50 sw. fr.

Much of the balance-of-payments literature of the last three decades has been concerned with the identification and/or meaning of a balance-of-payments disequilibrium (see, e.g., works of Iversen, Nurkse, Meade). The capi-

tal balance is considered the crucial sector and its elements are usually divided into two categories—autonomous and induced (or accommodating) capital movements. The former includes all the capital transactions which take place without regard to occurrences in the current account sector, while the latter includes movements caused directly by changes in the current account and/or in the autonomous capital sector.

In the first third of this monographic essay, Klaus Jacobi meticulously reviews contemporary interpretations of the capital sector of the balance of payments. In the remainder of the essay, he examines the various conditions under which induced capital exports have taken place, their significance in expanding trade, and their effects on the economies of the creditor and debtor countries.

Ever since the breakdown of the gold standard, there has been a quest for internal stability, even at the expense of external disequilibrium; restrictions on free trade have grown via increased quantitative restrictions and inconvertibility; and there has been a decline of free capital movements to needy countries due to lack of confidence in their economic and political stability. These factors and the postwar rigidity of exchange rates have brought about an increase in the relative importance of induced capital movements which are either directly or indirectly sponsored by governments. Jacobi divides induced capital exports into those of a bilateral and those of a multilateral nature.

Induced capital exports of a bilateral nature can occur in a number of ways. First, there are the clearing arrangements between two countries where private exporters of the creditor country are "forced" to bear the burden of an induced capital export. Under such a system, the government of the creditor country recognizes the claims of its exporters, while the government of the debtor country receives the payments of importers in local currency. But the debtor country will pay its debts only when it is in possession of the creditor country's currency. Only then will the creditor government, in turn, pay the exporters. Thus the burden is thrown on the exporters who are forced to extend credits of indefinite duration. The author gives an example of such a relationship between Switzerland and Bulgaria in 1949-53, where at one time "waiting periods" for repayment to exporters were as high as 35 months.

We have here the peculiar relationship in which the creditor is the private exporter and the debtor is the government of the importing country. Though such a bilateral arrangement could lead to an expansion of trade, it could also lead to a severe contraction if "waiting periods" for the private exporter should be overextended. It could also bring about contractionary forces inside the creditor country due to a curtailment of exports and due to the lack of capital for domestic expansion because of the forced expansion of credits to the foreign country.

A second type of bilateral arrangement consists of credit extensions by the government of the creditor country. In this case exporters are paid by their governments immediately. This method can be used by a government for purposes of domestic employment-creation by spurring exports, but it could also under certain circumstances lead to inflationary pressures since there is a decrease in the domestic circulation of goods, accompanied by a creation of credit by the government. A third type of bilateral relation is the "swing

credit" arrangement, where each state obligates itself to finance its partner's deficit up to a certain agreed-upon sum.

Jacobi's principal example of induced capital exports in a multilateral context is taken from the experience of the European Payments Union, with its provision for automatic credit extensions by net surplus members. He believes that this experience has shown that induced credit extensions are ultimately transformed into long-term capital exports for a number of reasons. An EPU-type of arrangement forces member countries, whose previous trade structure was not of a nature to have a balance within the EPU region, to change the direction of their trade in order to accomplish a balance within the Payments Union area. Such a change often requires long-term credit arrangements. Many countries will always put domestic goals ahead of the objective of a balance with the Payments Union, and if domestic policy leads to external imbalances, induced capital movements through extensions of credit will have to be of a long-term nature.

Jacobi notes that induced capital movements often act in a perverse way. He cites some examples during the EPU experience when induced capital moved from capital-poor to capital-rich countries, i.e., from countries with high interest rates and internal stability to countries with low interest rates and boom conditions. The former is a surplus country and is forced to export capital through the EPU mechanism. Induced capital movements could in most cases bring about remedial forces via "imported inflation." Under EPU arrangements the creditor country received part of its net surplus in gold and dollars, thus creating the possibility for increased domestic money supply, and most of the credit extended by the state was monetized internally. Thus, with an increased money supply and a drain on goods in circulation because of net exports, there is the possibility of inflationary forces appearing in the creditor countries, which could produce an adjustment in the external balance. Unfortunately, most creditor countries, like Germany, will resist such inflationary imports by counteracting them and thus stunting possible self-induced adjustments.

Jacobi is convinced that adjustments through changes in the domestic buying power of the surplus country's currency is a political impossibility. Therefore, like many other European economists, he is convinced that a resort to flexible exchange rates is the only alternative left.

Although his reasoning is impeccable, the reviewer questions his pessimistic outlook. His judgment seems to have been influenced too much by the imbalances developed by European countries in the early 'fifties. But what is wrong with induced capital exports through an EPU-type system or bilateral arrangement, if they go to less developed European and/or overseas countries? Even if they turn out to be long-term in character, i.e., even if imbalances in the noninduced sectors are of long duration because of development programs, should not European developed economies suffer such induced capital exports and thus make a contribution to the development of underdeveloped countries?

WERNER BAER

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Business Finance; Investment and Security Markets; Insurance

Portfolio Selection: Efficient Diversification of Investments. By HARRY M. MARKOWITZ. Cowles Foundation Monograph 16. New York: John Wiley, 1959. Pp. x, 344. \$7.50.

This monograph extends and amplifies an important earlier paper, "Portfolio Selection" (*Jour. Fin.*, March 1952, 7, 77-91), which proposed a mathematical theory of diversification. Using the variance of expected returns as a risk criterion, the paper sketched an approach for selecting a class of efficient portfolios that would dominate all others; every inefficient portfolio would be clearly inferior, offering lower returns at no less risk or more risk for no greater returns than some efficient portfolio. The monograph expands this idea, relates the selection technique to linear and nonlinear programming, and mentions several alternative risk measures—especially the semivariance, discussed at length. This much will appeal to econometricians and to statisticians interested in decision theory. But the book contains much more, and for this I can see no obvious audience.

The preface says: "Although the techniques are mathematical in nature, the monograph is written primarily with the non-mathematician in mind." Indeed, concern for the nonmathematician is painfully obvious at the start, but it disappears. Part I is an elementary introduction sufficient to give the nonmathematician an idea of what *Portfolio Selection* is all about, and here he should stop; yet Markowitz attempts to push him on through Part II, which the mathematically sophisticated are explicitly advised to avoid, with a tediously labored treatment of averages, dispersion, mathematical expectation, and rates of growth, aimed at readers without knowledge of probability and statistics, or even of elementary algebra. Thus (p. 87):

It is shown in high school algebra texts that, if A and B are any two numbers,

$$(A + B)^2 = A^2 + B^2 + 2AB.$$

That is, the square of (the sum of A and B) *equals* (the square of A) plus (the square of B) plus 2 times A times B. For example, if $A = 3$ and $B = 4$, then

$$(3 + 4)^2 = 7^2 = 49 \text{ and } 3^2 + 4^2 + 2 \cdot 3 \cdot 4 = 9 + 16 + 24 = 49,$$

illustrating that the number resulting from the operations $(A + B)^2$ is the same as the number resulting from the operations $A^2 + B^2 + 2AB$.

But after this elementary interlude, Part III defines and explains how to obtain efficient portfolios for readers who either know or are willing to learn matrix algebra—there are 16 pages of introduction on manipulating matrices—and preferably for those well up on linear programming. Part IV, on "Rational Choice Under Uncertainty," is perhaps easier than Part III, but it is still mathematical and in no way suited for the neophyte. For whom, then, is this book written? Hardly for anyone requiring the intellectual spoon-feeding offered in Part II, for he has virtually no chance of getting through.

Also, the book seems intended for econometric theorists and not for such practical financiers as can understand the mathematics. The subject is practi-

cal, and Markowitz drops hints of applications; but the treatment is theoretical. Thus Chapter 11 on "Utility Analysis over Time," which the practical financier might expect to find accompanied by "Railroad Analysis over Time," comprehends another kind of utility. Moreover, the theoretical contributions include statistical demonstrations of principles long known and practiced. Chapter 5 shows how diversification offers little protection when the returns from securities are highly intercorrelated, and how protection can be increased by including in the portfolio securities whose returns show a low correlation with the average. Translated into workaday financial language for the non-mathematical reader, this means that a portfolio should not consist solely of stocks that typically move together, presumably with the business cycle. And indeed, informed portfolio managers generally recognize that proper diversification requires a variety of industries as well as a number of issues, bonds and highgrade preferreds to provide a cyclically insensitive component, and countercyclical stocks when available. The last several decades have seen the development of formula timing plans as a means of coping with cyclical movements; and of these, dollar averaging provides those in the process of building portfolios with a device for diversification over time. But Markowitz does not discuss timing plans.

Markowitz' nearest approach to practical advice on portfolio management is to suggest that analysts form probability beliefs about future performance of securities and then process these on an electronic computer. He does not, however, explain how to acquire the beliefs. Instead (p. 28):

We shall not discuss the procedures of the security analyst in arriving at reasonable beliefs about securities. Works on security analysis are available. [Here is a citation to Graham and Dodd's *Security Analysis*.] The topic of this monograph is Portfolio Analysis. A portfolio analysis begins where security analyses leave off.

But Markowitz has left a gap, miles wide, between the point where down-to-earth security analyses leave off and the point where his analysis begins. Graham and Dodd have much to say on the adjustment of income for non-recurrent items, the detection of excessive depreciation, the preparation of a cash flow analysis, and even on the selection of suitable price-earnings ratios; but they do not offer suggestions for translating financial statements into probability beliefs suitable for feeding into a computer. If Markowitz is unwilling to bridge this gap, may we not conclude that any thought of practical application is premature?

Moreover, Markowitz offers no argument that personal probability beliefs provide a reliable basis for portfolio management, or that they are worth the cost of processing electronically. His argument rests on the concept of the Rational Man, who must act consistently with his beliefs. But the history of Wall Street suggests that such consistency may be unwise. There seems to be a human tendency, afflicting insiders as well as outsiders, to become progressively optimistic during a bull market and progressively pessimistic during a bear market—a tendency that leads to buying high and selling low, and often to financial disaster. One of the reasons behind the development of formula timing plans has been the desire to counteract this tendency with a set of

mechanical rules that will force the portfolio manager to act inconsistently with his probability beliefs. Far better to be an inconsistent success than a consistent failure!

In Chapter 13, "Applications to Portfolio Selection," Markowitz projects his theories far forward, possibly to the twenty-fifth century. His ideal is a Rational Man equipped with a Perfect Computing Machine, who regards portfolio selection as a single department of consumption and treats the whole as a problem in utility maximization. Of course, he admits that the Rational Man does not exist at all and that the Perfect Computing Machine will not exist in the foreseeable future, but the image of these nonentities seems to have colored his whole work and given it an air of fantasy.

DAVID DURAND

Massachusetts Institute of Technology

Financing of Economic Activity in Canada. By WILLIAM C. HOOD. Including: *A Presentation of National Transactions Accounts for Canada, 1946-1954*, by L. M. Read, S. J. Handfield-Jones and F. W. Emerson. Royal Commission on Canada's Economic Prospects. Ottawa: E. Cloutier, 1959. Pp. xv, 700.

This study is an extremely valuable analytical survey, albeit somewhat prolix, of the working of the capital market in Canada. The capital market is defined as "the set of contacts between buyers and sellers who effect exchanges involving non-monetary financial assets" (p. 11). So defined, the capital market includes the short- and long-term money markets, the new issue market, and the markets for particular types of securities such as the government bond and the stock markets. In short the capital market includes all arrangements and institutions having to do with the financing of economic activity in Canada.

In furtherance of the definition adopted, the author has an introductory chapter on the postwar financing of economic activity, followed by detailed discussions of the problems involved in the financing of the consumer, the farmer and business enterprise, in the work of financial intermediaries, in the operation of commercial banks and in the credit policies of the central bank. A final chapter is devoted to imperfections in the capital market and to needed remedial action.

The volume concludes with a section on national transactions accounts for Canada, set forth for the first time in this study. These represent an extension of the income and expenditure accounts in two directions: The sector classification is more detailed, and financial transactions are recorded as well as flows of real goods and services. The national transactions accounts differ from the American flow-of-funds analysis in that, in the Canadian accounts, all transactions included in the national accounts are to be found in the national transactions accounts, including imputed transactions and accrued income and expenditure. Data are presented for 11 main and 22 subsectors for a nine-year period, 1946-1954.

The author has made many contributions towards a better understanding of the Canadian capital market. These include, *inter alia*, excellent discussions of the financing of Canadian business firms by type and size; the influence of the •

corporate income tax on business decisions; the economics of depreciation reserves; the effect of the absence of a capital gains tax on stock ownership; the importance of foreign direct investment; the issues involved in the financing of small business; the principles which should govern life insurance company investments; the work of the Industrial Development Bank; and the influence of the central bank over commercial banks and the capital market. In developing these topics the author has brought together all available Canadian data, which makes the study an extremely important source-book.

The author concludes that there are various imperfections in the capital market and suggests certain remedial measures. These imperfections would be less significant, for example, if corporations were required to give more information about their financial operations, if life insurance companies and trust companies were permitted to invest a larger proportion of their assets in common stocks, if life insurance companies invested at longer term, if commercial banks were encouraged to increase the proportion of their holdings of urban mortgages, and if the rates of interest on loans insured or granted by governmental agencies were not subject to administered ceilings. Investment trust, common trust funds and pension funds, he feels, are contributing through their operations towards a more perfect capital market. And finally the capital market would work better if consumers were more sophisticated investors. They seem totally ignorant of the cost of funds, particularly in the area of consumer credit, and this distorts the allocation of funds and frustrates the efforts of monetary authorities to control credit. He would compel finance companies and other institutions in the field to state, in clear form, the cost of instalment contracts to consumers.

He concludes that on the whole the Canadian economy has functioned markedly well in the postwar period. There has been no major recession, population and real output have grown, consumption per head has increased. While large sums have been spent on industrial capital, large sums too have been devoted to social capital. Though great success has been achieved in these areas, the economy unfortunately has experienced sharp inflation. Throughout his study the author calls attention continuously to private and public policies which have either aggravated or restrained inflation. No small part of this inflation was generated by the policies of Canada's neighbor to the south.

B. H. BECKHART

Columbia University

Corporation Finance. By RICHARD C. OSBORN. New York: Harper & Brothers. Pp. xv, 637. \$6.50.

Textbooks in the field of business and corporation finance have in recent years tended to follow one of two basic approaches: the traditional, institutionally slanted, descriptive; or the more recent internal or administrative. Professor Osborn believes the older approach more important for the introductory course, anticipating that a large proportion of students will not go much farther into the field. "The internal functional analysis becomes important, but only after an understanding of the more inclusive aspects of a corporation has been attained" (p. xiv).

The general outline thus covers the major facets of business finance: forms of organization, corporate securities, capital structure, promotion, investment banking and security markets, working capital, income and its reinvestment or distribution, expansion, adjustments, and failure and reorganization. The author presents "generally accepted doctrines" and hopes to avoid controversial discussions, leaving the latter for advanced specialists in the field or reference to selected readings at the end of each chapter.

Several changes in the standard treatment are introduced: more emphasis on financing of newer forms of transportation, a separate chapter on the financial problems of small business, and a final chapter on the influence of "comprehensive macroeconomic influences" on corporate financing. An instructor's manual (112 p.), with answers to questions and problems in the text and some objective-type questions, is available to users of the text. Osborn has written clearly and in a very readable and direct style. The volume deserves serious consideration by those who prefer the particular approach of the author. Students should learn much, with a minimum danger of becoming confused or of acquiring erroneous information.

The final chapter, which Osborn calls a "unique" contribution, deserves some comment. The discussion moves from the role of individualism to collectivism and consequent management of the economy for the collective benefit, despite potential inflation. "Regardless of disagreement as to aims and techniques that will prove successful, the principle of centralized control has become firmly established, whether within the framework of a capitalistic or any other form of economy" (p. 608). The responsibilities of the federal government in economic affairs through the Employment Act of 1946 and the Council of Economic Advisers are outlined as an introduction to sections on "Collective Action and the Corporation" and "Influence of Government Action on Business Financing." Osborn concludes by stating that if business creates its own stability through regularizing investment, more bonds can be issued and the resultant leverage will permit lower costs of equity financing. The alternative is coercive action by government (pp. 620-21). This reader opines that the treatment would truly have been unique had it provided a method whereby regularization of business investment could be achieved without collective action.

FRANCIS J. CALKINS

Marquette University

Business Organization; Managerial Economics; Marketing; Accounting

Executive Compensation. By DAVID R. ROBERTS. Glencoe, Ill.: The Free Press, 1959. Pp. x, 189. \$6.00.

The corporation executive has not yet been fitted into the main body of economic analysis. This is true of the function he performs (and the conditions under which he performs it) as well as of the compensation he receives. It is a major virtue of this book that it sets out explicitly to fill this gap. While it must be said that the book does not fully succeed in this ambitious objective, •

which the author would have been wiser not to set forth so boldly in his preface, it does make a useful contribution to our knowledge about executive compensation; and, perhaps even more important, it raises a number of challenging questions which, if not always satisfactorily answered, should encourage further work in an area which has been too neglected by economists.

The larger part of Dean Roberts' study is concerned with an empirical investigation of the relationships between executive compensation and other possibly relevant variables: for example, sales, dollar profits and profit rate, type of industry, and geographic location. Using modern statistical techniques, he concludes that the absolute level of compensation is related to company size but not to the other variables which are sometimes cited. In particular, if allowance is properly made for company size, executive compensation does not vary significantly with profitability of the firm.

The most interesting part of this study comes in the last third of the book. Here the author attempts to develop a theoretical explanation of executive compensation and relate it both to his empirical findings and to some of the existing literature. What is even more useful, he offers a very suggestive analysis of the market for executives. While the findings may not be altogether new, I have not seen them pulled together in this way before, and the implications which are drawn are of considerable interest. For example, there is relatively little movement of executives among firms; what movement there is is related to personal characteristics rather than to differentials in compensation. Hence market forces of the usual sort play a very limited role in determining the level of compensation in individual companies, at least in the upper ranks, and there is not much of an association between available measures of "entrepreneurial productivity" and compensation. Further, increments of compensation above the levels prevailing in particular firms have limited incentive effects. One suggestive inference is that the difficulty of obtaining trained executives from other firms may be a significant impediment to the entry of new firms in industries where initial large size is required, and thus is another factor, in addition to those usually mentioned, tending to foster concentration.

One of the important virtues of this book lies, in a sense, in its methodology, which presumably can be attributed to the author's training and experience at the Graduate School of Industrial Administration, Carnegie Institute of Technology. Here a "practical" business subject is explored not only with the tools of statistical and economic analysis but also with some help from sociology and social psychology. This sort of analytical, empirical, and interdisciplinary study is clearly to be encouraged.

R. A. GORDON

University of California, Berkeley

Industrial Organization; Government and Business; Industry Studies

Monopoly in Economics and Law. By DONALD DEWEY. Chicago: Rand McNally and Co., 1959. Pp. 328. \$5.75.

The fact that this "small treatise" on monopoly is a "highly personal work" makes the job of review both easier and more difficult: easier because the

author has a story to tell which he does tersely and with verve; more difficult because of omissions, different intensities of treatment, and varying levels of sophistication. In fact, it is difficult to locate the level to which this book is addressed. But through it all the skepticism (or is it caution?), the occasional unorthodox proposition, and the recitation of the overlooked fact titillate.

The task of economists in the development of monopoly policy is described as that of providing policy-makers with "an increasingly accurate picture" of what the "business world is 'really' like" (preface). The task of economists to develop standards by which structure or performance is to be evaluated is not mentioned; no reference is made, therefore, to the extensive literature on workable competition, which is, though, mentioned several times as a condition to be achieved. This is not necessarily a fatal omission in a personal book. It is, however, quite precarious for a book whose most likely "special claim to novelty . . . derives from the conviction that economists and lawyers who are interested in monopoly as a policy problem cannot get along well without one another" (preface).

The contributions of economists are discussed under the theory of cartels, the theory of consolidations, and their respective refinements. The theory of cartels summarizes the conventional approach. The chapters on consolidation explore primarily the relative importance of scale economies and of the drive for market power. Dewey's concern in the second chapter on refinements of monopoly theory is refutation of the proposition "that in imperfect competition a policy of *laissez faire* must lead to a no-profit-no-loss equilibrium in which each firm is of less than optimum size" (p. 88). In the course of this treatment he defends Chamberlin's thesis that monopolistic competition is not the same as imperfect competition.

Monopoly at law accounts for about two-thirds of the book and is the most interesting part. The common law conception of monopoly policy in both this country and Great Britain is explored. From this Dewey concludes, among other things, that typically the rule of reason has been used by American courts to validate both direct and ancillary restraints of trade—a victory too easily won.

A quite brief summary of the passage and statutory standards of the Sherman Act is followed by a longer consideration of the enforcement procedures during which opposition is declared to both criminal and treble damage suits. More than a third of the book is devoted to the development of antitrust case standards in terms both of what the courts said and what they did. In this treatment the following headings are employed: cartels, unfair competition, mergers, good trusts, and labor monopoly.

As is generally recognized, the historical role of unfair acts of competition in the formation of large companies at the turn of the century led Congress in the Clayton and Federal Trade Commission Acts to assign to antitrust agencies the sometimes incompatible tasks of preserving both competition and competitors. This ambiguity was aggravated by the Robinson-Patman Act. According to Dewey, the "major inarticulate premise" of this "execrable concession to small business groups" is "simply wrong; most of the prices in the

world are not equal to the seller's average cost of production plus a 'fair' markup" (pp. 198-99). It could be, however, that his articulate major premise of marginal pricing is less relevant to the world as it "really" is than a full-cost pricing assumption.

In spite of the brave words of the 'forties, the law of good trusts is found to prevail; "so far as trust-busting is concerned, the new Sherman Act has become indistinguishable from the old" (p. 254). This result, however, was "inevitable' given judicial reluctance to disturb private [property] rights in the interest of promoting nebulous public goals" (p. 254). In fact, "courts are allowed to retain the judge-made remedies of dissolution and divestiture only because their conservative bias is good surety that these remedies will not be much used" (p. 254). Because of this powerful conservative force, Dewey concludes that the liberal must place less reliance on attempts to change market structure. Where then the liberal? For the future Dewey seems to suggest that the best alternative to trust-busting is the adoption of "guerilla tactics"—"in unspectacular Fabian sallies, notably the blocking of doubtful mergers, elimination of the secondary boycott, close scrutiny of the awarding of government favors, and curtailment of the patent privilege" (p. 256).

The law of labor monopoly rates one of the longest chapters. There is little doubt that Dewey considers this a sorely neglected area. His opposition is not to a double standard in antitrust policy but rather to the encouragement of industry-wide bargaining under the Wagner Act (a "damaging" defeat to the opponent of monopoly) and to the survival of the secondary boycott after the Taft-Hartley Act.

In a helpful chapter on monopoly policy in Great Britain Dewey gives a brief but well-formed explanation of the reasons for the British attitude toward monopoly. He cautions against making "too much of the difference" between such policies in Britain and the United States—although they are recognized as "real enough"—in the limited context of explaining the over-all performance characteristics of the two economies (pp. 298-99).

ROBERT E. SMITH

University of Utah

Risk and Technological Innovation: American Manufacturing Methods during the Nineteenth Century. By W. PAUL STRASSMANN. Ithaca: Cornell University Press, 1959. Pp. x, 249. \$4.00.

What has been the role of the entrepreneur in the process of technological innovation? In particular, how much risk has been involved in the innovating process? Professor Strassmann examined the history of technological innovation in four important American industries during the nineteenth century in order to assess the divergent views which have been held. For example, he contrasts the viewpoint of Schumpeter with that held by Veblen. Schumpeter emphasized the creativity and riskiness of the innovating entrepreneur's undertaking; Veblen held that the entrepreneur's role was at best permissive, not creative, with regard to technical innovations and that, at least in this

activity, little risk was run, particularly during the latter part of the nineteenth century. Strassmann concludes (p. 4):

... that both Schumpeter and Veblen were partly right and partly wrong. The leading promoters of new manufacturing methods were neither gamblers nor passive exploiters of the creations of others. They were a creative factor in economic development; but, by and large, they proceeded with a degree of caution that, with certain exceptions, reduced the chance of error to negligible proportions.

Strassmann defines and uses the term "risk" as a synonym for what some economists have chosen to call "uncertainty": the chance of loss involved in an act of innovation is not (ordinarily) known in an actuarial sense. However, although the uniqueness of any given act of innovation makes impossible the application of the laws of probability in a strict sense, Strassmann emphasizes that the innovating entrepreneur is ordinarily able to predict, with a greater or lesser degree of precision, the behavior of many of the factors which will determine the outcome of his attempt.

Four "case studies" are presented in order to determine the role and extent of risk-taking of the innovator during the nineteenth century: iron and steel, textiles, machine tools, and electric power. In all cases the focus is on innovations in techniques and equipment rather than on product innovation. The author is primarily interested not in the subjective estimates of risk on the part of innovators, but on what amounts to an *ex post* evaluation of the objective riskiness of innovating activity. He does, however, point out the frequency with which people in a given industry viewed those introducing innovations as very daring (indeed, often "insane") risktakers. Unfortunately, he does not always keep clear the distinction between the subjective (contemporary) evaluation of risk and his own estimation of the "real" risk facing the innovators in each of the four industries.

Strassman identifies and assesses the importance of the following types or sources of risk: *Interference risks*—the chance that an innovation may fail because of successful resistance to its introduction by those threatened by it. Lack of effective labor organization and continued growth of employment opportunities meant that this form of interference risk was insignificant. The patent system, at least until the latter part of the period (when patents became important, particularly in electric power equipment), did not constitute a major interference risk. And business rivals, while occasionally successful in providing an irritant, did not succeed in interposing serious interference to innovations.

Customer risks were likewise unimportant during the nineteenth century. New types of equipment and improved products were accepted quickly by customers in most cases. The exceptions are not explained so much by ignorant loyalty to old, tried and true ways of doing things as to lack of quality control and a resulting unpredictability inherent in the innovation at the time of its first introduction.

Nor were innovating firms particularly threatened by *timing risks*—the introduction of an innovation coinciding with a business depression. Certainly

the innovating firm did not suffer any more than any other firm from this source, nor in fact does Strassmann find that many innovations failed because of timing in the industries he examines.

The most serious type of risk which innovators faced were the *production risks*—the chance that the new technique wouldn't work at all, would be much more costly than expected, would be unusable because of the insufficiency (in the consuming industry) of the required labor skills and/or input materials. One difficulty this reviewer has with Strassmann's discussion of this type of risk arises from an apparent failure always to recognize the distinction between invention on the one hand and innovation on the other—a distinction the author makes clearly elsewhere. Specifically, while I am persuaded that the lack of trained scientists and engineers and their skills was important, I would argue that this lack was more important in explaining the nature of the *inventive* process (particularly during the first part of the nineteenth century) than in explaining the nature of the so-called production risks facing the innovator.

The studies of each of the four industries are well done and interesting, and a very complete bibliography has been included. Though one familiar with the history of any of these industries will find little that is new, the author is successful, in so far as the facts available permit, in assessing the degree of risk and its sources (or lack of) in each case. Strassmann is aware of the danger that the record of successful innovations is more complete than that of failures. But for the industries he has studied the records are complete enough for his purpose.

As he emphasizes, the outstanding characteristic which emerges is the relative caution of the innovators—though many of their contemporaries felt they were taking great risks. While Strassmann shares Schumpeter's view that innovators played a significant role in the growth of this country's economy during the nineteenth century, he does not find this breed to have been particularly daring. In fact, given the objective riskiness, he concludes that innovators and potential innovators and investors were irrationally cautious in their assessment of the probability of failure and their underestimation of the profitability of successful innovation. The latter was due in particular to a failure to recognize what Strassmann finds to have been a pervasive phenomenon, namely, the reciprocal complementarity of innovation within industries and among industries.

I was struck by one historical note of contemporary interest: the present importance of the government in promoting invention and innovation in the military field has a parallel in the early development of the machine tool industry which owes its start primarily to the government's demand for weapons and willingness to support what we have come to call "R and D." Certainly developments in such fields as aeronautics and electronics, primarily sponsored by the Defense Department, have had and will continue to have a significant impact on nonmilitary products and industries.

WILLIAM M. CAPRON

Stanford University

Razmeshchenie chernoï metallurgi SSSR. (The Location of the Iron and Steel Industry of the USSR). By R. S. LIVSHITS. Moscow: Academy of Sciences of the USSR, 1958. Pp. 375.

This book is one of dozens of Soviet monographs published every year about the economics of the Soviet iron and steel industry. In addition to books on various economic aspects there are hundreds of technical and scientific books published annually, each of which contains varying amounts of economic information and analysis about Soviet steel, not to mention thousands of periodical articles on these topics. Public and private enterprises are attempting to abstract and translate the more important journal articles, but they cannot begin to translate the ever-growing mountain of books. This review therefore, will interest primarily the small but increasing band of U.S. economists who read Russian. Mme. Livshits' work is important because it is the first full-length monograph in 26 years to be devoted exclusively to the location of the Soviet iron and steel industry.

It can be criticized on a number of grounds. It contains not a single map. Furthermore, the theoretical discussion is confined to unconvincing criticism of Western location theorists such as Weber and Predöhl. But rather than dwell on shortcomings, this reviewer will point out certain positive elements that might be useful to American scholars.

Historians and students of Soviet economy will be interested in the chapters on the location of iron and steel in prerevolutionary Russia and the Soviet period up to 1940. They will find statistics about the production of pig iron, steel and rolled metal in all of the important iron and steel plants in Czarist Russia, and a wealth of data about the plants, workers, labor productivity etc., for the years 1890, 1900, and 1913. Chapter 4 alone has 36 statistical tables dealing with the interwar period. Of the 14 tables in Chapter 5 the most interesting gives the regional distribution of steel production for selected years from 1913 to 1956. The emphasis of Chapter 6 is on the need to raise labor productivity; and for the first time a few postwar employment figures for the iron and steel industry are made available. The last third of the book deals with problems of the future development of the industry.

The latest data on unit costs of production of iron and steel products in the United States were published in abbreviated form in the early 1920's. Even these figures were given only in very broad cost categories. As Bela Gold puts it, "Sound thinking about industrial cost-price relationships has been seriously handicapped by the secrecy which necessarily veils individual company data and by the striking scarcity of research dealing with manufacturing cost at more broad levels. As a result, businessmen and economists have been forced, in dealing with immediate problems of policy, to bridge over these factual voids with an array of assumptions." Mme. Livshits, by contrast, has packed her book with data on production costs and the cost of shipping products to primary markets. Table 75, for example, gives a breakdown of the costs of producing one ton of conversion pig iron in 13 of the largest Soviet iron and steel plants for the years 1940, 1955, and 1956. The range in 1956 was from 101 to 521 rubles per ton. Seven of these plants, including the extremes, were visited by the U.S. iron and steel delegation in 1958. Their report

(*Steel in the Soviet Union*, American Iron and Steel Institute, May 1958) provides the technical and economic data needed to understand this pattern of Soviet production costs.

Two books by another Soviet economist carry on the cost analysis in much greater detail: D. L. Maizel's, *Organizatsiia, planirovanie i finansirovanie kapitalnogo stroitel'stva v chernoï metallurgii* (Organization, Planning and Financing Capital Construction in the Iron and Steel Industry), Moscow 1957; and his *Sebestoimost' chernykh metallov* (Costs of Iron and Steel), Moscow 1958. We are, therefore, in the interesting position of having both detailed costs and price data as well as technical information about a key Soviet industry, but we lack the corresponding data about any Western steel industry, which we need as a basis for comparison.

M. GARDNER CLARK

Cornell University

Land Economics; Agricultural Economics; Economic Geography; Housing

Twenty Years of Public Housing: Economic Aspects of the Federal Program.

By ROBERT MOORE FISHER. New York: Harper and Brothers, 1959. Pp. xiii, 303. \$6.50.

The echoings of the frenetic controversy over "socialized housing" prompt the reader to reach hopefully for this volume in search of a definitive answer. He will not find it; but the cautious nonpartisanship of the author does not effectively reduce the usefulness of this comprehensive survey of the 1937 United States Housing Act in operation.

After an abortive start, the federal public housing program got under way in 1937 and has resulted in the demolition, closing or repair of nearly 300,000 substandard urban dwellings. In 2,700 active housing projects in 110 localities, some 534,000 low-income families of former slum dwellers are presently housed. The author traces the origins of the public housing movement and discloses the complex features of the federal program with full statistical report on its accomplishments and costs. Giving recognition to the controversial nature of the definition of "substandard" housing, he considers the persistence of substandard housing in our urban areas as a challenge to the ability of free enterprise to meet the housing needs of society. In 1950, 5.6 million urban dwelling units or 22 per cent of our housing stock, were substandard. It is revealing that nonwhites occupied only one-quarter of the substandard units; that slum tenements were the exception, for three-fifths of the units were in one- and two-family structures; that one-third of the bad housing was occupied by home owners; and that among low-income families, with incomes below \$2,000, 60 per cent lived in standard housing. Another interesting fact is that a substantial proportion of the occupant families in substandard housing, up to three-fifths, were paying a lesser proportion of their income for rent than generally accepted norms and could afford to occupy better accommodations.

The chapters dealing with the costs of public housing to federal and local

governments are thorough treatments of the subject, but frustrating in their conclusions that the cost really can't be measured for reasons that are analytically sound but too involved to be summarized here. Fisher suggests that costs are higher than they need be by reason of the annual contributions-subsidy formula. The "annual contribution" of the federal government is determined by the difference between the yearly debt service on the local borrowings for project construction and the rental revenues above project operation costs. Thus larger capital expenditures or higher operating expenses are met by larger contributions and there is "no financial incentive to local authorities to build or manage units at lowest costs consistent with long-term needs."

The study throws some light on accomplishments of the program in eliminating substandard housing. However, it appears that at the present rate, it will take some three centuries to "eradicate all substandard accommodations existing inside standard metropolitan areas in 1959." This is not to say that the elimination of 300,000 such units is not an admirable accomplishment; but so vast a task, in the author's opinion, will require not only continued public action but greatly increased local interest and participation. It is a defect in the current program that the original provision requiring the "equivalent elimination" of one substandard dwelling for each new one provided has been so weakened by law and regulation that it is virtually inoperative.

Fisher amply illustrates the dilemma of the local housing authorities in administering the ability-to-pay basis for establishing rents against the objective of housing low-income families. Low incomes mean low rents and thus high subsidies. In periods of rising incomes, prosperous tenants whose higher rents reduce the subsidy are required to move to make way for families of lower incomes who create larger subsidy burdens. In 1957 the admission income limit of a typical project was \$2,800 yearly; families whose incomes increased by as much as 25 per cent, to \$3,500, were required to move out.

This book is well written, amply documented, comprehensive in coverage; it identifies the issues and points up deficiencies and limitations in the public housing program matched by counterbalancing data on accomplishments. No other objective treatment of equal scope has appeared to provide the basis for an economic evaluation of the public housing program. The study's limitation is that it provides a basis for an evaluation but somehow fails to add things up to a conclusion.

RICHARD U. RATCLIFF

University of Wisconsin

Labor Economics

Theory of Wages and Employment. By ALLAN M. CARTTER. Homewood, Ill.: Richard D. Irwin, 1959. Pp. xii, 193. \$5.00.

At only slight risk of overstatement, one may say that this book will appeal to that perhaps largely mythical group of economists known as "marginalists" and that it will prove somewhat distressing to the more readily identifiable

group of antimarginalists. The large body of academically cautious who tentatively accept marginalism in lieu of something better will probably feel that Cartter has gone too far in his wooing of this now elderly lady. A less ardent defense, with more space devoted to the nature of evidence which Cartter finds so convincing, would probably have been more influential.

Cartter's book is certainly not without virtue—the reader knows where Cartter stands, the presentation is generally lucid and seems to strike the right degree of brevity. The level of the discussion is probably appropriate for the average first-year graduate student. Some background in such matters as indifference analysis and Keynesian economics would be helpful to the student reader. On the other hand, too much background in industrial relations might prove embarrassing to some of Cartter's contentions.

Roughly the first half of the book is devoted to traditional marginal productivity analysis in a closed economy. After an appropriate consideration of the nature of theory, Cartter proceeds to the Clarkian and Marshallian versions of marginal productivity, the problem of the exhaustion of total product, and the meaning of the marginal revenue product. Selected criticisms of the theory are then taken up and often seen to be the result of short-run applications of an essentially long-run argument. Two chapters are then devoted to such matters as oligopoly, technically fixed requirements, assembly lines, shift operations, supply curve problems, monopsony, exploitation, and variable cost curves that decline to "capacity."

Part II opens with a consideration of some models of trade union behavior, a discussion characterized by the use of Fellner's trade union preference functions which Cartter prefers to Dunlop's membership functions. Bargaining is analyzed through a study of the relationships between trade union preference functions and employer preference functions, with the typical case of bargaining taken as one with a gradually increasing demand for labor.

The strike threat is then introduced into the bargaining picture and each party's bargaining attitude is analyzed in terms of the ratio between the cost to X of disagreeing with Y and the cost to X of agreeing on Y's terms. A value of the ratio of less than one is unfavorable to settlement, while a value greater than one is favorable. A further development of the bargaining model is then undertaken on the basis of the work of J. Pen (this *Review*, March 1952).

Cartter then moves to the aggregative level with a discussion of Keynesian and post-Keynesian problems. In this connection Cartter's discussion of the behavior of money wages is impaired by the fact that he does not indicate the source of the data used in the scatter diagram on page 147, so that the reader cannot evaluate the argument on his terms.

The penultimate chapter is largely concerned with macroeconomic (savings-investment) analysis of the effects of changes in labor's share of the national income. Cartter's pedagogic aims are not clear in this chapter and it impressed me as the least well-done in the book.

In the final chapter Cartter makes a plea for an "informed theoretic empiricism" in the study of the economics of labor. This is unassailable advice of which one may hope Cartter will partake liberally in the future. This re-

viewer also believes that a discussion of the secular inflation problem and its possible relation to wages would have increased the usefulness of the book as a possible text.

FRANCIS S. DOODY

Boston University

The Public Stake in Union Power. Edited by PHILIP E. BRADLEY. Charlottesville: University of Virginia Press, 1959. Pp. x, 382.

This book consists of a series of lectures by sixteen economists, who are identified in the preface as "distinguished economists who collectively may best be described as theorists or generalists." The sixteen apparently were selected to exclude those referred to as "persons versed in the skills of labor arbitration or . . . persons who follow the adaptive approach and emphasize psychological and other techniques that are helpful when adjusting to difficult situations." From the standpoint of participation the book is reminiscent of *The Impact of the Union* (D. McC. Wright, ed., New York 1951), for five people have contributed to both volumes. It is also like the earlier book in the sense that it is in large part a tract for the times on the perils of collective bargaining. At various places in the book unions are condemned for contributing to inflation, for prolonging depression, for standing in the way of progress, for denying efficiency and freedom. Several of the authors represent unions to be the most privileged and pampered of voluntary associations in the American economy.

The general theme of the book would seem to be that the public has an interest in limiting the power of unions. That this is not a novel theme is suggested by D. M. Wright's comment that "Practically everybody now agrees that unions should regulate in some way or other for some purpose or other (p. 111). (One wonders why he uses the word "now.") He then goes on, "But where trouble starts is in wondering how and for what purpose the union should be regulated." If "regulation" is an ambiguous word, it would appear that "power" is even more slippery. As the reader goes from one lecture to another in the book, he finds that each author has a different facet of power in mind.

It is possible to classify the lectures in terms of the type of power selected for consideration. One group, which is centered on the wage-push inflation problem, includes the papers of F. A. Hayek, Gottfried Haberler, William Fellner, and J. R. Meyer. E. H. Chamberlin deals with this as one of several issues. These several authors look with varying degrees of alarm at the power of unions to raise the general money-wage level and thereby to cause unemployment or inflation or both. They are all agreed that this is a problem, although Fellner feels ". . . moderately optimistic with respect to the degree of employment which would prove compatible in the long run with the insignificance of inflationary tendencies" (p. 254), and Meyer finds that ". . . the general effect of a wage increase would perhaps be desirable in times of a cost push inflation when investment capacity is fully utilized and consumer goods capacity is underutilized" (pp. 280-81).

A second group deals with the power of unions to interfere with or distort

the workings of the competitive market. Thus J. M. Clark reviews in a rather detached and whimsical manner all that is known and not known about wage determination in collective bargaining. F. H. Knight restates the liberal opposition to "political" interference in the free market. Wright cries out against unions as a restraining influence on the vital and progressive elements in competitive capitalism. The most intriguing paper in this group, one which throws off sparks in several directions, is the one by H. Gregg Lewis, "Competitive and Monopoly Unionism." This lecture, which is a working paper antecedent to further empirical research, seeks to develop a series of tests for measurement of the relative wage effects of unions' monopoly power. G. S. Becker pursues one of the implications of the Lewis paper, namely, that union power to affect relative wages will be indicated by price (initiation fees and dues) and nonprice techniques to ration entry to the union. Albert Rees confines his attention to two practices compatible with "competitive unionism," grievance settlement and seniority rules. G. W. Nutter inquires into the factors that determine the limits of union power. Bradley deals rather querulously with the question of how compulsory union membership comes into being. He concludes that neither Congress nor the executive branch of government, nor management, nor the courts have defended individual freedom from compulsory unionism. In the opinion of this reviewer the best single paper in the collection is the one by J. W. McKie, who goes directly to the issue of "Collective Bargaining and the Maintenance of Competition," illustrating the problems by specific cases and dealing in a realistic manner with the difficulties of finding practicable remedies.

Two of the lecturers do not talk about union power at all. P. T. Bauer lashes away at government regulation of wages in underdeveloped countries, urging that the cost of such regulation falls primarily on those excluded from the regulated activities. A. A. Alchian appraises the economic organization of higher education and finds it lacking. He looks first at faculty tenure rules which he considers neither necessary nor efficient, and then moves swiftly on to condemn the nonprofit, nonowned system which he identifies as the cause of tenure. His suggestion is that government subsidies be given to students rather than to schools, in the fashion of GI benefits. This would, he believes, "provide a chance for the private, profit-seeking school to provide us with a test of efficient education and truth seeking" (p. 369).

What does the whole book add up to? The book offers additional evidence that economists share a high regard for the competitive market and contumacy for some practices of some unions. However, it also shows a lack of agreement about how much and what kinds of power unions do in fact have and what limiting factors are presently operating. It is notable that when the discussion is about inflation the references are generally to industrial unions and when it is about "distortion" the examples are usually craft unions. Further lack of agreement appears in the area of policy recommendations even though only a few make any specific proposals for change. Some write from too lofty a perch to deal with such earthy particulars. This Wright says, ". . . so far as mere legal gadgets are concerned, there are plenty of measures available, and what is really lacking is not legal machinery but public opinion and public

understanding" (p. 122). Haberler and Fellner would rely principally on the reasonableness of unions; others stress prohibition of industry-wide bargaining, application of antitrust laws, and limits to union security agreements, picketing and secondary boycotts.

While the book does include varied treatments of a range of topics, the reader is given a decidedly one-sided interpretation of the issues. There is little sympathetic interpretation of unions' purposes (the Rees and McKie papers are exceptions) and nothing in the way of a staunch defense of them. It would have made more interesting reading to have the defendant in the dock, so to speak. One wonders if the students at Virginia needed to be inoculated against the virus of pro-unionism; is this like inviting a series of temperance lecturers to a Methodist church group?

The lack of "balance" or synthesis and the unevenness in the level of discourse and analysis will doubtless limit the book to use in specialized courses and to the researcher's reference shelf. However, this is an important use, and the University of Virginia, by publication of the lectures, has contributed to the evolution of national labor policy.

ROBERT J. LAMPMAN

University of Wisconsin

The Wage Rate Under Collective Bargaining. By J. PEN. Translated by T. S. PRESTON. Cambridge: Harvard University Press, 1959. Pp. xiv, 216. \$6.00.

This book could properly be subtitled "In Search of a Tautology." Pen tries to develop an equation system which must necessarily determine the wage rate resulting from collective bargaining. By definition unions agree to a wage settlement when the determining agent prefers to agree. If the union negotiator settles for a wage less than the wage that he regards as optimum, he does so because of the risk of conflict with the employer.

According to Pen's first model, the union negotiator will accept a wage from the employer when the additional benefits (as felt by the negotiator) of pushing to the optimum wage just equals the cost (as felt by the negotiator) of a conflict with the employer if the union insists on the optimum wage, when both are multiplied by the union negotiator's estimate of the probability of their occurrence— $(1 - r)$ and r respectively.

But this is not quite tautological enough for Pen, since it implies that the union negotiator is concerned with maximizing the expected value of his utility. Calling s the subjective weight given to r by the union leader, Pen substitutes s for r in producing his final equation of the equilibrium wage for the union.

A similar equation can be developed for the preferences of the employer negotiator. What we have, then, is an overdetermined system: two equations determining one unknown, w . The purpose of bargaining is so to change the functions in the two equations that they both yield the same w .

Pen does not quite achieve his tautology. That the optimum wage rather than w is on the margin of being worth the union negotiator's risk does not imply that w is the best wage for the negotiator to try to get. Some inter-

vening wage might be his best bet. To be valid, Pen's equations have to be rewritten in marginal terms. They then become special cases of utility maximization when marginal cost equals marginal revenue. Also Pen assumes that conflict utilities are independent of w , which is quite obviously not the case.

But these are rather minor revisions to produce Pen's goal of the collective bargaining tautology. The big question is: Once we've traveled to this destination (through some extremely muddy and unnecessary pages, I might add) how far have we gotten? Tautologies can be extremely useful, but they can also be nothing more than monuments to man's capacity to waste paper.

It is hard to say now how useful Pen's tautology will be. Certainly nothing Pen does with it demonstrates its worth. Rather he seems content to make his "theory" a rack on which to hang his casual observations of the bargaining process. Seeing a union negotiator bluffing a strike, he can say: "Ah, ha, that man is trying to increase the employer's estimate of the probability of conflict." Is this an intellectual advance?

Pen predicts (not from his "theory," which predicts nothing) that econometricians will be unhappy with his tautology. I think he is right. None of the terms in his equations refer to observable magnitudes. Nor do I see any rationale for assuming any particular behavior to these terms, and hence developing a theory that says something new about the way in which the world behaves.

On the way to his tautologies Pen criticizes Hicks and Zeuthen for their bargaining theories on the grounds that they are not tautological. But this is precisely the reason they yield testable implications. Hicks, for example, predicts that strikes are results of mistakes. Hence anything which would decrease mistakes, like length of the previous relationship of employer and union, should decrease strikes. Obviously this need not be the case. Isn't that refreshing!

This negative report is made in spite of Pen's imaginative, and sometimes insightful, display. The failure of this book is not so much Pen's, as that of an intellectual tradition.

PHILLIP NELSON

Columbia University

Probleme der Lohnstruktur: die wirtschaftliche und soziale Bedeutung der Lohnunterschiede. By FRIEDRICH FÜRSTENBERG. Tübingen: J. C. B. Mohr (Paul Siebeck), 1958. Pp. vi, 116. DM 9.80.

After a considerable interval, German economists have recently turned their attention again to labor economics. Erich Arndt's presentation of wage theory—reviewed in this *Review*, March 1958—has been followed by the present smaller monograph. This is, on the whole, an excellent summary of the present status of our knowledge about wage structures, their functions and—to a lesser extent—their changes. The work is mainly based upon American and British sources. In the six-page bibliography at the end of the book, more than half of the references are to Anglo-American literature. Unfortunately, the author does not seem to have closely followed the discussions of

union influence on relative wages. This, however, is the only major gap in the bibliography and in the body of the book that I have been able to detect.

The outline of the book is rather simple. The first chapter defines terms and lists seven types of wage differentials, including differences between time and piece wages. This is followed by chapters on the economic and social functions of the wage differentials, on changes in the wage structure, on problems in making the wage structure rational, and conclusions. On the whole, the author seems to share the basic position of Lloyd Reynolds, Richard Lester and others on the virtues and shortcomings of conventional labor-market theory, and places more emphasis on institutional factors than on the market as such.

Since the author's original contributions are somewhat limited in scope, I shall restrict myself to a few points on which significant disagreement exists. Fürstenberg seems to read more into the concept of the productivity of labor than can rightly be claimed for it. He identifies it, apparently, with that portion of the change in output that can be imputed to labor rather than investments, inventions or improved organizations (p. 9). At the same time, this concept of labor productivity is examined for its use as a yardstick for the appropriateness of income distribution (p. 11), and is found wanting. Needless to say, the concept has no more than a definitional meaning among social scientists and no necessary implications follow from it regarding changes in wage levels. The statistical evidence on which the author's contentions are based is singularly unconvincing: short-run comparisons are meaningless.

Wage differentials, the author finds, have little bearing upon the allocation of manpower. The evidence in support of this view is quite modest and would deserve more critical examination than the author has given it. Essentially, it seems to indicate that wage differentials are not always a necessary, nor a sufficient reason for job changes. That, however, means simply that job changes are a function of many variables, and few will dispute that wage differentials are one of these variables.

While the book will serve very well as an introduction to some aspects of wage theory for German-language students, its main interest for Americans will be the light it throws upon characteristically German problems. There is the difference between contract wage rates and effective wage rates. This is not the same problem as the "wage drift" Swedish style, but a regular part of the German collective bargaining system. Contract rates are merely minimum rates, comparable to our minimum wage under the Fair Labor Standards Act. Data on contract rates are, therefore, only indicative of effective rates, with the added difficulty that the relations between the two are likely to undergo appreciable changes from time to time, especially over the business cycle. Some wage statistics refer to contract rates, a few to effective rates, but on the whole there is a surprising lack of German wage data in the study—a good deal of the supporting evidence is taken from U.S. statistics. German wage statistics have not yet recovered from the ravages of the war.

The author presents some interesting data on the changes which improved education has produced in the possibilities of promotion for German workers.

Educational levels of skilled workers now "often surpass those of office employees"; they sometimes have academic high school degrees, and at least "in principle" workers may now even become engineers. The wall separating manual and nonmanual labor is gradually being removed.

Finally, this reviewer notes with a good deal of satisfaction the outspoken stand of the author against the idea—advanced in various ways in Germany—that there are objective standards which would permit a body of experts to set "correct" wage relationships. Indeed, the confrontation of some of the conflicting principles that can be used in determining relative wages is one of the best parts of this book, even though I would have wished for a more systematic analysis.

Altogether, it is a very useful book which might well serve as the starting point for future research. This is indeed precisely what the author intended his book to be.

ADOLF STURMTHAL

Roosevelt University

The Practice of Collective Bargaining. By EDWIN F. BEAL and EDWARD D. WICKERSHAM. Homewood, Ill.: Richard D. Irwin, 1959. Pp. xvii, 738. \$7.50

This book is true to its title for it emphasizes practice rather than theory. It probably will be of most interest to teachers and students technique courses and of little, if any, interest to professional labor economists. Economic aspects of collective bargaining are either missing or crudely treated. Such subjects as full employment, inflation, key wage settlements, and the wage-price spiral, are not even mentioned in the index. A short "digression" on wage theory is aptly titled. It does not succeed in its aim ". . . to make a modest contribution to the methods of theoretical analysis of wage determination . . ." (p. 155). The alleged contribution stems from two postulates: "Craft-union wage bargaining is for a *standard and uniform* union rate . . ." "Industrial-union wage bargaining . . . is for a *structure of unequal rates* . . ." (p. 155).

The central theme of the book may be summarized as follows: Collective bargaining can be explained and analyzed in terms of practice in two "model" situations, (a) craft unionism, and (b) industrial unionism. Part I of the book attempts to set the stage, with an historical review of collective bargaining in the United States, and descriptions of unions, employers and the role of government. The next four chapters deal with special types of union situations, in (1) craft industries, (2) factories, (3) extractive, distributive and service industries, and (4) white-collar situations. Part I closes with four chapters on: (a) areas of agreement and disagreement, (b) negotiation, (c) strike strategy and tactics, and (d) the nature of the labor agreement. The mission of Part I is apparently to make the case that craft and industrial unions are different and to argue that this explains differences in collective bargaining practice.

Part II is titled "Issues in Collective Bargaining," and includes conventional

subjects such as union and management security, wage structures and adjustments, hours and working conditions, pension and welfare plans, employment security, and technological change. It closes with a chapter on contract administration, a curious subheading under "issues in collective bargaining," and to this reviewer at least, giving too little emphasis to this subject.

Part III consists of 17 arbitration cases for student analysis. The book has an author and a subject index. The latter is weak.

Each chapter lists several "topics for investigation" presumably for students although these must be students of heroic stature. Thus, for example, one exercise consists of preparing and conducting an attitude survey of white-collar workers. Another consists of writing the history of a local industrial union. Selected annotated bibliographies of varying quality are given at the end of each chapter. There is no reference to wage theory in the bibliography on that chapter; the chapter including union critique of time study does not refer to Gomberg's basic work; the basic book by Turnbull, Williams and Cheit is not mentioned in connection with economic security, nor is Murray Latimer referenced in connection with GAW. In general, the numerous exhibits scattered through the text are helpful and of good quality.

The limited analysis provided by this book is based too frequently upon pontifical assertions and stereotyped descriptions. For example, "Wages are not and have never been the central issue for the factory employee" (p. 173); "The management man is likely to be a believer in . . . rugged individualism" (p. 244). It is suggested that the different approaches of unions and management reflect differences in "mentality" (p. 244).

Several topics are discussed inadequately, including economic versus unfair labor practice strikes; prevention of disputes; grievances, mediation and fact-finding; employment stabilization; arguments pro and con for industry-wide bargaining; and union security. Public policy aspects of collective bargaining typically are not discussions of social issues but rather emphasize impacts upon collective bargaining practices. Although the preface promises a discussion of the problem of labor racketeering, this section of the book is puny. The sections on union and employer policy could be strengthened. Addition of the concepts of agency-management, and unions as quasipublic institutions, would be helpful.

In summary, this book is descriptive rather than analytical, and chock-full of oversimplified generalities. It is organized around the premise that there are demonstrable differences between craft and industrial unions, and that these differences are important to the understanding, and the practice, of collective bargaining. The book is frankly oriented toward the pragmatic or practical reader. It provides some good descriptions of institutions and processes. It appears to be aimed at the elementary student in the technical course and night school students, rather than the would-be professional or professional labor economist, or industrial relations specialist. This volume will appeal most to those whose courses emphasize practice and the "practical" aspects of collective bargaining.

HERBERT G. HENEMAN, JR.

University of Minnesota

Labor Economics and Industrial Relations. By DALE YODER and HERBERT G. HENEMAN, JR. Cincinnati: South-Western Publishing Co., 1959. Pp. x, 726. \$6.75.

The usual question posed to the author (or prospective author) of a new text is "How does it differ from other works in the field?" The distinguishing characteristic of the book at hand is its emphasis on manpower management, the application of human resources in ways which develop and use their greatest skills and potentialities. Both authors, but particularly Professor Yoder, have published more advanced volumes on manpower management, so that the emphasis in this introductory text on that subject comes as no surprise. The book, therefore, does not focus the student's attention on unions and collective bargaining to the extent that most basic labor texts do.

The organization of the material begins conventionally enough with an introductory chapter followed by a discussion of the labor force. Part II deals with the institutional setting "in which human resources are allocated, used, and conserved." The institutions discussed are the labor market, unions, employers, and the government. The remainder of the book, with the exception of a summary section, is divided into four types of current labor problems: industrial unrest and conflict, employment, wages, and economic insecurity.

It is particularly in the chapters dealing with employment problems that the emphasis on manpower management is evident. The chapters on wage problems include one on historical wages, another on marginal productivity and bargaining wage theories, and a third on full-employment wage theory. The last mentioned chapter turns out to be primarily a discussion of national income determination, in which Keynes' views on wages and employment are dealt with too briefly and without sufficient clarity. This latter problem is not helped by an apparent error on page 519 in the following statement (*italics added*):

. . . Keynes notes that public policy may choose between the alternatives of a fixed price level and higher wages or a *rising* price level and stable wages.

The footnote reference is to pages 270-71 of *The General Theory*, where Keynes speaks of a *falling* price level and stable wages.

In the preface, the authors speak of two goals the text seeks to accomplish: to serve (1) as a general introduction to the broad field of modern industrial relations, and (2) as a background in citizenship training for those who take no additional courses in the field. In the opinion of the reviewer, the book is more suitable for the first objective than for the second.

The authors use a great amount of descriptive material, and they present it reasonably well. In any field of applied economics a certain minimum of such information is essential. For prospective practitioners of industrial relations, detailed descriptive material and frequent use of examples may be both necessary and worth while. However, for citizenship training, excessive information of this type may interfere with presentation of the analytical framework, which, it is hoped, will help the reader gain some insight into both current and future labor problems.

Another aspect of the book which tends to interfere with a clear development of the analysis of labor problems is the authors' apparent assumption that the student has had no previous work in economics. As a result considerable space is devoted to detailed explanations of the most elementary concepts of economics. Often this results in diverting the flow of thought from the discussion of the labor problem under study. Even in schools of business, most students are expected to take economic principles, usually before taking courses in industrial relations or labor problems. It would seem reasonable for the author of a text on labor economics and industrial relations to assume that the student has had some introduction to economics. On that assumption, preliminary exposition for the analysis of particular labor problems can be handled more expeditiously than is done in this text.

This volume is likely to be of most interest to schools of business or institutions specializing in industrial relations, since it does a good job of tying in manpower management with labor economics.

ROBERT R. FRANCE

University of Rochester

Population; Welfare Programs; Standards of Living

Population and Progress in the Far East. By WARREN S. THOMPSON. Chicago: University of Chicago Press, 1959. Pp. ix, 443. \$7.50.

Once again Warren S. Thompson has given us a broad survey of the "population problem" of Asia. As in his earlier books, *Danger Spots in World Population* (1929) and *Population and Peace in the Pacific* (1946), he is concerned with the threat of population growth to economic progress and world peace. The body of the work is a series of chapters assessing population, agriculture and industrialization in Japan, India, China, Pakistan, Ceylon, South-east Asia, Taiwan and Korea. They form the basis in turn for a gloomy forecast of Asia's prospects for raising living standards during the next generation, and a prediction that political tensions will rise as aspirations remain unfulfilled.

The volume can be recommended especially to college students and other nonspecialists seeking to orient themselves broadly in the demographic and economic affairs of Asia. The most authoritative sections are those reviewing trends in fertility, mortality and natural increase. Country by country the Malthusian realities of the scene are laid bare: birth rates lingering at 40-45; death rates dropping to 25 and below. Asia's population generally (excepting that of Japan) is expected to grow at a rate of 1.5 per cent or more for at least another generation.

Despite this prospect, says the author, few Asian leaders are yet committed to any determined, massive effort to bring birth rates under control. He may be a little less than just to the Indians in this regard. On the other hand, Peiping is credited with more purpose here than recent reports bear out. He himself refrains from any specific proposals as to how to overcome the formidable cultural and economic barriers to widespread family limitation. This is regrettable in view of his long study of the problem.

The economic analysis that accompanies these population projections is necessarily less rigorous and conclusive. Thompson interweaves fact and argument to support his view that development programs of the magnitude now prevailing in most Asian countries are unlikely to raise living standards substantially in the near future. This may well be true. At a number of points, however, his generalizations seem too sweeping, and sometimes unnecessarily pessimistic. For example, he cites "the strong movement toward autarchy" (p. 398) as foreclosing any significant relief of population pressure from international trade in food, machinery or other essentials. This leads him virtually to write off Japan's economic future, despite her remarkable comeback since the war. As for Communist China, his skepticism may be warranted. But the truth is that we still lack the data to assess her record since 1949, still more to predict her future. The author's chapters on China are less persuasive than those on India and the rest of southern Asia. He has some telling criticisms of India's complacency over her agricultural problem.

Mounting political tensions throughout Asia are bound to grow out of economic frustration, the author feels. In particular, he fears that disappointment among Asian peoples over their continuing poverty will arouse resentment over their relative disadvantage in land and other resources per capita. It is suggested that Japan may thus be led to try to seize New Guinea; China may move south into the unused lands of the Indo-chinese peninsula; India may cast covetous eyes on Burma or Madagascar.

Such suspicions seem gratuitous at the present juncture. In any event they are supported here with little historical or theoretical analysis. Even the case of Japan, so often cited, hardly bears out the "have not" theory of aggression. Had Japan possessed more rice land, or oil and iron a generation ago, would her militarists have been less inclined to territorial conquest, or her people less readily led and coerced down the road to war? It is easier to believe that more resources at home would have enabled Japan to conquer her neighbors more handily. So it may be with Mao's China today. Thompson himself remarks that the Peiping regime is ridden with an ideology that is tense and aggressive, regardless of the resources at its command. Significantly, the greatest international seizures of wealth and income since the second world war have been the depredations of the Soviet Union in Eastern Europe, through conquest, reparations and other levies, despite the great Russian frontier to the east.

This is not to say that economic pressures may not contribute to political tension and disorder. Nor is it to dismiss international inequality in wealth and income as of no concern to world peace. On the contrary, it poses one of the great dilemmas in building a stable world society based on liberal principles. Thompson is essentially saying in this book that it will become still more intractable if unrestrained breeding among non-Western peoples widens further the disparities that already exist. On this no one can disagree.

WILLIAM W. LOCKWOOD

Princeton University

Health Insurance. By O. D. DICKERSON. Homewood, Ill.: Richard D. Irwin, 1959. Pp. xvi, 500. \$6.00.

This book is intended to serve as a text in college courses and in training programs sponsored by insurance organizations. Its 19 chapters discuss, first, the general problem of ill health and the types of insurance developed to meet this risk, then the expense and income coverages presently available and, finally, the intricate operations of health insurers. Industry "codes," uniform policy provisions, etc. are included in appendices. Although the facts are well organized and clearly presented and the bibliography is reasonably complete, the treatment of certain crucial "problems and issues" is less than satisfactory.

Old-age, survivors and disability insurance is fully described, for example, and the author acknowledges that social insurance is indeed insurance, a viewpoint not shared throughout the industry nor even the Ways and Means Committee. The "disability rider" available through the Veterans Administration is, however, overlooked. The author laments the paucity of morbidity data (p. 359 *et seq.*) but is apparently unaware of the National Health Survey, a nation-wide monthly sampling survey begun in 1957 by the Public Health Service that is complementary and analogous to the well-known Current Population Survey of the Bureau of Census. And, despite their manifest relevance, the book's index lists neither "collective bargaining," "fringe benefit" nor "labor union." The omissions are the author's, not the indexer's.

More significant matters are also handled cavalierly or superficially. With respect to health insurance after age 65, the author merely concludes that "adequate coverage will presumably have to wait for development of policies which provide for funding the benefit costs throughout the insured's working life" (p. 333). Not from this text will the student learn that extension of old-age, survivors and disability insurance to provide retirees with hospital and surgical insurance benefits is currently the most controversial question in this field. Even more serious is the scanty discussion of overutilization (pp. 136-41, 187-90). The author ignores the substantial evidence that prepayment of physicians' services for outpatient diagnosis and early treatment tends to reduce the incidence of hospitalization. This has been the experience of the consumer-sponsored plans (so summarily dismissed on p. 188) that offer the best alternative to further government intervention into the market for health insurance.

Dickerson's work is welcome, but it should be considered only a first approximation of a thorough college-level text on the subject of health insurance. Future efforts should emphasize the disparity between the demand perceived by demographers and the supply offered by health insurers. Morbidity data indicate increasing long-term illness and the advantage of preventive services; as yet, most policies cover only short-term illness and emergency treatment. There may still be time for private, voluntary health insurance to redress the balance, but the hour grows late.

THEODOR SCHUCHAT

Washington, D.C.

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Population; Welfare Programs; Standards of Living

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Related Disciplines

- TULLOCK, G. Problems of majority voting. *Jour. Pol. Econ.*, Dec. 1959, pp. 571-79.

NOTES

George W. Stocking, of Vanderbilt University, has been appointed chairman of the American Economic Association nominating committee. Suggestions for officers of the Association in 1961 should be sent to him as early as possible.

John Perry Miller, editor of the forthcoming *Cumulative Index to Economic Periodical Articles in the English Language 1886-1959*, is now editing author entries and would like to have the correct form of names that have undergone change. Such changes—for example, married women who have published under maiden names, “juniors” that have been dropped, variant spellings—should be sent to Mrs. Dorothy F. Livingston, Yale University Library, New Haven, Connecticut.

THE ECONOMICS INSTITUTE FOR FOREIGN STUDENTS

In the summer of 1960 (June 27 to August 28) the third Economics Institute will be held at the University of Colorado with Wyn F. Owen as Director. The Economics Institute is designed to refresh and, where necessary, to supplement foreign member students' preliminary training in economics, English, and mathematics, so that they may begin their graduate studies on a basis more nearly comparable to that of students whose undergraduate training has been obtained in U.S. colleges or universities. The testing program of the Institute clearly reinforces the judgment of its founders that foreign students commonly begin graduate work in the United States at a very considerable handicap, and that this handicap goes beyond the matter of language. Even though the Institute in 1959 was attended predominantly by students who had been awarded scholarships for the regular academic year, only a small minority demonstrated subject-matter background of a quality likely to carry them through an initial semester of graduate work without academic difficulty.

The program is being administered by the Institute of International Education, with the guidance of an advisory board nominated by the American Economic Association. Current members of the board are Rendigs Fels, Carter Goodrich, Michael Hoffman, D. Gale Johnson, Irving Kravis, and Lorie Tarshis.

Information relating to student fellowships may be obtained from departmental chairmen and foreign student advisers.

1960 SURVEY OF CONSUMER FINANCES

The Survey Research Center of the University of Michigan is conducting the 1960 Survey of Consumer Finances with the support of private business and part of a \$300,000 grant from the Ford Foundation. Conducted annually since 1946, the Survey has previously been supported entirely by the Federal Reserve Board.

One part of the Ford Foundation grant is for the purpose of making Survey of Consumer Finances data more readily available to the academic world and more useful for tests of economic theory. An interuniversity facility, administered by an executive board, has been set up to prepare data from the annual Survey for research at other universities and to arrange a series of summer workshops for teachers and researchers. The first Summer Workshop on the Use of Consumer Survey Data will be held from July 18 to August 13, 1960.

The Center will also conduct a special study of low-income families next spring with support from the Ford Foundation, supplemented by the Office of Education and the Office of Vocational Rehabilitation.

BOOKS FOR ASIAN STUDENTS

The Asia Foundation would like to have donations for its Books for Asian Students program, now in its fourth year. Through this program, the Foundation has shipped more than a million books and almost a quarter million journals to universities and libraries in eighteen Asian countries. University, college and secondary level books (published after 1945) and scholarly, scientific and technical journals in series of five or more years are needed. The Foundation will pay transportation costs from the donor to San Francisco and thence to Asia. All shipments or questions concerning the program should be addressed to: Books for Asian Students, 21 Drumm Street, San Francisco 11, California.

Deaths

- Arthur B. Adams, University of Oklahoma, August 10, 1959.
 James H. Alphen, December 23, 1959.
 Eugen Altschul, University of Kansas City, April 26, 1959.
 Richard T. Bohan, United States Internal Revenue Service, July 21, 1959.
 Bertram J. Cahn, August 11, 1959.
 Ralph L. Dewey, Ohio State University, September 27, 1959.
 Ralph C. Epstein, University of Buffalo, November 21, 1959.
 Henry D. Locke, Liberty Mutual Insurance Company, October 18, 1959.
 W. Rupert MacLaurin, Massachusetts Institute of Technology, August 17, 1959.
 Marvel M. Stockwell, University of California, Los Angeles, December 15, 1959.

Retirements

- John B. Condliffe, University of California, Berkeley, December 1958.
 Paul F. Gemmill, Wharton School, University of Pennsylvania, June 1959.
 Brantson B. Holder, Washington and Lee University, June 1959.
 Carl Landauer, University of California, Berkeley, June 1959.
 Benjamin F. Lemert, Duke University, Spring 1960.

Visiting Foreign Scholars

Michael Beesley, University of Birmingham: visiting associate professor of industry, Wharton School, University of Pennsylvania.

John R. Hicks, All Souls College, and Ursula K. Hicks, Nuffield College: lecturing at the University of California, Los Angeles, and visiting Stanford University and the University of California, Berkeley, March 1960, en route from Oxford to Japan to lecture at Osaka University.

Murray C. Kemp, McGill University: visiting associate professor of economics, Massachusetts Institute of Technology, 1959-1961.

Frederic Schreier: visiting professor of marketing, University of California, Los Angeles.

Ingvar Svennilson: visiting research professor of economics, Yale University.

Shitego Tsuru, Hitotsubashi University, Japan: visiting professor, Johns Hopkins University, first term, 1960-1961.

Leslie C. Wright, Edinburgh University: visiting Whitney professor economics, Rice Institute.

J. N. Wolfe, University of Toronto: visiting professor of economics, Purdue University. •

Promotions

- Morris A. Adelman: professor of economics, Massachusetts Institute of Technology.
- Robert L. Ashenurst: associate professor of applied mathematics, Graduate School of Business and the Institute for Computer Research, University of Chicago, July 1960.
- Edward C. Atwood, Jr.: associate professor of economics, Washington and Lee University.
- A. J. Barkley: associate professor of economics, North Carolina State College.
- Jean A. Crockett: associate professor of finance, Wharton School, University of Pennsylvania.
- John L. Davidson: assistant professor of management and marketing, Louisiana State University.
- Harold Demsetz: assistant professor of economics, University of Michigan.
- Franklin B. Evans: assistant professor of marketing, Graduate School of Business, University of Chicago.
- Paul J. FitzPatrick: ordinary professor of economics, Catholic University of America.
- Arthur M. Freedman: assistant professor of finance, Wharton School, University of Pennsylvania.
- Francis W. Gathof: assistant professor of economics, American University.
- Bruce Glassburner: associate professor of economics, University of California, Davis.
- Everett E. Hagen: professor of economics, Massachusetts Institute of Technology.
- Reed R. Hansen: assistant professor of economics, School of Economics and Business, Washington State University.
- Dominic N. Khactu: assistant professor of economics, Drake University.
- R. V. Lesikar: professor of management and marketing, Louisiana State University.
- Harold M. Levinson: professor of economics, University of Michigan.
- Russell F. McDonald: instructor (research), Ohio State University.
- S. L. McDonald: professor of finance, Louisiana State University.
- Gerald M. Meier: professor of economics, Wesleyan University.
- Sidney L. Miller: assistant professor of transportation, Wharton School, University of Pennsylvania.
- Simon Naidel: professor of economics, American University.
- Dick Netzer: assistant vice president, Federal Reserve Bank of Chicago.
- Paul Pigors: professor of industrial relations, Massachusetts Institute of Technology.
- Raymond P. Powell: associate professor of economics, Yale University.
- James P. Quirk: assistant professor of economics, Purdue University.
- Chester Rapkin: professor of finance and city planning, Wharton School, University of Pennsylvania.
- Robert M. Reeser: instructor (teaching and research), Ohio State University.
- Paul N. Rosenstein-Rodan: professor of economics, Massachusetts Institute of technology.
- Rubin Saposnik: assistant professor of economics, Purdue University.
- Ronald A. Shearer: assistant professor of economics, University of Michigan.
- Abraham J. Siegel: associate professor of industrial relations, Massachusetts Institute of Technology.
- Warren L. Smith: professor of economics, University of Michigan.
- Benjamin H. Stevens: assistant professor of regional science, Wharton School, University of Pennsylvania.
- Daniel B. Suits: professor of economics, University of Michigan.
- Morton Zeman: supervisor, economic research section, IBM Corporation, New York City.

Administrative Appointments

Robert C. Beetham: director of program, United States Council, International Chamber of Commerce.

Allan M. Cartter: dean, Graduate School of Arts and Sciences, Duke University.

Joseph L. Fisher: president and executive director, Resources for the Future, Inc., succeeding Reuben G. Gustavson, who retired August 1959.

Curry W. Gillmore: promoted to director of statistics and economics, IBM Corporation, New York, N. Y.

Craufurd Goodwin: executive secretary, Commonwealth Studies Center, Duke University.

Floyd A. Harper: research director, Foundation For Voluntary Welfare, Burlingame, California.

John J. Hooker: head of department of economics, Catholic University of America.

Paul Keat: promoted to supervisor of the Statistics Section, IBM Corporation, New York, N. Y.

H. T. Koplin: acting director, Honors College, University of Oregon.

Francis J. Lacy: associate survey director, Opinion Research Corporation.

David W. MacEachron: assistant manager, Intergovernmental Economic Affairs Department, California Texas Oil Corporation.

Stuart L. Mandell: head of department of economics and management, Lowell Technological Institute.

Dan M. McGill, Wharton School, University of Pennsylvania: executive director, S. S. Huebner Foundation for Insurance Education.

S. Sterling McMillan, Western Reserve University: secretary-treasurer, Reliable Spring and Wire Forms Company.

John L. O'Donnell: assistant director, Bureau of Business and Economic Research, Michigan State University.

Richard Ruggles: chairman, department of economics, Yale University.

Kenneth P. Sanow: promoted to program director, Industry Surveys, National Science Foundation, Washington, D.C.

Sheldon Schaffer, Metals Division, Olin Mathieson: manager of industrial economics, Southern Research Institute, Birmingham, Alabama.

Arthur Smithies: chairman, department of economics, Harvard University.

Jack Stieber: director of the Labor and Industrial Relations Center, Michigan State University, and professor of economics.

Frank T. Stockton: executive secretary, Kansas State Interdepartmental Committee on Aging.

Ben B. Sutton: vice president, Apache Oil Corporation.

Ernst W. Swanson: professor and head of department of economics, North Carolina State College.

Fred R. Yoder, Washington State University: chairman, Division of Social Sciences, Campbellsville College, Kentucky.

Appointments

Wroe Alderson: professor of marketing, Wharton School, University of Pennsylvania.

Jakangir Amuzigar, Occidental College: Brookings research professor, 1960-1961.

Dole A. Anderson: professor and advisor, ICA-Michigan State University project to organize School of Business Administration, University of Bahia.

Robert Armstrong: lecturer in economics and business administration, Geneva College.

Henry G. Aubrey: research fellow, Council on Foreign Relations.

Bela Balassa: assistant professor of economics, Yale University.

- Morton S. Baratz, Bryn Mawr College: Brookings research professor, 1960-1961.
- Donald S. Barnhart: lecturer in economics, Wharton School, University of Pennsylvania.
- Howard Baumgarten: corporate economist, Crown Zellerbach Corporation.
- Arthur A. Brennan, Jr.: instructor in statistics, Wharton School, University of Pennsylvania.
- Robert L. Bunting: associate professor of economics, North Carolina State College.
- Carl Campbell: assistant professor of economics, Whitman College.
- Neil W. Chamberlain: professor of economics, Yale University.
- William H. Chartener: economist, senior staff, Stanford Research Institute.
- Manuel H. Chavez: instructor in insurance, Wharton School, University of Pennsylvania.
- Arnold E. Crotty, University of North Carolina: instructor in accounting, School of Business Administration, University of Miami, Florida.
- Paul G. Darling, Bowdoin College: Brookings research professor, 1960-1961.
- Philip Davidowitz: research associate, Harvard University Graduate School of Business Administration.
- Dennis Davin: instructor in economics and business administration, Whitman College.
- Frederick H. Ecker: professor of life insurance, Wharton School, University of Pennsylvania.
- David Eiteman: assistant professor of finance, Graduate School of Business Administration, University of California, Los Angeles.
- Stephen Enke: visiting professor of economics, Yale University.
- Robert Evans, Jr.: assistant professor of industrial relations, department of economics, Massachusetts Institute of Technology.
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Glenn W. Miller, Ohio State University: visiting professor of economics, University of Arizona.

Thomas E. Miller: associate professor, School of Business, University of Kansas.

Roy E. Moor, Williams College: Brookings research professor, 1960-1961.

Roger Opdahl: instructor in economics and business administration, Geneva College.

Charles F. Phillips, Jr.: assistant professor of economics, Washington and Lee University.

Gerald A. Pinsky, Columbia University: instructor, Dartmouth College.

Harold L. Royer, Southwest Missouri State College: assistant professor of accounting, School of Business Administration, University of Miami, Florida.

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Paul F. Smith: associate professor of finance, Wharton School, University of Pennsylvania.

David Snell: assistant professor of finance, Graduate School of Business Administration, University of California, Los Angeles.

David Solomons: professor of accounting, Wharton School, University of Pennsylvania.

Benjamin P. Spiro: senior finance analyst, international economic group, Stanford Research Institute.

Case M. Sprenkle: instructor in economics, Yale University.

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William Stober: instructor in economics, North Carolina State College.

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J. S. G. Wilson, London School of Economics: chair in economics and commerce, University of Hull.

Leaves for Special Appointments and Assignments

Edward Ames, Purdue University: visiting associate professor, Johns Hopkins University, 1959-1960.

Arthur M. Borak, University of Minnesota: University of Korea, 1959-1960.

Colin D. Campbell, Dartmouth College: tax economist, Brookings Institution's Korean Tax Advisory Group, 1959-1961.

John S. Chipman, University of Minnesota: Duke University, 1959-1960.

Bruce Glassburner, University of California, Davis: chairman, field staff, Indonesian economics project, University of California-University of Indonesia.

Bert G. Hickman, Brookings Institution: visiting lecturer, University of California, Berkeley, spring semester 1960.

Francis B. May, University of Texas: visiting professor, University of Minnesota, 1959-1960.

Lawrence S. Ritter, New York Federal Reserve Bank: staff of The Commission on Money and Credit, 1959-1960.

Wilson E. Schmidt, George Washington University: visiting professor of economic development, Institute on ICA Development Programming, School of Advanced International Studies, Johns Hopkins University.

Karl U. Smith, University of Minnesota: visiting professor of business administration, Indiana University, second semester 1959-1960.

Mervin G. Smith, Ohio State University: visiting professor, Iowa State University, (Ames), second semester 1959-1960.

John G. Turnbull, University of Minnesota: visiting professor of economics, Iowa State University (Ames), spring term.

Dilworth Walker, University of Utah: to head Institute of Public Administration, University of Tehran, 1960-1962.

L. Leslie Waters, Indiana University: Rose Morgan visiting professor, University of Kansas, second semester 1959-1960.

Stanislaw H. Wellisz, University of Chicago: visiting professor of economics, Warsaw School of Planning and Statistics, and seminars, University of Warsaw, winter quarter 1960.

Resignations

Paul Donham: Graduate School of Business Administration, Harvard University.

Harold J. Ecker: Ohio State University.

James J. Foley: Graduate School of Business Administration, Harvard University.

Laurent R. La Vallee: Whitman College.

Edward Rosenbaum: American University.

Paul A. Vatter: Wharton School, University of Pennsylvania.

Miscellaneous

William L. Doremus, New York University: national chairman, Advertising Teacher's Committee, American Marketing Association.

Clifford Hildreth, Michigan State University: editor, *Journal of the American Statistical Association*.

Fritz Machlup: president, Southern Economic Association, 1959-1960.

M. G. Taylor, University of Toronto: president, Canadian Institute of Public Administration.

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Manuscripts and editorial correspondence relating to the regular quarterly issues of this REVIEW should be addressed to Bernard F. Haley, Managing Editor of THE AMERICAN ECONOMIC REVIEW, Stanford University, Stanford, California. *Style Instructions* for guidance in preparing manuscripts in acceptable form will be provided upon request to the editor.

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NUMBER THREE

AN INDEX OF SOVIET INDUSTRIAL OUTPUT

By NORMAN M. KAPLAN AND RICHARD H. MOORSTEEN*

In this paper we present (a) the results of a calculation of an index of Soviet industrial output, and (b) some comments on, and applications of, these results. Underlying this paper are two studies: one on machinery prices and production [6] and another, which incorporates the relevant results of the machinery study, on the output of all industrial products [4]. This paper is a summary of the second of these studies and, as such, omits the detailed documentation and explanations which are available there or which, with respect to the machinery component, will be available when the machinery study is published. The results, shorn of important details, are presented here because the topic appears to be of some current interest. The results are, however, tentative for a variety of reasons; some of them are indicated below but others appear only in the underlying materials.

In Section I we present the index of industrial output and briefly discuss its nature and limitations. Section II is a miscellany in which we: (a) compare our index with others; (b) discuss some dynamic aspects of Soviet industrial growth; (c) explore the indications of future rates of growth; (d) present an index of the output of final products; and (e) estimate changes in productivity.

I. *The Index*

Table 1 presents the computed index for all industrial products and for major components thereof; the last column presents the weights employed in obtaining the index from the subindexes for the major components. The index is intended to measure the net product of industry, i.e., gross output less materials consumed in mining, manufacturing, and electric power production. At various stages in the aggregation, however, important compromises and approximations were neces-

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TABLE 1—AN INDEX OF SOVIET INDUSTRIAL OUTPUT
(1950=100)

	1927/ 28	1932	1937	1940	1945	1950	1951	1952	1953	1954	1955	1956	1957	1958	Weight for Commodity Group (percentage)
All Industrial Products	27.1	41.6	67.4	71.2	36.6	100.0	111.5	118.9	130.3	143.6	158.1	171.7	188.4	202.3	100.0
1. Machinery	6.8	19.5	40.9	34.3	13.6	100.0	98.5	100.2	111.4	120.7	136.2	150.6	175.1	185.0	19.4
2. Other producers' goods	21.4	41.1	66.5	71.5	43.1	100.0	112.7	121.5	131.2	145.0	160.1	173.2	189.8	206.8	54.8
a. Ferrous metals	15.7	24.5	66.1	68.0	43.3	100.0	114.2	127.5	140.5	153.2	167.9	179.9	189.7	202.7	9.5
b. Fuels	21.5	39.8	61.6	73.1	54.5	100.0	109.8	119.4	130.1	143.9	167.2	190.2	216.5	242.0	14.4
c. Electric power	5.5	14.8	39.7	53.0	47.5	100.0	114.0	130.6	147.4	165.1	186.6	210.2	229.9	255.9	1.5
d. Chemicals	6.9	17.8	52.6	56.5	25.4	100.0	110.6	119.5	130.1	146.0	165.3	182.2	198.6	214.3	3.3
e. Lumber, wood products and paper	28.7	56.8	72.9	78.7	42.5	100.0	113.1	116.5	120.5	132.7	139.1	143.8	153.2	161.8	19.9
f. Building materials	18.8	37.6	72.3	62.9	26.0	100.0	116.7	132.6	150.3	169.3	190.4	204.7	232.0	260.5	6.1
3. Consumers' goods	54.3	59.4	89.1	98.3	40.2	100.0	118.8	127.3	142.9	157.9	170.3	184.2	195.4	205.7	25.8
a. Foods	66.7	70.0	104.6	109.2	49.4	100.0	115.0	124.0	136.2	141.9	156.7	168.7	180.3	187.9	10.0
b. Nonfoods	46.4	52.7	79.4	91.4	34.4	100.0	121.2	129.5	147.1	167.9	178.9	194.0	204.9	217.0	15.8

Source: [4, Table 22].

sary: the index is computed by aggregating physical output series at various stages of fabrication, using 1950 price weights within certain groups of commodities and 1950 wage-bill weights among these commodity groups. Though limitations of space preclude a full account and appraisal,¹ we will describe, and comment briefly on, each aspect of the computation: (a) the underlying physical output series, (b) the first stages of aggregation, and (c) the higher levels of aggregation.

A. *The Physical Output Series*

Except with respect to machinery, no extensive survey of Soviet sources has been attempted and the present study relies primarily on the output data provided by the recent Soviet statistical handbooks—particularly by [15] and [16]. The most important but inevitable omission from our output sample is munitions and other military end-items. Probably the most important in the long list of other omissions are chemical equipment, nonferrous metals, refined petroleum products, sewn goods, nonalcoholic beverages, bread and bakery products, furniture.²

For a number of the important omissions other than munitions, we have attempted by imputation to employ surrogate commodities. That is to say, by appropriate choice of the weights, we let ferrous metals stand for nonferrous metals as well, textile fabrics for sewn goods as well, and similarly in other cases. Though each of these imputations undoubtedly gives rise to errors, the errors are believed to be less than those involved in no imputations, for the latter would imply a relative increase in output for the excluded commodities which is equal to that for all included commodities.

In addition to the omissions, there are a number of other difficulties with respect to the physical output series which we have been unable to allow for in the index computation. Probably the most important are the following: (1) The output data for the various periods refer to the

¹ For a detailed statement of sources and methods and for comments on the results, see [4, pp. 1-73 and Tables 1-22].

² All in all, the machinery index includes 210 physical output series; other producers' goods, 36 physical output series; and consumers' goods, 37 physical output series.

Not all the output series available from the statistical handbooks have been included in the index; some series do not extend over all years of interest and others were omitted because of the absence of appropriate weights. An alternative calculation was made incorporating the additional output series available for the 1950-1955 period and for which weights could be obtained; the result was a small but perceptible increase in the 1955 index with 1950 = 100 [4, Tables 14 and 15].

Also, many of the physical output series are themselves aggregates within which additional output data are available for some years. For example, the index includes pig iron output in tons and the statistical handbooks also provide data on six varieties of pig iron—cast iron for steel, foundry iron, spiegeleisen, etc. An alternative calculation for the 1950-1955 period, which incorporated the maximum detail available, yielded essentially no change in the index [4, Tables 10 and 11].

respective territorial boundaries of those periods. Hence, part of the 1927/28-1940 increment in output represents the 1939-1940 expansion in territory. (2) Some, though far from all, of the consumers' goods series exclude the 1927/28 output of small-scale industry. Hence, the 1927/28 value of the index is understated.

B. *The First Stages of Aggregation*

The weights by which the physical output series are combined into indexes for machinery and for the indicated commodity groups within other producers' goods are, or are intended to approximate, Jan. 1, 1950 wholesale prices. Prices of this date were chosen (a) because wholesale prices during the 1950's are believed to correspond at least roughly with production costs as reckoned in Soviet accounts, and (b) because previous studies of Soviet prices provide more abundant information for Jan. 1, 1950 than for any later date.

In the absence of wholesale price data for consumers' goods, the indexes for foods and nonfoods are obtained by weighting the relevant output relatives by the 1950 value of retail sales or approximations thereto. When these weights are multiplied by the corresponding output relatives, the results differ from the producers' goods values (price times output) in two ways: (a) the values are retail rather than wholesale;³ and (b) they differ from a price-weighted output series to the extent that quantities *sold* differ from quantities *produced*.

Neither set of weights is available in sufficient detail or extent to correspond exactly with all the output series used. Hence, various approximations were often used: (a) estimates of average wholesale prices for output series more aggregative than the commodities for which price information exists; (b) the adjustment to Jan. 1, 1950 of prices of another date by the use of price index numbers; (c) the substitution of the product of a 1950 retail price and 1950 output for retail sales in cases where the latter are unavailable.

C. *The Higher Levels of Aggregation*

We regard an index of the net product of industry—i.e., of gross output less materials consumed—as an appropriate general-purpose measure of changes in the level of industrial activity. It is a measure which in principle (a) avoids the double-counting of gross outputs, (b) is in-

³ However, as will be indicated below, we have used our weights to combine official Soviet indexes for branches of industry into indexes for foods and for nonfoods in the 1950-1955 period; the results are very close to the official indexes for foods and nonfoods. Since the official indexes use wholesale price weights, these results suggest that relative retail prices within foods and within nonfoods are not sufficiently different from wholesale prices to yield appreciable differences in the output indexes, at least in the 1950-1955 period. See [4, Table 26].

sensitive to changes in vertical integration, and (c) eliminates the contribution of nonindustrial sectors, in the form of materials supplied, to industrial output.

Unfortunately, we have not found it possible to construct such a measure directly for lack of adequate data. Instead, we have sought an approximate indirect measure—an index of output with value-added weights.⁴ And even the latter is not properly constructed. We do not have value-added weights but must rely on wage-bill distributions as approximations. Furthermore, the wage-bill distributions cannot be applied at all levels of aggregation but are used only to obtain (a) the index for consumers' goods from the food and nonfood indexes, (b) the index for producers' goods other than machinery from the indicated sub-indexes, and (c) the index for all industrial products from the indexes for machinery, other producers' goods and consumers' goods.

The relevant wage-bill distributions, obtained from the 1950 distribution of wage-earners by industrial branches and estimates of 1950 average monthly earnings, are summarized in the last column of Table 1. The most important of the difficulties encountered in the derivation of the wage-bill distribution is the necessity to exclude from the data for the machine-building and metal-working industry that portion which represents payments to workers producing munitions. On the basis of somewhat ambiguous official data on the distribution of the gross value of output, we have taken the share of munitions workers in the machine-building and metal-working wage-bill to be 50 per cent. Though this is a rough calculation, substantially different alternative shares do not greatly affect the index for all industrial products: as against our index of 202 in 1958 and 27 in 1927/28, alternative shares of 25 and 75 per cent for munitions yield, respectively, indexes of 201 and 204 in 1958 and indexes of 25 and 29 in 1927/28.

Thus, the index in Table 1, conceived as a measure of the changes in the net product of civilian industry, is subject to errors which arise from various considerations: (1) ambiguities in, and exclusions from, the physical output series; (2) inaccuracies and imputations in the price, retail sales, and wage-bill weights; (3) the heterogeneity of the weighting system as a whole; and (4) the approximate nature of a value-added-weighted index.⁵ In most cases, neither the magnitude nor

⁴ Given complete commodity coverage, the direct and indirect measures will be identical if, for each commodity produced by industry, value added (gross value minus materials consumed, both at weight-year prices) varies over time in direct proportion to the output of that commodity. However, to the extent that real cost reductions (increases) have occurred in materials requirements, a value-added-weighted index will understate (overstate) the growth of net product.

⁵ Also, for some of the years covered in Table 1, the indexes for some commodity groups are obtained by interpolations or extrapolations based on sets of commodities which are

the direction of error is known. In some cases, however, at least the direction of error is clear and these cases suggest that our index understates the 1950-1958 increase in output and overstates the 1927/28-1950 increase.

The overstatement arises from our exclusion of some small-scale industry output for 1927/28 and, in terms of growth rate comparisons, from our inclusion of the output in territories added in 1939-1940. The understatement of the 1950-1958 increase arises primarily from a deficiency in the post-1950 machinery index.⁶ In the machinery index an attempt is made to allow for changes over time in the average quality of the products included within any single physical output series. Though precision in this aspect of the calculation is hardly possible, the problem of allowing for quality change in the 1927/28-1950 period is not so acute. Machinery output then was characterized by considerable standardization; and changes in quality, although large, tended to occur discretely and infrequently for most items. With the beginning of the 1950's, however, the level of technical sophistication in Soviet machine-building rose rapidly, the number of models produced proliferated, and the models changed frequently. Because of an insufficient allowance for post-1950 improvements in the quality of machinery output, the machinery index is believed to understate significantly the actual increase in output from about 1950 on.

II. *Some Implications and Applications of our Results*

A. *Comparisons of Our Indexes with Others*

Our index of industrial output, of course, is not the first computed by students of the Soviet economy: among others, D. R. Hodgman [1], N. Jasny [2], G. W. Nutter [7], F. Seton [12], and D. B. Shimkin

different from those underlying the indexes for other years. The indexes and commodity groups involved are the following: (a) the 1951, 1952, and 1954 indexes for foods and non-foods and the 1958 index for nonfoods; and (b) the 1951-1954 and 1956-1958 indexes for chemicals and building materials and the 1951-1954 indexes for fuels. However, the errors involved in interpolation or extrapolation are relatively small. See [4, Section 3 and Tables 16, 17, 19, and 20].

⁶There is evidence that other factors also cause our index to understate the increase in output over the period as a whole and over the post-1950 period separately. Among them: (a) our omission of nonferrous metals output series; (b) our exclusion of output data available only for recent years; and (c) our use of wage-bill data rather than wage-bill plus capital consumption to represent value-added. See [4, pp. 71-73 and Table 15]. In each of these cases, the error appears to be relatively small, though taken together their effect may be appreciable.

The last of the factors listed above deserves an additional comment. In an effort to test the sensitivity of our results to inclusion of other factor payments, we have performed an alternative, illustrative calculation in which combined capital consumption and wage-bill weights replace the wage-bill weights used in Table 1. The result is an index which increases only slightly more than the index in Table 1 [4, pp. 33-36, 55-56].

and F. A. Leedy [13] have also made such computations. We add our estimates to those already in existence for the following reasons: (a) For various purposes, including the determination of changes in the annual rates of industrial growth, it is desirable to obtain annual indexes for the post-1950 period extending to the most recent years possible. The Hodgman index terminates in 1951, the Jasny index in 1950, and only the Shimkin-Leedy index goes beyond 1955 to 1956. Only the Seton and Shimkin-Leedy indexes provide annual 1950-1955 data. (b) The machinery component of our index, the most troublesome component of an index of industrial output, rests upon an independent and intensive study of machinery prices and production [6]. (c) While the indexes already available differ in results, little by way of published details, except for [1] and [12], are yet available to account for the differences.

Table 2 summarizes the results obtained in these other studies in comparison with ours. In some cases the results differ for more or less obvious reasons. The Hodgman indexes preceded the appearance of the recent Soviet statistical handbooks and hence rely on far fewer output series, particularly for the postwar years. The Seton index is not a

TABLE 2—COMPARISONS BETWEEN KAPLAN-MOORSTEEN INDEXES AND OTHERS
(All Indexes with 1950=100)

	1928	1937	1940	1955
All Industrial Products				
(i) Hodgman	15	57	67	—
(ii) Jasny	21	61	—	—
(iii) Nutter	29	68	66	146
(iv) Seton	14	52	63	165
(v) Shimkin-Leedy	23	63	68	165
(vi) Kaplan-Moorsteen	27	67	71	158
1. Machinery				
(i) Nutter	13	56	42	125
(ii) Shimkin-Leedy	7	38	32	147
(iii) Kaplan-Moorsteen	7	41	34	136
2. Other Producers' Goods				
(i) Nutter	24	65	68	154
(ii) Shimkin-Leedy	17	61	70	163
(iii) Kaplan-Moorsteen	21	67	72	160
3. Consumers' Goods				
(i) Hodgman	39	92	87	—
(ii) Nutter	56	86	93	161
(iii) Shimkin-Leedy	55	87	91	168
(iv) Kaplan-Moorsteen	54	89	98	170

Sources: Hodgman [1, pp. 89, 123, 134]; Jasny [2, p. 57]; Nutter [7, pp. 402, 404-5]; Seton [12, p. 30]; Shimkin-Leedy [13, p. 7]; Kaplan-Moorsteen: Table 1 above. We ignore here the difference between the calendar year 1928, and the fiscal year 1927/8 (Oct. 1, 1927-Sept. 30, 1928).

weighted aggregation of output series but is obtained from a few selected series, regarded as inputs, and a regression equation, relating such inputs and output from data for other countries.

In other cases, however, no ready explanation of the differences is available. The indexes do differ in weights: Shimkin-Leedy use a "modification" of Hodgman's 1934 wage-bill weights, Nutter a combination of 1955 prices and employment distributions, and Jasny "real 1926/27 prices." Though there are considerable areas of agreement among the Shimkin-Leedy indexes, the Nutter indexes, and ours, there are also appreciable and conspicuous differences. If the major differences among these indexes were differences in the *date* of the weights, two consequences would ensue: (a) our results and Nutter's should coincide quite closely, for the difference between 1950 and 1955 weights should yield insignificant differences in the results; and (b) our results should show substantially smaller increases than the Shimkin-Leedy indexes on the grounds that early-year weights (such as 1934) typically produce a more rapidly growing index than do late-year weights (such as 1950 or 1955). Yet our postwar results for all categories of output are closer to the Shimkin-Leedy than to the Nutter indexes, and in the 1927/28-1950 period our results for machinery are almost identical with the Shimkin-Leedy index but far different from the Nutter index.

It is clear that an explanation of the differences in results requires detailed comparisons of the individual output series, weights, and various adjustments employed. Both limitations of space and the absence of published details (including as yet those which underlie our machinery component) make such comparisons infeasible here. Until such comparisons are undertaken, our results and the subsequent applications we make of them should, for these reasons also, be regarded as tentative.

Let us turn now to the comparisons between our results and the official indexes in Table 3. For the 1928-1950 period and for all commodity groups, the official indexes and ours bear essentially no resemblance to each other beyond the fact that both record increases in output: the official indexes show increases from 1928 to 1950 which are from $2\frac{1}{2}$ to 6 times the increases shown by the corresponding Kaplan-Moorsteen indexes. These divergences are not surprising. In the 1928-1950 period, the official indexes are the 1926/27 ruble indexes which should yield increases substantially greater than ours for at least two reasons: (a) the official weights are, in principle, 1926/27 prices which, referring to an essentially "preindustrialization" economy, yield greater increases in output than an index constructed with weights of a recent year; and (b) in the 1928-1950 period, new products were often introduced into the index at essentially current prices—and, because of inflation, higher

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prices—rather than 1926/27 prices, thus imparting an upward bias to the official index. In addition, the official indexes for all industrial products and machinery include, and ours exclude, munitions; and the output of munitions in 1928 was negligible.

Though the divergences themselves are not surprising, three points perhaps deserve some emphasis. First, the divergences pervade all commodity groups in Table 3. Secondly, if the official and Kaplan-Moorsteen indexes for the 1928-1950 period are broken down into subperiod indexes—i.e., 1937 indexes with 1928 = 100, 1940 indexes with 1937 = 100, and 1950 indexes with 1940 = 100—the divergences pervade all commodity groups and all subperiods, except machinery in the 1940-

TABLE 3—COMPARISONS BETWEEN KAPLAN-MOORSTEEN AND OFFICIAL INDEXES^a
(All Indexes with 1950=100)

	1928	1937	1940	1945	1950	1953	1955	1957	1958
All Industrial Products									
(i) Official (including munitions) ^b	9	40	58	53	100	145	185	225	247
(ii) Kaplan-Moorsteen (excluding munitions)	27	67	71	37	100	130	158	188	202
1. Machinery									
(i) Official (including munitions) ^b	2	27	47	60	100	159	217	279	318
(ii) Kaplan-Moorsteen (excluding munitions)	7	41	34	14	100	111	136	175	185
2. Other Producers' Goods									
(i) Official ^c	9	40	52	46	100	139	174	208	—
(ii) Kaplan-Moorsteen	21	67	72	43	100	131	160	190	207
3. Consumers' Goods									
(i) Official ^b	20	61	81	48	100	144	176	208	224
(ii) Kaplan-Moorsteen	54	89	98	40	100	143	170	195	206
a. Foods									
(i) Official ^d	25	78	104	52	100	138	160	192	202
(ii) Kaplan-Moorsteen	67	105	109	49	100	136	157	180	188
b. Nonfoods									
(i) Official ^d	26	67	89	55	100	143	178	200	218
(ii) Kaplan-Moorsteen	46	79	91	34	100	147	179	205	217

^a The comparisons here, and others, for years through 1955 are found in [4, Table 27]. For further details see [4, notes to Table 27, and Section 4.3]. We ignore here the difference between the calendar year 1928 and the fiscal year 1927/8.

^b For all industrial products, machinery, and consumers' goods, the official indexes for, respectively, gross industrial output, machine-building and metal-working output and group B output are used. They are found in [16, pp. 10, 32, and 37], and, for 1957 and 1958, in [15, p. 146].

^c For other producers' goods, the official indexes are of two kinds. (a) In the 1928-1950 period the indexes are obtained by subtracting in each year the value of machine-building and metal-working output in 1926/27 rubles from the value of group A output in 1926/27 rubles. (b) In the 1950-1957 period, the indexes are obtained as averages of official indexes for relevant branches of industry, weighted by our wage-bill weights for those branches. The official branch indexes are found in [15, p. 140].

^d The official indexes for foods and nonfoods are the official indexes for the food and light industries, respectively, from [15, p. 140] and [11, Jan. 16, 1959].

1950 period [4, Table 28]. Thirdly, the entire divergence observed should not be attributed to the "upward bias" of the official indexes if, by "bias," statistical malpractices such as the treatment of new commodities are meant. Part of the divergence, though a quantitatively unknown part, is explainable by differences between the official and Kaplan-Moorsteen indexes in weights and in coverage; nevertheless, a more detailed analysis of specific differences indicates that substantial "upward bias" exists, i.e., quantitatively important statistical malpractices are involved [4, pp. 97-102].

Contrary to their behavior in the 1928-1950 period, the official indexes and ours are, in general, not dissimilar in the 1950-1958 period. Only the machinery indexes exhibit a divergence which is reminiscent of the 1928-1950 behavior. Even so, appreciable divergences between the indexes persist in the 1950-1958 period; the official indexes tend to exceed ours; and the ratio of the official index to ours tends to increase over time.

Though in the 1950-1958 period the official indexes abandoned the use of 1926/27 price weights, the official indexes still differ from ours in several respects: (a) The official indexes for all industrial products and machinery include munitions. (b) The official indexes are price-weighted aggregates of gross output; in the 1950-1955 period, Jan. 1, 1952 prices are used, and in the 1955-1958 period, July 1, 1955 prices. (c) The official indexes are comprehensive in coverage, whereas ours refer only to a limited set of commodities. (d) The official indexes still appear to suffer from methodological shortcomings; for example, from official methodological statements it is quite possible to infer—though not with certainty—that the treatment of new products continues to impart an upward bias to the official index in the post-1950 period [12, pp. 4 ff.].

On the point of weight differences, two comments are necessary: First, for the post-1950 period, the differences in weight years are probably of limited significance, as the intervals between weight years are short and the changes in industrial price structure known to have occurred are limited. Secondly, the difference between the official set of price weights and our heterogeneous set of price, retail sales, and wage-bill weights also appears to be of limited significance at least for the 1950-1955 period. We have taken official indexes for branches of industry—ferrous metals, meats, cotton fabrics, etc.—and aggregated them by means of *our* weighting system into indexes for foods, consumers' goods, all industrial products, etc., and compared the results with official indexes for the more aggregative categories [4, Table 26]. Both our retail sales weights and our wage-bill weights yield results which are very close to the relevant official indexes. Hence it appears

that we can compare the official indexes and ours for the post-1950 period without particular concern about the disturbing effects of differences in weights.

On the point of differences in coverage: First, our inadequate coverage of the proliferation of new machinery models in the post-1950 period and other evidence lead us to the belief that our machinery index significantly understates the actual increase in output. Secondly, it is not clear whether the commodities omitted by us increase, on balance, more or less rapidly than those included [4, Tables 4 and 25, pp. 107-10].

Finally, with respect to official statistical malpractices, we have compared the official indexes for branches of industry—e.g., ferrous metals, textiles, etc.—with corresponding aggregations of our physical output series [4, Table 29] [6, Ch. 7]. In a number of cases such comparisons could be made for branches which appear to be well covered by our physical output series and which are believed to include no munitions. In some of these cases, the output increases shown by the official branch indexes and by our aggregations correspond very closely or exactly. In many cases, however, the official branch indexes show appreciably larger increases which, in the absence of other information, suggests the existence of an upward bias in the official index.

Thus, there are a number of reasons to expect the official indexes and ours to diverge in the 1950-1958 period.⁷ The official indexes for all industrial products and machinery should (a) differ from ours because of the inclusion of munitions in the former and (b) exceed ours because of our inadequate coverage of new machinery models.⁸ All the official

⁷ Also, the categories for which the comparisons in Table 3 are made do *not* have the same meaning in the official and in the Kaplan-Moorsteen indexes. In addition to the difference in coverage of munitions, the following differences are notable: (a) the official index for machinery includes consumers' durables and certain kinds of repairs, whereas our indexes exclude repairs entirely and include consumers' durables in nonfoods; (b) the official index for consumers' goods is the group B index and, hence, excludes a number of commodities which, with further fabrication, eventuate as consumers' goods—notably, textile fabrics—and which are included in our consumers' goods index; and (c) the official index for nonfoods is the light industry index which excludes consumers' durables. However, consumers' durables and repairs are relatively unimportant. And in our index textile fabrics stand for sewn goods output which is otherwise absent. Hence, on the assumption that textile fabrics are a reliable surrogate in this respect, the only significant difference among those enumerated is the difference in the treatment of munitions.

⁸ These divergences may be explored in terms of employment and productivity increases in the 1950-1958 period. The increase in man-years of employment in machine-building and metal-working was about 36.7 per cent during 1950-1956 [4, Table 45] and about 8.7 per cent during 1956-1958 [15, p. 133]; i.e., the increase for 1950-1958 was roughly 48.6 per cent. Over the same interval, 1950-1958, the increase in output per man-year for workers in all industrial branches *other* than machinery and metal-working amounted to about 48.1 per cent (see Table 7 below). Thus, if the increase in productivity in machinery and metal-working was the same as in the other industrial branches, the 1958 output index (1950 = 100) would be 220 for machinery and metal-working and 210.9 for all industrial

indexes should (a) differ from ours because of differences in coverage, and (b) exceed ours because of the apparent existence of methodological shortcomings in the official indexes. Without additional information, however, it is impossible to assess the relative importance of the various reasons for divergence.

B. Major Trends in the Annual Rates of Growth

Table 4 presents the average annual rates of increase of industrial output, as implied by our indexes, over various periods from 1927/28 to 1958. The major trends in growth rates are as follows:

1. In the prewar years, the rates of increase for all industrial products decline slightly from 1927/28-1932 to 1932-1937, reflecting very substantial declines in the rate of increase for machinery and other producers' goods and a very substantial increase in the rate of increase of consumers' goods. In the 1937-1940 period, marked by intensive rearmament, the rate of increase for all industrial products is very modest, less than one-fifth the previous rates of increase; machinery output actually decreases over this period and the rates of increase for other producers' goods and consumers' goods are substantially below the 1927/28-1937 rates.

2. The war years, of course, saw a very large decline in civilian output—a decline of about 50 per cent from 1940 to 1945 in all industrial products, 60 per cent in machinery and consumers' goods, and 40 per cent in other producers' goods (see Table 1). Following these declines, the early postwar period shows unprecedentedly high rates of increase in all major categories of output. By 1948 the prewar levels of output were reattained for all industrial products, machinery and other producers' goods, though consumers' goods did not reach the 1940 level until 1950 and foods not until 1951 (Table 1).

3. In the 1950-1958 period, the rates of increase for all major categories of output decline sharply as against the immediately postwar rates. In comparison with the 1927/28-1940 rates, the 1950-1958 rates

products, including munitions. (The latter index is obtained by combining the machinery and munitions index just described with the Table 1 indexes for other producers' goods and consumers' goods, weighted by the wage-bill distributions *inclusive* of munitions in [4, Table 7].)

In other words, the assumption of equal productivity increases yields an index for all industrial output somewhat greater than ours (which stands at 202 in 1958) but substantially less than the official (which stands at 247 in 1958). On the other hand, the official machinery and metal-working output index, taken with the increase in employment indicated just above, implies a 1950-1958 productivity increase for the branch of over 10 per cent per year, double that for the rest of industrial output. While it seems possible for productivity in machinery and metal-working to have increased somewhat more rapidly than in the rest of industry, the rate of increase implied by the official index is so large as to suggest that differences with respect to munitions and new machinery models are not the sole explanation of divergences between the official machinery index and ours.

TABLE 4—AVERAGE ANNUAL RATES OF INCREASE OF INDUSTRIAL OUTPUT ^a
(Percentages)

	1927/28- 1932	1932- 1937	1937- 1940	1945- 1950	1950- 1953	1953- 1955	1955- 1958	1927/28- 1940	1940- 1950	1950- 1958	1927/28- 1958
All Industrial Products	10.6	10.1	1.9	22.3	9.2	10.1	8.6	8.2	3.5	9.2	6.9
1. Machinery	28.2	16.0	Decrease	49.0	3.7	10.6	10.7	14.1	11.3	8.0	11.6
2. Other producers' goods	16.5	10.1	2.5	18.4	9.5	10.5	8.9	10.3	3.4	9.5	7.8
a. Ferrous metals	11.0	22.0	1.0	18.2	12.0	9.3	6.5	12.7	3.9	9.2	8.8
b. Fuels	15.6	9.1	5.8	12.9	9.2	13.4	13.1	10.5	3.2	11.7	8.3
c. Electric power	26.3	21.8	10.1	16.1	13.8	12.5	11.1	20.3	6.6	12.5	13.5
d. Chemicals	24.9	23.7	2.4	31.5	9.2	12.7	9.0	18.7	5.9	10.0	12.0
e. Lumber, wood products and paper	17.5	5.1	2.6	18.7	6.4	7.4	5.2	8.6	2.4	6.2	5.9
f. Building materials	17.7	14.0	Decrease	30.9	14.5	12.6	11.0	10.4	4.7	12.7	9.1
3. Consumers' goods	2.1	8.5	3.3	20.0	12.6	9.2	6.5	5.0	0.2	9.4	4.5
a. Foods	1.2	8.2	1.5	15.1	10.9	7.3	6.2	4.1	Decrease	8.2	3.5
b. Nonfoods	3.0	8.6	4.8	23.8	13.7	10.3	6.6	5.7	0.9	10.2	5.2

^a These are average annual rates of increase compounded annually over the indicated period. Calculated from the data underlying the indexes in Table 1, they may not quite coincide with the average annual rates of increase implied by these indexes which have been rounded to one decimal place.

are smaller for machinery and other producers' goods and larger for consumers' goods and all industrial products. In comparison with the 1927/28-1937 rates, the 1950-1958 rates are smaller for all industrial products, machinery, and other producers' goods and larger for consumers' goods; however, correction of the indicated understatement of the post-1950 growth of machinery output might be sufficient to remove or reverse the observed inequality in the 1927/28-1937 and 1950-1958 rates of increase for all industrial products.

4. Over the 1927/28-1958 period as a whole, by far the largest annual rate of increase among the major categories of output is that for machinery and by far the smallest is that for consumers' goods. This serves to place the 1950-1955 behavior of consumers' goods output in proper perspective: the increase in output for consumers' goods exceeded that for machinery and other producers' goods in the 1950-1955 period but *not* in the 1927/28-1940, 1940-1950, or 1955-1958 period.

5. Of some current interest is the question whether annual rates of increase have tended to decline since 1950. There is no evidence of retardation within the 1950-1955 period: the average annual rates of increase for all industrial products, machinery and other producers' goods are larger in 1953-1955 than in 1950-1953. There is, however, evidence of retardation in 1955-1958: (1) the 1955-1958 average annual rate of increase is less than the 1953-1955 rate for each of the commodity groups presented in Table 4 except machinery for which the rates are approximately the same; and (2) for all industrial products, producers' goods other than machinery, and consumers' goods, the 1955-1958 rates of increase are also less than the 1950-1953 rates. There is, however, considerable fluctuation in the annual increases for all industrial products and for the separate commodity groups. Calculated from the indexes in Table 1, the annual increases in all industrial products are (in percentages):

1953	1954	1955	1956	1957	1958
9.6	10.2	10.1	8.6	9.7	7.4

Thus, the observed decline in rates of increase within the 1953-1958 period is neither monotonic nor, except for 1958, substantial.⁹

⁹ The official index for all industrial products (which includes munitions) exhibits the following annual rates of increase (in percentages):

1953	1954	1955	1956	1957	1958
11.8	13.3	12.4	10.6	10.0	9.7

[15, pp. 135, 137]. Thus, the official indexes also show a retardation in growth within the 1953-1958 period; but the marked decline in the 1958 annual increase fails to appear in the official data. One important factor which contributes to the 1958 retardation as measured by us is a very large decline in the 1953 machinery growth rate. Given the lack of information on the behavior of munitions output during this period and the limitations of our calculation for civilian machinery, one should not make much of this single year's variation in our index for all industrial products.

C. Indications of Future Rates of Growth

What does the seven-year plan, covering the 1959-1965 period, suggest with respect to future rates of growth in comparison with recent past rates of growth? Unfortunately, the physical output goals stated in the plan cover substantially fewer commodities than those underlying the indexes in Table 1. Hence, in order to discuss future versus past rates of growth, it is necessary to construct new indexes of output for 1950, 1955, 1958, and 1965(P),¹⁰ which cover those commodities for which there are published 1965(P) goals and output series for the preceding years. The average annual rates of increase for 1950-1955, 1955-1958, and 1958-1965(P), which are implied by such indexes, are presented in Table 5. The underlying indexes require the following com-

TABLE 5—AVERAGE ANNUAL RATES OF INCREASE IMPLIED BY 1965(P) GOALS IN COMPARISON WITH RECENT RATES OF INCREASE^a
(Percentages)

	1950-1955	1955-1958	1958-1965(P)	Percentage of 1950 Value of Commodities Included ^b
1. Machinery	7.0	8.2	9.2-10.7	34.3
2. Other producers' goods ^c	10.7	9.7	8.2- 8.8	85.3
3. Foods	8.8	12.1	8.6- 8.8	34.5
4. Nonfoods	11.9	6.3	6.4- 6.6	87.6

^a The calculations underlying these results are summarized in [4, Table 37].

^b The 1950 value of output or sales of the commodities included here divided by the 1950 value of output or sales of the commodities included in the corresponding indexes of Table 1.

^c For other producers' goods, the aggregates are obtained as the sum of price times quantity data without the use of wage-bill weights for commodity groups within other producers' goods.

ments: (a) Their commodity coverage—particularly, in the cases of machinery and foods—is relatively small in comparison with the indexes in Table 1 (see last column of Table 5). (b) In the case of other producers' goods, commodity coverage is inadequate for lumber, wood products and paper, chemicals and building materials, though relatively high for ferrous metals, fuels and electric power [4, Table 36]. (c) Because of these coverage problems, the index for other producers' goods is a price-weighted aggregation of output series without any use of wage-bill weights. (d) Also because of these coverage problems, discussion is confined to foods, nonfoods, machinery, and other producers' goods separately. (e) Because the seven-year plan states a range of 1965(P) goals for many commodities, the indexes computed therefrom for 1965(P) and the implied 1958-1965(P) rates of increase also have a range.

¹⁰ I.e., planned for 1965.

The average annual rates of increase in Table 5, taken as representative for the corresponding commodity groups, suggest the existence of a further decline in rates of growth in the seven-year plan as against the 1950-1958 period. Only for machinery is the 1958-1965(P) rate of increase substantially above the preceding rates. For other producers' goods and foods, the 1958-1965(P) rates are substantially below the 1955-1958 rates, and for nonfoods the rates are about equal. For other producers' goods and nonfoods, the 1958-1965(P) rates are substantially below the 1950-1955 rates; and for foods the rates are about equal.

The conclusion that future rates of growth will be less than those in the immediate past is subject, however, to at least two qualifications: First, the coverage of foods, machinery, and some commodity groups within other producers' goods is so inadequate as to cast doubt on the representativeness of the rates of growth calculated in Table 5; indeed, it is precisely in foods and machinery that the 1950-1958 rates of growth in Table 5 diverge markedly from those in Table 4 implied by our indexes with more comprehensive coverage. In support of retardation, however, are the official indexes which show 1958-1965(P) rates of growth less than both 1950-1955 and 1955-1958 rates of growth for all industrial output and for each of the major components thereof.¹¹

Secondly, the conclusion assumes no overfulfillment of the output goals of the seven-year plan. There are, however, several indications of expectations to overfulfill. The Soviet press has reported "spontaneous" promises by individual plants and regions, frequently followed by Central Committee decrees of encouragement, to fulfill the 1965 goals in 1964 and, sometimes, in 1963.¹² Furthermore, some Soviet economists in analyzing the seven-year plan find substantial "reserves" towards overfulfillment.¹³ Also, an October, 1959 decree called for increased

¹¹ The official annual rates of increase in percentages are:

	1950-1955	1955-1958	1958-1965(P)
All Industrial Output	13.1	10.2	8.8
1. Group A Industry	13.7	11.2	9.2-9.4
a. Machine-building and Metal-working	16.8	13.7	10.4
2. Group B Industry	12.0	8.2	7.1-7.4

The 1950-1955 rates are calculated from indexes in [16, pp. 10, 32, 37]. The 1955-1958 rates are calculated from indexes in [15, p. 146]. The 1958-1965(P) rates are calculated from indexes in [11, Feb. 8, 1959]. As before, all are average annual rates of increase, compounded annually over the indicated periods.

¹² E.g., see [11, June 11, June 18, June 23, 1959].

¹³ For examples, see [14] and [5]. Also, in the last few years there has been a tendency for planned annual rates of increase of industrial output to be exceeded according to the official indexes [11, Feb. 6, 1957, Jan. 27, 1958, Dec. 20, 1957, Jan. 16, 1959, Dec. 23, 1958, Jan. 22, 1960]. Such claims are not always made [11, Dec. 27, 1955, and Jan. 31, 1957].

output of consumers' durables and items of household use over the 1959-1961 period and, thereby, fulfillment of the seven-year plan goals for these commodities before 1965.¹⁴ Of course, these indications are too tenuous for predictive purposes. Nevertheless, if the 1965(P) goals are fulfilled a year earlier, the retardation inferred from Table 5 disappears. The 1958-1964 average annual rates of increase become: 10.1-10.3 per cent for foods, 7.5-7.7 for nonfoods, 10.9-12.6 for machinery, and 10.2-10.8 for other producers' goods. In the case of the official indexes, a similar conjecture yields 1958-1964 average annual rates of increase which are (a) on the average, equal to the 1955-1958 rates, but (b) less than the 1950-1955 rates.¹⁵

D. *An Index of Final Products*

In addition to the indexes in Table 1, we have computed an index of the output of final industrial products—i.e., products which are components of final demand.¹⁶ We have done so because of possible interest in three aspects of the results: (a) they may be of use as interpolating or extrapolating indexes for national product components; (b) they may be of interest in relating changes in the level of industrial activity to changes in industrial deliveries of final products; and (c) the consumers' goods component, when related to population changes, serves as a measure of the changing contribution of industry to living standards.

The index (Table 6) includes the following commodity groups: machinery, building materials and consumers' goods. Of course building materials are not final products, strictly speaking, but are inputs to con-

¹⁴ See [11, Oct. 16, 1959]. The decree calls for a 13 per cent average annual increase in 1958-1960 and a further 12 per cent increase in 1961 in the production of consumers' durables and housewares. Though our indexes do not cover all such commodities, we do have data on many. If we construct an output index for those items for which we have output series, the implied average annual rates of increase are 23 per cent in 1950-1953, 30 per cent in 1953-1955, and 6 per cent in 1955-1958. See [4, Table 3 and p. 130, fn. 1]. The index for consumers' durables and housewares increases much more rapidly than that for all nonfoods in the 1950-1955 period and somewhat less rapidly than that for all nonfoods in the 1955-1958 period. Thus, it appears that the decree in question is an attempt to improve a sector which in the 1955-1958 period failed by far to maintain the 1950-1955 annual rates of increase and that the original seven-year plan goals for this sector implied something much closer to the 1955-1958 rates of increase as against the 1950-1955 rates.

¹⁵ Thus, the official rates of increase for 1958-1964 become (in percentages):

All Industrial Output	10.3
1. Group A Industry	10.8-11.1
a. Machine-building and Metal-working	12.2
2. Group B Industry	8.4-8.7

For the corresponding 1950-1958 rates of increase, see footnote 11 above.

¹⁶ For the details of this calculation and for further discussion of the index, see [4, Section 4.5].

TABLE 6—INDEX OF OUTPUT OF FINAL PRODUCTS OF INDUSTRY^a
(1950=100)

	1927/ 28	1932	1937	1940	1945	1950	1955	1958	Weight for Commodity Group ^b (Percentage)
All Final Products	37.3	45.9	74.0	75.6	31.5	100.0	158.6	200.8	100
1. Capital goods	9.3	23.3	47.4	40.2	16.2	100.0	147.5	200.6	42.0
a. Machinery	6.8	19.5	40.9	34.3	13.5	100.0	136.2	185.0	33.3
b. Building materials	18.8	37.6	72.3	62.9	26.0	100.0	190.4	260.5	8.7
2. Consumers' goods	57.6	62.2	93.2	101.2	42.6	100.0	166.7	201.0	58.0

^a See [4, Table 40 and pp. 135-41] for the details of the calculation and for postwar annual values of the index of final products.

^b Since an index of final products should have gross-value rather than value-added weights, we have reweighted the subindexes as follows: (1) The consumers' goods component is obtained by weighting the indexes of foods and nonfoods from Table 1 by the 1950 retail sales of each. (2) The machinery and building materials components are the indexes in Table 1—i.e., each with Jan. 1, 1950 wholesale price weights. To obtain the capital goods index the machinery and building materials indexes are weighted by the 1950 value of output for the two groups as computed in the present study—i.e., without imputations. (3) The resulting indexes for capital goods and consumers' goods are combined into an index of all final products by weights derived, with difficulties and uncertainties, from official gross value of output data.

struction activities whose outputs are final products. In the absence of direct measures of construction output at constant prices, others have used materials inputs as an indirect measure and it is in this sense that we include building materials as final products of industry.¹⁷

The index omits a number of important construction materials—notably, sawn wood and metals used in construction. If we substitute for our building materials index Raymond P. Powell's more comprehensive index of construction materials inputs and increase the weight assigned to construction materials accordingly [9], the index of all final products is essentially unchanged, while the capital goods index for the 1927/28-1950 period is changed substantially and shows a much smaller increase in output [4, pp. 142-43].

If we compare the index of final products with a similarly weighted index of all industrial products¹⁸—so that the only difference between

¹⁷ For a materials inputs index of the volume of construction, see [10]. For a discussion of this type of index and of alternatives, see [3] and [8].

¹⁸ I.e., an index of producers' goods other than machinery is obtained with Jan. 1, 1950 price weights throughout, and this index is combined with the consumers' goods and machinery indexes in Table 6 using the weights shown there. This gross-value weighted index differs only slightly from the value-added weighted index of all industrial products in Table 1 [4, pp. 144-45].

the two indexes is the inclusion of "intermediate products" in the latter—the two indexes move quite closely together in the 1950-1955 and 1955-1958 periods, but the final products index shows a much smaller increase over the 1927/28-1950 period than the index of all industrial products. Though these results suggest a more rapid increase for intermediate products than for final products in the 1927/28-1950 period and approximately proportionate increases in the 1950-1958 period, they should not necessarily be interpreted as indicative of changing structural relationships within Soviet industry. The results omit from consideration one additional commodity group—agricultural products consumed in industry, primarily in the production of consumers' goods—which (a) undoubtedly increased much less rapidly than intermediate products in the 1927/28-1950 period, and (b) increased very substantially in the 1950-1958 period.

Dividing the consumers' goods index in Table 6 by a population index [4, Table 41], we obtain the following index of per capita consumers' goods output:

1927/28	1932	1937	1940	1945	1950	1955	1958
69.9	70.8	102.9	93.7	45.4	100.0	153.6	177.4

This index shows essentially no change in the per capita output of consumers' goods between 1927/28 and 1932, a substantial increase between 1932 and 1937, an appreciable decline between 1937 and 1940, recovery beyond the 1940 level by 1950 but not yet to the 1937 level, and substantial increases between 1950 and 1955 and between 1955 and 1958. At best, however, this index measures not living standards but the contribution of industry to living standards. Omitted are a number of important elements of Soviet consumption: services, consumption in kind, direct sales by agriculture to households. If we confine our attention to the commodity component of living standards and examine the available data on consumption in kind and direct sales to households, both omissions appear to result in the same direction of error in the index of per capita consumers' goods output. On both counts, the "true" change in the commodity component of living standards between 1927/28 and later peacetime years appears to be either a smaller increase than our index indicates or a decrease [4, pp. 148-50].

E. Changes in Productivity

Table 7 presents estimates of changes in employment and in output per man-year for consumers' goods and producers' goods other than machinery.¹⁹ The estimates require the following comments:

¹⁹ Details of these calculations and a fuller statement of the underlying rationale and argument are found in [4, Sections 4.61 and 4.62].

TABLE 7—INDEXES OF EMPLOYMENT AND OUTPUT PER MAN-YEAR FOR PRODUCERS' GOODS OTHER THAN MACHINERY AND CONSUMERS' GOODS
(1950=100)

	1927/ 28	1932	1937	1940	1955	1956	1958
Indexes of Employment							
1. Producers' goods other than machinery	31.7	56.8	59.6	69.0	124.4	127.3	—
2. Consumers' goods	62.6	66.2	90.2	97.5	129.3	131.9	—
3. Consumers' goods and producers' goods other than machinery	43.6	60.5	71.6	80.2	126.3	129.1	137.6
Indexes of Output per Man-Year							
4. Producers' goods other than machinery	68.7	74.1	112.5	103.8	127.5	134.1	—
5. Consumers' goods	86.7	89.6	98.8	100.8	131.7	139.6	—
6. Consumers' goods and producers' goods other than machinery	79.3	80.8	105.8	102.4	129.2	136.3	148.1

Source: The employment and productivity indexes are from [4, Tables 45 and 46, and p. 168].

1. The productivity indexes are the conventional kind: indexes of output divided by indexes of employment. For this measure, however, the output indexes used are not quite the same as those in Table 1. We have substituted employment for wage-bill weights wherever the latter are used in Table 1. This enables us to interpret the reweighted output indexes as indicators of labor requirements for the given year's output with base-year (1950) labor productivity. The ratio of this magnitude to the labor actually required—i.e., to the index of actual employment—is a measure of the change in labor productivity between the base and given years.

2. Our estimates refer only to consumers' goods and producers' goods other than machinery. They are limited in this manner because: (a) the employment data available for the machine-building industry include both civilian machinery and munitions employment, whereas the output data refer only to civilian machinery; and (b) productivity calculations for all industry are quite sensitive to this distinction. Although we are unable to distinguish with precision that portion of total employment in machine-building which is devoted to civilian output, we have attempted illustrative calculations based on alternative assumptions about the distribution of employment. These suggest: (a) that over the 1927/28-1937 period, the increase in output per man-year for *all* civilian output (i.e., including civilian machinery, other producers'

goods and consumers' goods) was about the same as that for other producers' goods and consumers' goods; and (b) that over the 1927/28-1950 period, the increase in output per man-year for all civilian output was considerably greater than that for other producers' goods and consumers' goods. We are unable to make similar calculations for the post-1950 period.

3. The employment series refers to man-years worked by direct production workers. Since, at least since 1932, *total* employment in industry—i.e., man-years worked by direct production workers, maintenance personnel and white-collar workers and technicians—appears to have increased less rapidly than employment of production workers [16, p. 23], our measures appear to understate the increase in productivity for the more comprehensive group of employed persons. Also, the number of hours worked per year has tended to decline over the 1927/28-1932 period, to increase over the 1932-1950 period, and to remain about constant thereafter; thus, for the pre-1950 period, output per man-hour changes somewhat differently from output per man-year.

4. The employment indexes are also subject to errors, the most important of which is the following: Employment in small-scale industry accounts for about 30 per cent of our estimated total in 1927/28 and about 18 per cent in 1932. The underlying data on small-scale industry employment are sufficiently ambiguous to yield the possibility of appreciable error in our employment and productivity estimates for those years.

With the foregoing qualifications in mind, let us turn to the productivity trends which emerge from Table 7: (1) During the prewar period, there were modest increases in productivity from 1927/28 to 1932, substantial increases from 1932 to 1937, and small changes in both directions from 1937 to 1940. For the prewar period as a whole, output per man-year increased much more rapidly for producers' goods than for consumers' goods. (2) The productivity indexes move uniformly upward in the post-1950 period, the increases for consumers' goods exceeding slightly those for producers' goods. In 1950, however, output per man-year was still below the peak prewar levels, and this in spite of a substantial increase in hours worked per year. (3) For the 1927/28-1958 period as a whole, output per man-year has increased by about 87 per cent, an increase compounded from a 33 per cent increase between 1927/28 and 1937 (the peak prewar year) and a 40 per cent increase between 1937 and 1958. Judging from the 1927/28-1956 data, the increase for producers' goods is substantially greater than that for consumers' goods.

The average annual growth rates in employment and output per man-

year for producers' goods other than machinery and consumers' goods are as follows (in percentages):

	1927/28- 1932	1932- 1937	1937- 1940	1927/28- 1940	1950- 1955	1955- 1958	1950- 1958
Output per Man- Year	0.4	5.5	decrease	1.9	5.3	4.7	5.0
Employment	8.0	3.4	3.9	5.0	4.8	2.9	4.1

In the post-1950 period, the rate of productivity increase declines between 1950-1955 and 1955-1958, but only slightly, and is substantially higher than that for the prewar period as a whole. In the post-1950 period, the rate of employment increase declines appreciably and is below that for the prewar period as a whole. Thus, our calculations show: (1) no general trend toward retardation in the growth of productivity within the prewar period or as between the prewar and postwar periods, and (2) a slight, possibly insignificant, retardation within the postwar period. They suggest as well that the productivity growth rates may have been supported by a tendency toward decline over time in the rate of increase of employment.

The seven-year plan implies a further decline in the future rate of productivity increase. It suggests, without clearly indicating, that the number of industrial workers is to increase by about 3.2 per cent per year in the 1958-1965 period, as compared with the 3.3 per cent rate at which employment in all branches of industry, including machine-building, grew during 1955-1958.²⁰ Since the seven-year plan output goals imply a significant decline in 1958-1965 rates of increase relative to the 1955-1958 rates, they also imply a rate of increase of labor productivity significantly below that realized during 1955-1958. Should the growth of productivity continue at about the 1955-1958 rate, the plan would be fulfilled ahead of schedule and the indicated retardation in the growth of industrial output would not occur. Perhaps this is one of the "reserves" for overfulfillment mentioned above.

It would be desirable to accompany the foregoing discussion of trends in labor productivity with a parallel discussion of trends in capital productivity and to obtain measures of changes in capital-labor ratios. Unfortunately, the only capital measures now available are the official series about which little is presently known. Since we are unable to vouch for the reliability of the capital series, about all we can do is to make explicit the relationships implied by the official capital series when compared with our employment and output indexes. Such com-

²⁰ For the seven-year plan data, see [11, Feb. 8, 1959], and also [4, pp. 175-76, fn. 1]. For 1955-1958, see [4, Table 45, and p. 168].

parisons for 1927/28, 1932, 1937, 1940, 1950, 1955, and 1957 yield the following highly tentative and provisional results:

1. The ratio of capital to labor has increased monotonically and substantially over the observed years from 1927/28 through 1957.²¹

2. The ratio of capital to output has increased (or capital productivity has decreased) monotonically and substantially over the observed years from 1927/28 through 1950, but the change is ambiguous from 1950 through 1957.²²

²¹ See [4, Table 49] for these comparisons through 1955. For the 1957 capital index, see [15, p. 59]. For the 1957 employment index see [4, Table 45]. Both the capital and employment indexes include factors engaged in munitions output; and hence, on this account, the observed changes in the capital-labor ratio are relatively reliable.

²² See [4, Tables 22 and 49] and [15, p. 59]. Even if the official capital indexes are accepted as reliable, this comparison encounters a number of difficulties—especially, that the capital indexes include capital engaged in munitions output whereas the output indexes exclude munitions; and that the capital indexes appear to resemble chained rather than fixed-base index numbers. Thus to make the comparison more precise, one should include munitions in the output index and reweight the prewar output indexes with early-year weights. However, on examination of the resultant output increase which is necessary to yield a decrease in the capital-output ratio, it is still clear that the official capital series implies a substantial increase in the capital-output ratio from 1927/8 through 1950. In the post-1950 period, on the other hand, it is quite possible that inclusion of munitions output might have arrested or reversed the apparent increase in the capital-output ratio. For the underlying argument, see [4, Section 4.63].

A calculation of the change in the productivity of labor and capital combined over the 1927/8-1955 period, with hypothetical weights of $\frac{3}{4}$ for labor and $\frac{1}{4}$ for capital (and similarly for weights of $\frac{4}{5}$ to $\frac{1}{5}$ and $\frac{3}{5}$ to $\frac{2}{5}$), shows an increase in combined factor productivity from 1927/8 to 1940, from 1950 to 1955, and from 1927/8 to 1955. If, however, the weight of capital is increased to $\frac{1}{2}$, then only the 1932-1937 and 1950-1955 periods emerge as periods with output increases clearly in excess of the increase in labor and capital combined. See [4, Table 50, and pp. 183-88].

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THE EQUALIZATION OF RETURNS AND REGIONAL ECONOMIC GROWTH

By GEORGE H. BORTS*

The possibility of interaction between resource earnings and regional growth patterns has been pointed out by many writers.¹ From one point of view, regional growth may be regarded as the outcome of resource movements generated by earnings differentials. In turn, these are caused by initial differences in regional endowments of capital and labor. The resulting pattern of regional growth represents an equilibrating process which will tend to eliminate geographic differences in the returns to resources.

From another point of view, the differentials in resource movements and regional growth patterns are not explainable either in terms of initial differences in the prices of resources or in resource endowments; instead, they are to be explained by a difference in production functions or in the demand for a region's exports.

In this paper I shall examine the empirical implications of these two approaches and try to identify the forces operating through regional growth patterns in the United States in the last four decades. In a sense neither framework offers a complete growth model. Each provides an explanation of the economy's adjustments to changes in tastes, technology, and the stocks of labor and capital. Nor are the two explanations inconsistent. It is conceivable that in some areas, investment is attracted by low real wages and a high marginal product of capital, while in other areas investment is explained by other considerations. It is the purpose of this inquiry to determine which of these relationships has been responsible for the observable growth differentials of the recent past.

The hypotheses to be examined are consistent with the assumptions of a competitive economy, with capital and labor mobile between regions. It is assumed that the total stocks of capital and labor are fixed in the short run, and expansible in the long run through accumula-

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¹ Statements of this problem may be found in articles by Hoover and Ratchford [5], North [10] [11], and Tiebout [11], [15].

tion and reproduction. It is not intended to explain growth in the country at large. National growth patterns will be taken for granted, and only regional differences will be examined.

I. Relations between Growth and Returns to Resources

A. The Equalization of Returns to Resources

A simple model of regional growth is first constructed wherein observable growth patterns are generated by initial disparities in resource endowment.

If we assume that each region produces the same single output with the same production function, those regions with the highest proportion of capital to labor will evidence the highest real wage and the lowest marginal product of capital. In a free market we would observe capital moving from high-wage to low-wage areas, with the consequence that the low-wage areas experience higher rates of growth of capital and of the return to labor. If the regional wage differentials were large enough initially, we might also observe labor migrating from the low-wage to the high-wage areas. This would yield the same equilibrating effect on the return to capital and labor, although the effects on the growth of output in each region would be somewhat different. Whether or not labor migrates, capital movements would produce an eventual elimination of regional differences in resource endowment, in the real wage, and in the marginal product of capital. The adjustments which this model describes are movements along the production function of each region.

The assumptions underlying the model are: (1) The total supply of labor to all regions taken together is fixed. The only way in which one region may employ more labor is through migration from other regions. (2) A single homogeneous output is produced in each region. (3) There are zero transport costs between regions so that the price of output is regionally uniform. (4) The same production function exists in each region, being homogeneous of degree one in the inputs labor and capital. (5) There are zero costs of converting output into capital goods.

Let $X = f(C, L)$ be the single, regionally uniform production function for output in general. L is the quantity of labor employed, C the physical quantity of capital employed, where C is the accumulated stock of past unconsumed outputs. The assumption of a production function homogeneous of degree one means that the marginal physical product of labor f_L , and the marginal physical product of capital f_C are both functions of the capital-labor ratio:

$$f_L = g(C/L); \quad f_C = h(C/L).$$

Under these assumptions, if C/L is greater in region A than in region B, the real wage is higher in region A; i.e.,

$$g(C/L)_A > g(C/L)_B.$$

Further, the marginal physical product of capital is higher in region B,

$$h(C/L)_B > h(C/L)_A.$$

Under the assumption that output is homogeneous, and that there are zero costs of converting output into capital goods, the marginal efficiency of investment is h .² Therefore, capital will flow from region A to region B, and labor may flow from region B to region A.

This model abstracts from differences in production techniques due to the varying fertility and location of natural resources. Nevertheless, it provides a movement towards equilibrium and suggests two testable hypotheses. They are: (1) Low-wage regions will experience the highest rates of growth of capital, and of the ratio of capital to labor. (2) Low-wage regions will experience the highest rates of increase of wages.

The implications of the model will be contradicted if certain of the assumptions are contradicted. For example, in the long run the growth of the labor supply will differ among regions. If the supply of labor grows faster in low-wage regions and if labor is insensitive to regional wage differentials, different conclusions are suggested. Low-wage regions will still experience the highest rates of growth of capital, but they may not experience the highest rate of increase of wages; for the growth of the labor supply would prevent a rise in the ratio of capital to labor.

This problem also arises if we examine separate sectors of a region's economy. The supply of labor in the nonagricultural sector of a low-wage region may be increased through transfer of some of the region's labor force from agriculture. At the same time this sector may experience a very high rate of capital accumulation. However, the attraction of labor to the sector would prevent a rise in the ratio of capital to labor and in the real wage in the sector.

B. *Regional Growth and the Allocation of Resources*

The conclusions of the first model depend upon the assumption that a single uniform good is produced by each region. If we consider two

² In this simple model, the marginal efficiency of investment and the marginal physical product of capital are identical. This is due to the use of one output which can be consumed or accumulated, and a zero marginal cost of transforming output into capital goods. In the more complex models considered below the two concepts are not identical. The marginal physical product of capital is f_c . Assuming capital to be permanent and to yield a constant annual future income stream, the marginal efficiency of investment is a schedule showing

for different levels of investment an ordinate whose value is $\frac{f_c \cdot P_c}{P_o}$ where $\frac{P_c}{P_o}$ is the ratio

of the price of output to the price of capital goods.

output sectors in a region, different conclusions are possible. As will be shown, a high-wage region may then grow faster than a low-wage region. In addition, an increase in physical capital in one region may not yield a change in factor combinations and in factor payments, but instead a reallocation of the region's output in favor of the commodity produced by the capital-intensive sector.

These possibilities have been recognized by Samuelson in his work on factor price equalization [14]. He specifies sufficient conditions for interregional trade in two or more commodities to result in the equalization of factor prices. The conditions imply that a reshuffling of resources within each region is sufficient to eliminate the inducements for interregional factor movements. While regional growth differences might occur, they would not generate a pattern of differences in returns to resources. Nor would they be generated by such differences.

In the second model, I shall therefore assume empirical restrictions on the data which prevent factor price equalization from occurring through internal reallocation. These restrictions take the form of either: (a) regional differences in production functions, or (b) transport costs which prevent commodities from having the same relative prices in each region.

The model now to be presented is one in which growing regions may have higher real and money wages than declining regions. Let us assume that each region produces a positive quantity of commodities in each of two industrial sectors X and Y; and that the output of sector Y is transportable among regions while the output of sector X is not. Further, sector Y need not be identical among regions.

The money wage level in each region depends upon the marginal physical products of labor and the prices of output in the two sectors of the region; the marginal physical products of labor in turn depend on the ratio of physical capital to labor in the region and the composition of output. The same set of variables determines the marginal efficiency of investment in the region.

A region with low money wages may not grow faster than a region with high money wages because it may have a lower schedule of the marginal efficiency of investment.³ It may have a lower schedule if its low wage level is due to a relatively low price of the commodity produced in its transportable sector, or is due to production functions

³ Define the marginal efficiency of investment schedule for each region as a set of points showing values of $\frac{f_0 \cdot P_x}{P_c}$ on the ordinate, while the abscissa shows the ratio of investment to the existing stock of capital. If the actual rate of return on investment is the same in all regions, then the region with the lowest schedule will have the lowest rate of growth.

which yield relatively low values of the marginal products of capital and of labor.⁴

We can now specify the conditions under which a region with high money wages will grow faster than a region with low money wages: (a) A higher marginal efficiency of investment in the high-wage region than in other regions. This could be due to either of two causes: (i) production functions in the high-wage region which yield higher marginal physical product schedules for both labor and capital;⁵ (ii) a rise in the price of the region's export commodity relative to the export commodity of other regions. (b) For noneconomic reasons, population migrates to this region, and the migrants transfer capital with them; or the migrants demand capital once they have completed the move. (c) Residents of the region save a higher proportion of income than residents of other regions, and this is invested, for noneconomic reasons, in enterprises within the region. Any one of the above causes will lead to a rise in the region's income through the familiar investment income-generating process. The income boom will also lead to a passive current balance and a real transfer of capital from other regions.

This is likely to be true even if the income boom is caused initially by a rise in export prices. The export surplus will be short-lived, because it coincides with a rise in the region's marginal efficiency of investment. That is, the price of export goods rises relative to the price of capital goods which are either produced in the domestic sector or are imported. Unlike the traditional balance-of-payments multiplier, a secondary investment boom generates new income and a new demand for imports. While the initial impact of the exports boom is a favorable balance of payments with other regions, its eventual effect must be destabilizing with respect to the balance of payments if the region is to import capital.⁶

1. *Accumulation and the Increase in Wages.* There are two reasons

⁴ Because each region need not produce the same transportable commodity, any particular region may suffer a decline in money wages if, for example, there is a decline in the demand for its product. In addition, a low-wage region might have a low marginal physical product of capital because the ratio of capital to labor is higher in each of its industries than in the high-wage region. This would imply a high marginal physical product of labor, but not necessarily a high money wage.

⁵ In an analysis which deals with only two resources, labor and capital, it is reasonable to think of differences in production functions among regions, even though the "state of the arts" be widely disseminated.

⁶ It is conceivable that one region may grow faster than other regions in a free-trade economy without importing capital. However, it is difficult to see why this should occur unless the residents of the growing region have an irrational demand for externally generated securities. The forces generating growth in the region should also generate a demand for the region's securities.

why a growing region will enjoy a rise in the money wage per worker: (a) The investment income-generating process leads to higher prices of goods in the two sectors X and Y and therefore to a higher marginal value product of labor. This will also represent a rise in the real wage, if the prices of goods imported into the region are now lower relative to the region's money wage. (b) The accumulation of capital may raise the marginal physical product of labor. To accomplish this, accumulation must raise the ratio of capital to labor in both output sectors. This requires that accumulation be accompanied by an increase in the price of output in the sector which is labor-intensive relative to the price in the sector which is capital-intensive.

With the aid of simplifying assumptions, it is possible to specify the conditions under which this will occur. Let us assume that: (1) production functions for the two outputs are homogeneous of degree one; (2) with given resources, there is a unique ratio of capital to labor for each production function which will yield a given marginal rate of substitution between the two products.

These conditions guarantee that factor proportions in the two sectors are unique functions of the relative prices of their outputs. A rise in the proportion of capital to labor in both sectors requires that there be an increase in the relative price of output in the labor-intensive sector. If during accumulation the relative prices of outputs remained unchanged, the factor proportions would not change. All that would happen would be a simultaneous reallocation of output in favor of the sector which is capital-intensive with no change in marginal physical products.

We must therefore restrict the demand conditions for the outputs of the two sectors. If accumulation is to raise the marginal physical product of labor, the demands for the two commodities must be such that the relative price of the labor-intensive commodity rises as the new capital is allocated to the two sectors. A graphic demonstration of these propositions is to be found in the Appendix.

2. *Accumulation and the Real Wage.* With the aid of a number of realistic assumptions, we may specify the effect of growth on the real wage. Let us assume that the products of the transportable sector (Y) of the region are capital-intensive while those of the nontransportable, domestic sector (X) are labor-intensive. For example, the capital-intensive sector may be thought of as manufacturing and mining, while the labor-intensive sector is services.⁷ Further assume that the region is experiencing rapid growth because of upward shifts in the

⁷ If we ignore transportation and communications, it appears reasonable to regard as relatively labor-intensive such activities as: wholesale and retail trade; finance, insurance, and real estate; services; and construction.

demand for the output of its export sector. The major cause of an increase in the real wage is the rise of money wages relative to the prices of commodities imported into the region. It is less certain that the real wage will be improved through an increase in the marginal physical product of labor. As we have seen, it requires an increase in the ratio of capital to labor in each industry. It is brought about by a rise in the price of the product of sector X relative to that in the Y sector. This might be due to the high income elasticity of demand for the product of the labor-intensive sector and to the respending of new income generated in the region. However, if this rise went far enough, it would offset any initial increase in the export price and would wipe out the higher marginal efficiency of investment generated by the export boom.⁸ For if the growth process is to continue, there can be no rise in the marginal physical product of labor, as this would imply a fall in the marginal physical product of capital. If the marginal physical product of labor does rise, it will impede the growth process by leading to a lower marginal efficiency of investment.

The only exception to this would occur if capital goods were exclusively imported. For then we could have: a rise in the prices of both sectors of the region relative to the price of capital goods; a rise in the price of the labor-intensive domestic sector relative to the capital-intensive export sector; a rise in the marginal efficiency of investment, a rise in the marginal physical product of labor, and a fall in the marginal physical product of capital.⁹

This analysis permits us to understand how the real wage and the marginal efficiency of investment may simultaneously be higher than the national average in a growing region and simultaneously lower than

⁸ In a two-community framework, the marginal efficiency of investment can be written as

$\frac{P_y}{P_z} \cdot MP_z^y$, where P_y is the price of output, P_z the price of capital and MP_z^y the marginal physical product of capital in the Y industry. Let us assume that X, the labor-intensive domestic sector, produces capital goods. If resources are efficiently allocated between the two industries, the following condition holds:

$$\frac{P_y}{P_z} \cdot MP_z^y = MP_z^x.$$

By assumption, MP_z^x is a function of the capital-labor ratio in X. Therefore, a reduction in P_y/P_z (an increase in the relative price of the labor-intensive sector) raises the capital-labor ratio in X and Y, lowers the value of MP_z^x and MP_z^y and lowers the marginal efficiency of investment in the region. For a graphic demonstration, see the Appendix.

⁹ Let Z denote the imported capital goods. Then the marginal efficiency of investment (MEI) is:

$$\frac{P_y}{P_z} \cdot MP_z^y = \frac{P_z}{P_z} \cdot MP_z^z$$

The values of these terms could rise due to an increase in the price ratios P_y/P_z , P_z/P_z . At the same time, P_y/P_z could be falling with the consequence that MP_z^y and MP_z^z would fall, although not so much that the MEI would decline.

the national average in a declining region. It has been shown that the real wage in the region depends upon the marginal physical products of labor and the prices of the region's outputs relative to the prices of its imports. The marginal efficiency of investment depends on the marginal physical product of capital in the region and the prices of the region's exports relative to the prices of domestically produced and imported capital goods. Even though the marginal physical product of labor is inversely related to the marginal physical product of capital through factor proportions, favorable movements of the relevant price ratios may raise both the real wage and the marginal efficiency of investment. The crucial assumptions underlying the analysis are either regional differences in production functions, or regional differences in the ratio of the prices of export goods to the prices of goods produced in the domestic sector.

From this analysis, it becomes clear that a region's growth depends strongly upon the behavior of the prices of its export sector. A high-wage region may grow more rapidly than a low-wage region if the demand for its export goods is growing. Because a high-wage region can produce a different export commodity than a low-wage region, the export boom of one region will not be shared by the other. A high-wage region may, therefore, have a greater growth of capital and a greater increase in money wages than a low-wage region. It may also have an increase in the marginal physical product of labor and in the real wage under the conditions specified above.

This model, therefore, allows conclusions which contradict the one-commodity model. For the one-commodity model implies that capital will grow faster in low-wage regions. Here we see that the opposite is possible. Capital may grow faster in a high-wage region. Further, if the elasticity of labor supply does not differ among regions, the money wage rate will increase fastest where capital is growing the fastest. If the money wage rate does not increase rapidly where capital is growing rapidly, then we may look to a high growth of employment as the explanation.

In the following sections these alternative frameworks will be tested against the experience among United States regions in the last four decades.

II. *Statistical Tests*

In order to subject the above hypotheses to test, it is necessary to measure the changes in capital, labor and wages among regions. In all cases, the regions examined are the forty-eight states. The time period runs from 1919 to 1953. This period is broken into shorter intervals marked by business-cycle peaks as the initial and terminal dates. The purpose is to measure growth as the change during each interval. Such.

changes are independent of cyclical movements in the sense that intervening cyclical troughs do not influence the statistical measurement of growth from one peak date to the next. The intervals chosen are 1919 to 1929, 1929 to 1948, and 1948 to 1953.

The growth of capital in each state is measured by the growth of the income to capital.¹⁰ We can think of the total income produced in the state as the sum of payments to wage and salary earners and to property owners. The percentage change in the income to property owners in the nonagricultural sector of each state is used to measure the change in the total return to capital in that sector in each state.¹¹

Percentage changes in nonagricultural employment in each state are computed for the intervals 1919-1929, 1929-1948, and 1948-1953. For 1919 and 1929, the employment estimates are derived from the Census of Population [17] [19]. The employment data for 1948 and 1953 are estimates of the Bureau of Labor Statistics [23].

These sources are also used to derive estimates of employment by states in two nonagricultural sectors: (1) mining and manufacturing, and (2) a composite sector which I have called services. The services sector includes the following employment categories: wholesale and retail trade; finance, insurance and real estate; service and miscellaneous. This dichotomy excludes employees in construction, transportation, communication, public utilities and government. In 1950, these

¹⁰ The total return to capital is the product of three elements: the number of physical units of capital, the marginal physical product of capital, and the price of the output produced by capital. In the two models presented above, capital is treated as a homogeneous physical commodity. In the first model, there is one good which can either be consumed or accumulated. In the second model, the capital good is either produced by the domestic sector of the region or imported. In the first model, increases in the quantity of capital employed would in all likelihood be accompanied by increases in the total return to capital. The exception would occur if the schedule of the marginal physical product of capital were inelastic in the range in which accumulation was taking place. In the second model, increases in the quantity of capital employed will be accompanied by increases in the total return to capital. There are two reasons for this: (a) The money return to capital will be increased by an increase in the prices of goods produced by capital. In a growing region experiencing an investment income-generating process, all prices are likely to be rising. In particular, the prices in the capital-intensive sector will rise due to an export boom. (b) The marginal physical product of capital will not fall rapidly, if it falls at all, because of the expansion of the scale of both the capital-intensive and labor-intensive sectors. Thus, increases in physical capital will be accompanied by slight if any declines in the marginal physical product of capital.

¹¹ For 1929, 1948, 1953, the payments to wage and salary earners and to property owners are derived from [22]; for 1919, from [7]. This measure of capital accumulation was checked for consistency against three other methods of estimation: (a) growth of corporate income after taxes generated by firms resident in each state; (b) growth of total assessed value of property by state; (c) growth of cumulative value of construction by state. For the periods under review, the estimates are close enough to permit reliance on the measure described above. An analysis of methods of estimating returns to capital will be presented by the author to the 1961 Conference on Research in Income and Wealth, in a paper entitled, "Problems on the Distribution of National Income by Regions."

excluded sectors made up approximately 30 per cent of employed non-agricultural workers.

The wage per employee in the two nonagricultural sectors is computed from the income and employment data previously mentioned. The wage change used is the percentage change in the wage income per employee between 1919 and 1929, 1929 and 1948, 1948 and 1953.

A. *The Growth of Wages, Capital, and Employment*

We may use the statistical data to test the hypotheses presented earlier: (a) Capital grows fastest in low-wage areas. (b) Wages grow fastest in low-wage areas unless growth of employment prevents a rise in the ratio of physical capital to labor.

Table 1 indicates for each time interval the average wage level and average percentage change in wages for groups of states classified by their position with regard to the median growth of employment and median growth of capital. This is shown for the nonagricultural sector of each state. For each interval the table shows n , the number of states; w , the average wage income per nonagricultural employee at the initial date; and \dot{w} , the percentage change in w . Also shown are the average values of w and \dot{w} for the entire 48 states. If the data are consistent with the hypotheses, we should observe low values of w and high values of \dot{w} in states with a high rate of capital growth. Also, in each interval the value of \dot{w} in the lower left box should be higher than in the upper left box; further, the value of \dot{w} in the upper right box should be lower than in the lower right box.

TABLE 1—LEVEL AND PERCENTAGE GROWTH OF WAGES IN 48 STATES CLASSIFIED BY GROWTH OF CAPITAL AND OF EMPLOYMENT, 1919-1929, 1929-1948, 1948-1953^a

Time Interval:		1919-1929		1929-1948		1948-1953	
		High Capital Growth	Low Capital Growth	High Capital Growth	Low Capital Growth	High Capital Growth	Low Capital Growth
High Employment Growth	n	13	11	16	8	20	4
	w	\$986	\$970	\$1051	\$1355	\$2799	\$2320
	\dot{w}	16.24%	3.83%	161.73%	128.41%	30.99%	25.51%
Low Employment Growth	n	11	13	8	16	4	20
	w	\$1074	\$1017	\$940	\$1203	\$2912	\$2665
	\dot{w}	16.26%	11.75%	158.81%	127.93%	30.39%	26.80%
U. S. Average	w	\$1011		\$1134		\$2713	
	\dot{w}	12.19%		144.42%		28.75%	

^a n : the number of states; w : the average wage income per nonagricultural employee at the initial date; \dot{w} : the percentage change in w .

As the table indicates we frequently observe the contrary of these events. In two of the three periods, the states with a high rate of capital growth have high values of w . In the 1919-1929 interval, the state groups where capital grew fastest had on the average a wage 2 to 5 per cent greater than in the other states. In the 1948-1953 period, the state groups where capital grew fastest had a wage 10 to 20 per cent greater than the other states. In these same two periods, the fastest wage increase occurred among state groups with relatively high wages. Only in the 1929-1948 period do rate of capital growth and rate of wage increase appear greatest in low-wage states. Finally, the value of \dot{w} does not appear to be influenced by the rate of employment growth. Only in the 1919-1929 interval does high employment growth appear to slow the increase in the wage.

This evidence casts considerable doubt on the hypothesis that low wages are a major influence on capital movements. Or, to put it another way, there is doubt that wage differentials are a prime influence on the marginal efficiency of investment. While it has been true over a long period that wages have tended towards equality among states, the strength of this movement appears markedly weaker in the past four decades.¹²

The strength of these convergence and divergence patterns may be seen in the following contingency table which covers the three periods examined. Table 2 classifies the states according to wage level and rate of wage increase. Shown at the bottom is the value of chi-square (χ^2) which has been computed for each time-interval as a measure of association.¹³ There is a strong divergence pattern among states in the first period, a strong convergence pattern in the second period and no pattern in the third. The association in the last period between high average wages and rate of wage increase alluded to previously results from the classification of states by capital and employment growth. There is nevertheless no pattern of wage divergence in this last period.

Table 1 also supports quite strongly the view that an increase in the total income to capital relative to the amount of labor employed will yield an increase in the wage. Reading across the table, we see

¹² Hanna's data [4] show that the coefficient of variation of wage and salary income per capita declines from 1929 to 1948, and then remains roughly constant to 1951. He does not give any evidence on this coefficient in 1919, presumably because of a different source of data and slightly different definitions. Easterlin's findings [2] indicate that convergence did not occur between 1919-1921 and 1949-1951. Muth's findings [9, pp. 854-77] confirm the failure of state differences in income per worker to narrow in the period between 1920 and 1929, and again after the second world war.

¹³ χ^2 is defined as $\sum_{ij} [f_{ij} - Q_{ij}]^2 / Q_{ij}$ where $Q_{ij} = \sum_i f_{ij} \cdot \sum_j f_{ij} / \sum_{ij} f_{ij}$; f_{ij} is the number of observations in the i th column, j th row.

TABLE 2—CLASSIFICATION OF 48 STATES BY WAGE LEVEL AND RATE OF WAGE INCREASE, 1919-1929, 1929-1948, 1948-1953

Wage Level	Rate of Wage Increase					
Above Average Below Average Value of χ^2	1919-1929		1929-1948		1948-1953	
	Above Average	Below Average	Above Average	Below Average	Above Average	Below Average
	14	9	5	21	11	9
	6	19	20	2	15	13
	6.71		24.52		.01	

that without an exception in each time interval an increase in capital growth relative to employment growth is accompanied by a greater increase in the wage. This conclusion is also supported by evidence on correlation between the change in the total return to capital, the change in employment, and the change in wages. Let us measure the change in the ratio of capital to labor as the difference between the percentage change in capital and the percentage change in labor.¹⁴ Denote this variable as x , and denote the percentage change in wages per employee as y . Then, we have the following correlation coefficients for each time interval:

1919-1929	$r_{xy} = +.508$
1929-1948	$r_{xy} = +.727$
1948-1953	$r_{xy} = +.236$

The first two coefficients are significant in the probability sense at the 5 per cent level, the third is not. However, for the 1948-1953 period, the partial correlation between the change in capital and the change in wages, holding the change in employment constant, is much larger, yielding a significant coefficient of $+.688$.¹⁵ The partial correlation results do indicate that wages respond to increases of capital. However, the observed relation for the 1948-1953 period cannot be stated in terms of the ratio of capital to labor.

B. *The Influence of Agriculture*

It is conceivable but unlikely that the widespread decline in agricultural income in the 1919-1929 and 1948-1953 intervals is responsible

¹⁴ For incremental changes, $\frac{\Delta(C/L)}{C/L} = \frac{\Delta C}{C} - \frac{\Delta L}{L}$.

¹⁵ The discrepancy between the two coefficients occurs because of the nature of the simple correlations between the variates. The correlation of two ratios, X/Z and Y/Z will yield the same results as the partial of X on Y , given Z , when the regressions of X on Z and Y on Z are homogeneous. This condition is not fulfilled in the 1948-1953 period. For a proof of the proposition, see Meyer and Kuh [8, App. C].

for the absence of wage equalization in those periods. An examination of the data indicates that it would be a mistake to attribute the different behavior of wages and capital in the three periods to the varying fortunes of agriculture.

The states which suffered declines in agricultural income were in fact largely low-wage states. It is reasonable to expect that the demand for nonagricultural services would also suffer in these states; hence, low-wage states would experience a low rate of increase of nonagricultural wages as a consequence of the depression in the agricultural sector. To carry the argument further, the low-wage states would experience the strongest wage increase in the 1929-1948 period as a consequence of the sharp expansion of agricultural income at that time.

The explanation requires that the wage convergence of 1929-1948, and the wage divergence of the other two periods, be observable mainly in the agricultural states. It is contradicted by the data. The behavior of wages in the agricultural states frequently departs from this historical pattern. Therefore we can reject the presumption that the wage behavior in all states in the three periods is due to the influence of national agricultural patterns acting on the states most heavily dependent on this industry.¹⁶

C. The Interaction between Increase in Wages and Reallocation

In Section I, it was argued that an increase in the relative price of output in labor-intensive industries would raise the real wage. This proposition is demonstrated graphically in the Appendix. It is there also shown that the elimination of excessive employment in labor-intensive industries might raise the real wage in states with a high proportion of such industries. The possible existence of these two influences has been investigated for the 1929-1948 period. This period was chosen out of the three because it was long enough to permit the adjustments which the analysis presumes. Further, it encompasses the strongest equalization movement of the three periods. The data indicate that it is the one period out of the three in which the sharpest reallocation of employment occurred between the labor-intensive and capital-intensive sectors of each state. Any effect which the elimination of misallocation might have on the real wage should show up in this period.

If these two influences are to be detected, they will show up as a

¹⁶ These conclusions are based upon an examination of the behavior of wage levels and wage growth in agricultural and nonagricultural states. The states in Table 1 were classified by the importance of agriculture. It was observed that the wage behavior observed in the three periods held as well for the states least dependent upon agriculture.

change in wages over and above that already explained by the change in the return to capital relative to the number employed.¹⁷

It is not possible to measure directly the change in relative prices of the labor-intensive and capital-intensive sectors of each state. An approximation is made on the basis of empirical specification of the content of the two sectors.¹⁸ I shall approximate the relative increase of prices in each state sector by the difference between the relative increase in wage earnings per employee in the two sectors. This measure is useful only when wages in the two sectors are initially and finally unequal. If they were always equal, the index would have a value of zero. Because wage equality does not exist initially or finally, the index should provide a useful measure of the extent to which the prices of the products of the labor-intensive sector have risen relative to the prices of the products of the capital-intensive sector.¹⁹

The actual measure employed is the percentage rise of service wages less the percentage rise of manufacturing wages. This measure will be

¹⁷ The wage per worker in one sector of a region's economy may be written as:

$$W = f_L \cdot P_x$$

where f_L is the marginal physical product of labor, and P_x the price of output. Similarly, the money return per unit of capital may be written as:

$$R = f_C \cdot P_x$$

where f_C is the marginal physical product of capital. f_L and f_C are functions of C/L the ratio of physical capital to labor. Ignoring second-order terms,

$$dW = f_L \cdot dP_x + P_x f'_L d\left(\frac{C}{L}\right)$$

$$\frac{dW}{W} = \frac{dP_x}{P_x} + \frac{f'_L}{f_L} d\left(\frac{C}{L}\right)$$

and

$$dR = f_C \cdot dP_x + P_x f'_C d\left(\frac{C}{L}\right)$$

$$\frac{dR}{R} = \frac{dP_x}{P_x} + \frac{f'_C}{f_C} d\left(\frac{C}{L}\right)$$

Substituting, we have

$$\frac{dW}{W} = \frac{dR}{R} + \left(\frac{f'_L}{f_L} - \frac{f'_C}{f_C}\right) d\left(\frac{C}{L}\right)$$

The wage per worker will rise: (a) if there is a rise in the money return per unit of capital, (b) if there is a rise in the proportion of physical capital per worker, (c) if there is a rise in the marginal product of labor and therefore a fall in the marginal product of capital. The effects of (a) and (b) on dW/W have been taken into account by correlating the change in wages with the change in the return to capital relative to the number of workers. The effect of (c) is to be taken into account by determining the extent to which unexplained changes in dW/W are related to changes in the relative price of labor- and capital-intensive sectors.

¹⁸ The two sectors are defined in footnote 7.

¹⁹ The use of this measure requires that technological change either be absent or have little effect on the ratio of prices to wages in each sector.

used to classify the residuals from the regression of wage growth on the growth of the capital-labor ratio. The relation is summarized in Table 3. There is a strong significant association between the residuals from the regression and the relative increase of wages in the two sectors.²⁰ In those states where average wages grew by a greater percentage than predicted by capital accumulation the service wage grew far more rapidly than the manufacturing wage.

TABLE 3—CLASSIFICATION OF RESIDUALS FROM REGRESSION BETWEEN WAGE INCREASE AND INCREASE IN THE CAPITAL-LABOR RATIO, 48 STATES, 1929-1948

Increase in Wages in Services Sector less Increase in Wages in Manufacturing Sector	Wage Increase Greater than Predicted by Regression	Wage Increase less than Predicted by Regression
	States	States
Greater than Average	15	9
Less than Average	5	19

The interpretation of this relation must be clarified. It may exist because of the elimination of wage differentials in the two sectors or because of the alteration of service prices relative to manufacturing prices.²¹ The index of wage changes may be written as follows:

$$\frac{\Delta W_s}{W_s} - \frac{\Delta W_m}{W_m} = \frac{W_s^1 - W_s^0}{W_s^0} - \frac{W_m^1 - W_m^0}{W_m^0}$$

where W_s denotes service wages, W_m denotes manufacturing wages; the superscript 1 denotes the later time period, the superscript 0 denotes the initial time period. If we assume that wages in the two sectors are equalized in the final period, the index becomes:

$$\frac{W_s^1 - W_s^0}{W_s^0} - \frac{W_m^1 - W_m^0}{W_m^0} = \frac{W_s^1 (W_m^0 - W_s^0)}{W_s^0 W_m^0}$$

The index then depends upon the initial discrepancy between wages in the two sectors and the final level of wages. This form of the index was computed to determine its relation to the regression residuals. In

²⁰ A significance test on the contingency table yields a value of $\chi^2 = 8.58$ which is significant at the 1 per cent level. A test on the average value of the residuals from the regression yields a significant value of $F = 7.758$, with 13 per cent of the variance of the growth of wages explained by this classification.

²¹ The elimination of sectoral differentials would yield an increase in the average wage if the sector with growing wages strongly outweighed in importance the other sector.

a sense this provides a test of the hypothesis that the observed association is explainable in terms of the equalization of wages which took place in the two sectors. For it removes from the index any influence of sectoral wage differences in the final period. The index was computed for each state in the form shown above. When the index is compared with the residuals from the regression, the contingency table which results is not significant. That is, classification by the index in the above form does not classify the regression residuals in a significant fashion.²² The index was also computed under the assumption that wage equality results among states as well as within states. That is, the index computed was

$$\frac{W_m^0 - W_s^0}{W_m^0 \cdot W_s^0}.$$

Again, there is no relation between this classification and the residuals from the regression.²³

From this we conclude that elimination of the initial discrepancy in wages between sectors in each state does not by itself explain any of the wage increase in the period. The residual wage increase is, however, explained by the increase of service wages relative to manufacturing wages. This indicates that those states which enjoyed the greatest increase of service prices relative to manufacturing prices appear to have undergone a reallocation of resources yielding an increase in the capital-labor ratio, and an increase in the marginal product of labor.

The results of these tests indicate that the second model plays a powerful role in explaining the increase of wages and capital among

²² The resulting contingency table is the following :

Increase in Service Wages less Increase in Manufacturing Wages	Wage Increase Greater than Predicted	Wage Increase Less than Predicted
Greater than average	10	14
Less than average	10	14

²³ The resulting contingency table is :

Increase in Service Wages less Increase in Manufacturing Wages	Wage Increase Greater than Predicted	Wage Increase Less than Predicted
Greater than average	10	14
Less than average	10	14

states. In the next section, certain aspects of this model are examined in greater detail.

D. *The Convergence of Wages*

We have seen that nonagricultural wage differences failed to narrow in two of the three time intervals examined. They diverged considerably from 1919 to 1929; and only in the 1929-1948 period was there a continuation of the convergence pattern which investigators found prior to 1920. It is useful to inquire why these different patterns occurred, and what light they shed on the possibility for future narrowing of wage differences. Possible answers can be phrased in terms of the general framework presented at the beginning of this paper.

1. The failure of wages to converge is rather surprising in view of the available evidence that these periods witnessed considerable interstate migration from low- to high-wage states.²⁴

While migration was going on in the right direction it is conceivable that varying rates of migration might be responsible for the failure of wages to converge. If interstate migration from low- to high-wage areas slowed down, then the rate of wage convergence might stop. Further, if the rate of migration failed to empty an area as fast as the birth rate was filling it, the pressure of labor supply on wages in a low-wage area might prevent convergence. What evidence exists seems to contradict this explanation.²⁵ However, even if the migration data sup-

²⁴ Migration data have been collected on a uniform basis in [6]. The states have been classified by their average wage and by the degree of in-migration or out-migration (measured in migrants as a per cent of population). The results for the two time-intervals for which migration data are available are shown below. They indicate a strong positive association between migration and wage level. That is, low-wage states experienced greater out-migration, and high-wage states experienced in-migration.

	High-Wage States		Low-Wage States	
	1919-29	1929-48	1919-29	1929-48
States showing net in-migration:				
1920-1930	14	—	2	—
1930-1950	—	15	—	5
States showing net out-migration:				
1920-1930	9	—	23	—
1930-1950	—	11	—	17

In both time intervals, the strong association is due primarily to the high out-migration from low-wage areas. The degree of association is stronger in the earlier period.

²⁵ We have already seen in the previous footnote that the relation between migration and wages is stronger in the 1919-1929 period, when wages diverged. In this period, there is also a strong association between wage increase and population growth. That is, migration

ported this explanation, it would still not deal with the evidence on capital accumulation. Capital was growing more rapidly in high-wage states in two of the three periods examined. Migration in response to wage differentials cannot explain this movement of capital.

Migration might be used to explain capital formation through the influence of new in-migrants on capital requirements. It might be argued that when a large population influx hits an urban area, there is a demand for capital to provide housing and public and private services. This was suggested earlier as one of the causes of growth of high-wage areas. Both the 1919-1929 and 1948-1953 intervals follow periods in which there was strong migration from low- to high-wage states. Prior to each of these periods there were restrictions on the volume of urban construction due to wartime use of materials and of capital funds. It is possible that the higher growth of capital in high-wage areas in these two periods represents a catching up in the stock of urban capital required by prior increases of population. The same capital formation pattern would not be observed in the 1929-1948 period due to the depression and then the war.

The evidence on migration and on population growth does not give uniform support to this explanation. In Table 4, for 1919-1929 and for 1948-1953, the states are classified by capital growth and employment increase. For each case the average rate of population growth and rate of migration for the period prior to the one examined is shown. If the above explanation be valid, then states with higher rates of capital formation should have had higher rates of in-migration (shown as positive) and higher rates of population growth in the prior period.

The implication of the hypothesis with regard to population growth is contradicted by the data. In 3 out of 4 cases, holding employment increase constant, the states with higher capital growth had smaller rates of population growth. The data on migration agree with the hypothesis in three out of four cases, showing higher in-migration or lower

was strong enough to slow the rate of population growth of low-wage regions. This is seen in the following table which classifies the 48 states by the wage level in 1919 and the rate of population growth between 1919 and 1929.

	Above Average Wages	Below Average Wages
Above average population growth 1919-1929	14	4
Below average population growth	9	21

Thus in the period 1919-1929, migration was sufficient to produce an emptying of low-wage areas and yet insufficient to produce wage equalization.

TABLE 4—AVERAGE RATE OF POPULATION GROWTH AND AVERAGE RATE OF IMMIGRATION (+) IN 48 STATES, CLASSIFIED BY EMPLOYMENT GROWTH AND BY CAPITAL GROWTH, 1919-1929, 1948-1953

	1919-1929		
		High Capital Growth	Low Capital Growth
High Employment Increase	1. Average Rate of Population Growth 1910-1920 2. Average Rate of Migration Per Capita 1910-1920	17.84% +.019	18.64% +.002
Low Employment Increase	1. Average Rate of Population Growth 1910-1920 2. Average Rate of Migration Per Capita 1910-1920	10.60% -.010	15.63% +.013
	1948-1953		
		High Capital Growth	Low Capital Growth
High Employment Increase	1. Average Rate of Population Growth 1930-1950 2. Average Rate of Migration Per Capita 1930-1950	38.86% +.107	12.05% -.205
Low Employment Increase	1. Average Rate of Population Growth 1930-1950 2. Average Rate of Migration Per Capita 1930-1950	14.54% -.067	19.66% -.070

out-migration in states with higher rates of capital formation. However in one case, the agreement is by the very small margin of three migrants per 1000 of state population. In the cases where agreement is strong, the difference is as large as 310 migrants per 1000 of state population. The hypothesis receives its strongest support in the 1948-1953 period. It receives no confirmation in the 1919-1929 period. For these reasons I would not attribute a great deal of importance to the role of migration as a general explanation of differences in capital formation.

2. There is a second possible explanation of the failure of wages to converge in two periods. If the marginal efficiency of investment was influenced primarily by the demand for commodities produced by the export sector of each region, there might be an export boom in high-wage states. This explanation requires that in the 1919-1929 and 1948-1953 periods demand for the products of high-wage industries

increased relative to the demand for the products of low-wage industries. In the 1929-1948 period the reverse would have to occur.

Investigators have discovered a high correlation between the industrial composition of a state and the average wage level.²⁶ A state with a large proportion of nationally high-wage industries tends to have a higher average wage than a state with a large proportion of nationally low-wage industries. Therefore, a relative increase nationally in the demand for the products of high-wage industries would primarily affect the states containing such industries.

The influence of demand conditions can be perceived partly through the changes of prices. If demand for a certain class of goods rises more than for another class, we can expect the prices of the former class to rise by a greater proportion, other things being equal. The factors remaining equal would be technology and the other influences on the elasticity of supply. We would also expect the output of this sector to rise more rapidly than in other sectors. Using this as a background, we can inquire how the high-wage and low-wage industries of the country behaved in these periods.

Is there evidence that from 1929 to 1948 demand for the products of low-wage industries rose more rapidly than demand for products of high-wage industries? The price data do not support such an expectation. The wholesale prices of 34 manufactured products were examined between 1929 and 1948. The wage levels and wage increases in the industries producing these commodities were also examined.²⁷ There is no indication that products of low-wage industries rose more in price than products of high-wage industries. At the same time it appears that the wage increases in high-wage industries were on the average less than that in low-wage industries. This is the convergence pattern among industrial wages alluded to previously.²⁸ This indicates that there was a difference in the elasticity of labor supply facing the two sectors. Labor shifted from low- to high-wage sectors, especially after 1939 with the appearance of war-induced employment opportunities. This would increase the likelihood that a greater increase in demand for low-wage products would yield greater price increases than

²⁶ See Muth [9, pp. 878-939], Hanna [4], and Perloff [12].

²⁷ The rank correlation among industries between the wage level in 1929 and relative price increase from 1929 to 1948 is not significantly different from zero. Price data for 1919, 1929, 1948, and 1953 were taken from [24] [25] [26] [27] [28]. Also, data for 1953 were taken from [29]. Data on wages and wage increases by industry for 1919, 1929, 1948 and 1953 were taken from the relevant Censuses of Manufactures for the years 1919, 1929, and 1947 [18] [20] [21]; data for 1948 and 1953 were taken from [30].

²⁸ The Spearman rank correlation among 34 commodities between the 1929 wage level and the 1929-1948 percentage wage increase is $-.36$, which is significant in the probability sense at the 5 per cent level.

in high-wage products. Nevertheless, the data gave no support to such an expectation. There is no indication that the demand for low-wage products grew more rapidly than that for high-wage products from 1929 to 1948.

What of the other two periods? Here the evidence is mixed. There is clear support for the demand thesis in the 1948-1953 period. In this period there is a strong positive correlation between the initial wage level and the subsequent growth of prices. There is also a strong positive correlation between the initial wage level and the subsequent increase in wages. In this period, the high-wage industries experienced greater wage growth and greater relative price increase than the low-wage industries.²⁹ Thus, the regional growth patterns appear to be dominated by national demand conditions in the 1948-1953 period.

For the 1919-1929 period there is no evidence that demand conditions had a differential effect on wholesale prices or wages. In view of the divergence of wages among states in this period we should expect to find prices and wages in high-wage sectors growing more rapidly than in low-wage sectors. Actually, we find no pattern at all. There is no relation between the level of wages paid in 1919 and the subsequent growth in prices. Further, there is no relation between the initial level of wages and the subsequent increase in wages. Finally, there is no relation between wage increases and price increases. Despite this lack of confirmation from wage and price data, there are two kinds of evidence which support the demand thesis:

(a) It is clear that the physical output of high-wage sectors grew by a greater proportion than the physical output of low-wage sectors. Output indexes and wage data for 59 manufacturing industry groups were examined. A significant positive association was found between the growth of output and the level of wages.³⁰

²⁹ The rank correlation among 34 industries between the wage level and relative price growth is $+0.49$. The rank correlation between the wage level and relative wage growth is $+0.71$. Both are significant at the 5 per cent level.

³⁰ The following table classifies the 59 industry groups by percentage growth of physical output and wage level. The resulting value of χ^2 is 12.37, which is significant at the 1 per cent level.

Growth of Output	Wage Level 1919	
	Above Median	Below Median
Above median	21	8
Below median	8	22

Manufacturing output indexes are taken from S. Fabricant [2a].

(b) Innovations among the high-wage manufacturing industries appear to have been labor-saving relative to innovations among the low-wage manufacturing industries. This would explain why increases in the demand for high-wage products had not yielded corresponding increases in prices or in wages of this sector.³¹

These two pieces of evidence indicate that demand for high-wage products grew more than demand for low-wage products from 1919 to 1929. While this did not generate a differential pattern of wage or price increases among industries, it apparently affected capital accumulation in the states containing such industries. It also affected wages in the other industries of these states.

3. A third possible explanation is that the marginal efficiency of investment was influenced primarily by wage differentials in the 1929-1948 period, but not in the other two periods. A strong case can be made

³¹ The degree to which an industry has experienced a labor-saving innovation is measured by the change in the ratio of wage payments to value added. The logic underlying this measure can be seen by assuming the production function in an industry to be homogeneous of the first degree in the inputs of labor and capital, and that labor is paid the value of its marginal product. Then, the marginal physical product of labor (MP) and the average product of labor (AP) are both decreasing functions of the ratio of labor to capital (L/C).

A decrease in the ratio $\frac{MP}{AP}$ will occur if L/C increases or if there has been an innovation

such that at the new equilibrium $\frac{MP}{AP}$ is lower than before. In view of the fact that this

was a period of rapid capital accumulation, and that wages in these industries did not fall relative to wages in other industries, it seems unlikely that L/C increased. Therefore, it seems

reasonable to attribute declines in $\frac{MP}{AP}$ to the emergence of labor-saving inventions. The

ratio $\frac{MP}{AP}$ is measured by the ratio of the wage bill to value added, which is, essentially,

$\frac{MP \cdot P_o}{AP \cdot P_o}$, where P_o is the price of output. It should be noted that the ratio of wages to value added might fall if there was an increase in the monopoly power of an industry, since the wage would be not $P_o \cdot MP_L$, but $P_o(1 + 1/\eta) MP_L$, where η is the elasticity of demand. However, this would have shown up as a price increase in certain industries; and since it fails to show up, I have excluded it as a possible explanation. The relation between the wage level in an industry in 1919, and the change in the ratio of the wage bill to value added from 1919 to 1929 is shown below:

Ratio of Wage Bill to Value Added	High Wages Industries	Low Wages Industries	$\chi^2 = 9.22$, significant at the 1 per cent level.
Rose	1	10	
Fell	21	13	

Data are from [18] [20].

for the influence of demand in the other two periods, and I have cited the data which support this view.

The one group of products where wage differentials could be expected to have the greatest influence is manufacturing. In this sector one is most likely to find exportable commodities which are neither materials nor market-oriented. In the mining and agricultural sectors, production-function differences are likely to dominate any influence of wage differentials. In the service, transportation, and construction sectors, the growth characteristics of the market are likely to dominate the influence of wage differentials. An examination of the growth of manufacturing employment in the period 1929-48 supports the presumption that low wages were an influence on the marginal efficiency of investment. Table 5 shows the average growth of manufacturing employment in the

TABLE 5—AVERAGE RATE OF INCREASE OF MANUFACTURING EMPLOYMENT IN 48 STATES, CLASSIFIED BY NONAGRICULTURAL WAGE LEVEL, GROWTH OF NONAGRICULTURAL CAPITAL AND INCREASE IN NONAGRICULTURAL EMPLOYMENT, 1929-1948^a

	1929-1948			
	High Growth of Capital		Low Growth of Capital	
	A	B	A	B
High Increase in Nonagricultural Employment	n 5	11	n 8	0
	X 15.98%	48.29%	X 47.66%	—
Low Increase in Nonagricultural Employment	A	B	A	B
	n 2	6	n 10	6
	X 16.07%	34.98%	X 26.14%	38.27%

^a n: number of states. X: average percentage increase of manufacturing employment. A: nonagricultural wages above the national average. B: nonagricultural wages below the national average.

48 states, classified in the same fashion as above, i.e., by the growth of total nonagricultural capital, and by the increase in nonagricultural employment. In addition, the states are classified within each box by the average nonagricultural wage. The states with wages below the national average are shown under B, those with wages above the national average are shown under A. X is the average growth of manufacturing employment of the states in the subclassification.

It can be seen that within the three boxes which permit comparison, the high-wage states showed less increase in manufacturing employment than the low-wage states. This indicates that low wages are an influence on the demand for labor; or more specifically, on the marginal efficiency of investment in manufacturing.

This relation is either much weaker or nonexistent in the other two

periods. Thus, there is confirmation that the influence of wage differentials on the marginal efficiency of investment was much stronger in 1929-48 than in the other periods. Table 6 shows the same classification of states for the 1919-1929 and 1948-1953 periods. In the 1919-1929 period the influence of wage differentials is much weaker than in 1929-1948 in three cases, and is opposite to what is expected in one case. In the 1948-1953 period, comparison is possible in only three boxes. In

TABLE 6—AVERAGE RATE OF INCREASE OF MANUFACTURING EMPLOYMENT IN 48 STATES, CLASSIFIED BY NONAGRICULTURAL WAGE LEVEL, GROWTH OF NON-AGRICULTURAL CAPITAL, AND INCREASE IN NONAGRICULTURAL EMPLOYMENT, 1919-1929 AND 1948-1953^a

	1919-1929			
	High Capital Growth		Low Capital Growth	
High Increase in Nonagricultural Employment	A	B	A	B
	n 7	6	n 5	6
	X 0.80%	4.56%	X 8.76%	16.45%
Low Increase in Nonagricultural Employment	A	B	A	B
	n 7	4	n 4	9
	X -12.25%	-15.88%	X -8.10%	-1.91%
	1948-1953			
	High Capital Growth		Low Capital Growth	
High Increase in Nonagricultural Employment	A	B	A	B
	n 10	10	n 0	4
	X 26.69%	22.41%	X —	13.87%
Low Increase in Nonagricultural Employment	A	B	A	B
	n 3	1	n 10	10
	X 7.66%	14.22%	X 5.33%	6.33%

^a n: number of states. X: average percentage increase of manufacturing employment. A: nonagricultural wages above the national average. B: nonagricultural wages below the national average.

two of these the influence of wage differentials is again weaker than in 1929-1948; and in the third, we observe the opposite of what is expected.

III. Conclusions

1. The role of demand appears to have a strong influence on the relative movements of capital and on the increase in wages in the different states. This appears to be the chief reason why wage convergence failed to appear in two out of three periods; and it indicates strong support

for a model of regional growth based on the demand for a region's exports.

2. The influence of wage differentials on the marginal efficiency of investment appears only in the 1929-1948 period. Here we observe a convergence of wages among states. However, this period also witnessed an influence favorable to wage increases which is not likely to be repeated. This was the increase in the marginal product of labor arising from the relative increase in prices in the labor-intensive sectors of growing states.

3. In all periods examined, interstate migration occurs, as expected, from low- to high-wage areas. Nevertheless, migration does not appear sufficient to produce convergence. It clearly produces less divergence than would occur were migration to halt. The chief influence favorable to the increase in wages appears to be the movement of capital, and this is apparently not explainable by migration patterns.

In view of these conclusions, the prospects for wage convergence appear to depend on three elements: (a) continuation of migration from low- to high-wage areas; (b) elimination of the major driving force behind migration, namely the high population reproduction rate in low-wage areas; (c) the direction of capital formation to low-wage areas. Whether wages will continue to converge in the future depends on the relative strength of these three elements. We have observed two periods in which they interacted to prevent convergence.

APPENDIX

The influence of accumulation on the marginal physical product of labor may be demonstrated graphically with the aid of the familiar box diagram.³² In Figure 1 the quantity of labor employed is measured along the vertical axis, capital along the horizontal. Inputs for sector X (labor intensive) are measured from the southwest origin, inputs for sector Y are measured from the northeast origin. The ray from each origin shows the ratio of labor to capital that would be employed at a given set of factor prices in the production of each commodity. At their intersection point α , the isoquants (not shown) are tangent to one another; and their slope at this point gives the marginal rate of substitution between L and C employed in both sectors. This slope corresponds to the relative prices of the factors. The dashed line through α is the contract curve showing other maximum values of X that could be produced for other given values of Y . It is assumed that α is the only efficient point in the box with this value of the marginal rate of substitution. Under these assumptions every point on the contract curve will be represented by a corresponding point on a convex production-possibility curve in X and Y . The combination of X and Y at α will be produced only for a unique value of P_x/P_y .³³

³² A complete demonstration is to be found in Rybczynski [13].

³³ See Bator [1], Worswick [16], and Green [3].

Therefore, the capital-labor ratios in X and Y will be used only for that value of P_x/P_y . If the value of P_x/P_y changes, then production will shift to some other point on the contract curve,³⁴ and other values of the capital-labor ratios will be employed.

Now let us increase the amount of capital by ΔC . This shifts the horizontal dimension of the box as shown. At α' , where the production of Y has increased and X decreased, the capital-labor ratio is unchanged in each sector, and the marginal rate of substitution between the products X and Y is unchanged. Therefore, the value of P_x/P_y at α' is the same as at α . However, if output had shifted to α^* (on the new contract curve going through α'), the capital-

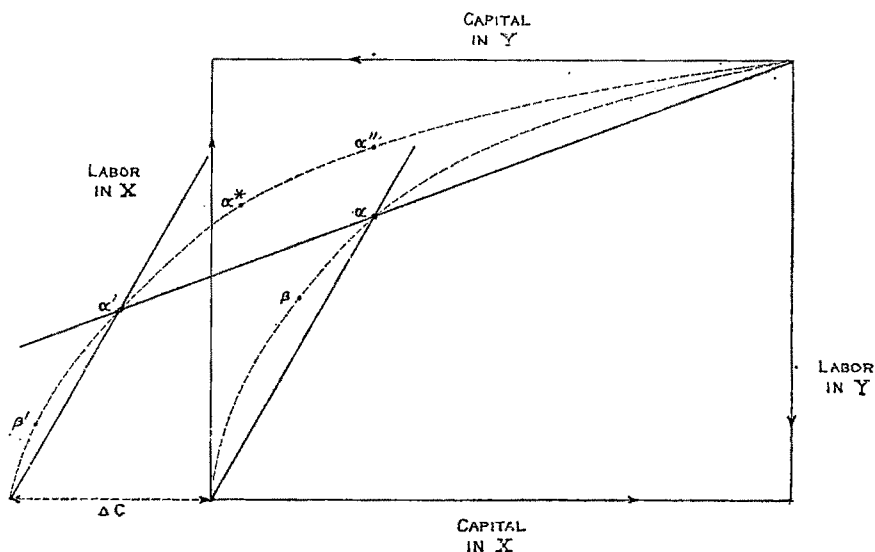


FIGURE 1

labor ratios in both industries would have risen. This would occur when the price of X had risen relative to the price of Y.

Thus, we have reached the following conclusions:

1. Accumulation will lead to an increase in the marginal physical product of labor when it is accompanied by a rise in the ratio of capital to labor in each industry sector.
2. This will occur when the price of output of the labor-intensive sector rises relative to the price of output of the capital-intensive sector.
3. This means that the output of the capital-intensive sector has failed to expand by enough to leave the ratios of input combinations unchanged.
4. If accumulation leads to an increase in the marginal physical product of labor, it will also result in a decline in the marginal physical product of capital.

³⁴ Points on the contract curve are consistent with long-run equilibrium in each industry. Short-run equilibrium positions which are not long-run equilibrium positions represent movements off the contract curve.

Similar conclusions would be reached if in the initial situation labor was misallocated and wage differentials existed between the X and Y industries. The elimination of the misallocation can, but does not necessarily, provide an impetus to the increase in wages over and above any produced by accumulation. For example, assume that efficient point α is consistent with the long-run equilibrium of demand and supply in industries X and Y. Further, assume that the pre-accumulation allocation was inefficient and was located at α'' in Figure 1. This would involve an excessive employment of labor in the labor-intensive sector X.³⁵ At point α'' , the price of X relative to Y (P_x/P_y), the marginal physical product of labor in X, and the wage per worker in X all were lower than they would have been at α , while the marginal physical product of labor and the wage per worker were higher in Y than they would have been at α . If the X sector were large enough, its relatively low wage would overshadow the relatively high wage paid by the Y sector and pull down the total wages paid in the region. In this case the elimination of misallocation and a movement from α'' to α would raise the total wage payment.

That this does not necessarily happen, however, can be seen from the following: Suppose that α'' were to become an efficient point as a result of the accumulation ΔC ; that is, it becomes a point on the new contract curve going through α' and α^* . Further suppose that the price ratio in effect at α'' remained unchanged due to the nature of changes in demand conditions for the two sectors accompanying the accumulation. The result would be a change in allocation between the two sectors such that the marginal physical product of labor in the two sectors is either lower or unchanged from the situation when labor was misallocated.

This result can be seen in the following way. Initial demand conditions were such that α would be the efficient point with a value of the price ratio equal to P_x'/P_y' . At α'' the price ratio in effect was lower than P_x'/P_y' because more X and less Y was produced than at α . Call the price ratio at α'' P_x''/P_y'' . There is an efficient production point corresponding to P_x''/P_y'' , which is to the southwest of α . Call this point β . Corresponding to β is an efficient point β' on the new contract curve with the same price ratio. If when α'' becomes efficient, the price ratio remains at P_x''/P_y'' , allocation will move to β' . At β' , the ratio of capital to labor in Y is lower than at α'' , while the ratio in X is not perceptibly changed from what it was when α'' was inefficient. At β' , the money wage in X is equal to that in Y. This has been accomplished through a fall of the wage in Y.

We see that the movement from α'' to β' is a depressant on the marginal physical product of labor outweighing the elimination of the misallocation. Thus, the elimination of misallocation will not necessarily raise wages in a region when accompanied by accumulation. This depends on the final production point.

Note that if P_x/P_y had risen above P_x''/P_y'' , there could have been an increase in wages. For example, if P_x/P_y had risen sufficiently, allocation could

³⁵ Such a description appears to be a reasonable explanation of the wide discrepancy in earnings between the services sector and the manufacturing sector to be found in many low-income states prior to the second world war.

have remained at α' after it became efficient. All of the additional capital (ΔC) would then have been used in X to raise the marginal physical product of labor in that sector up to a level where the money wage in X again equaled that in Y.

Thus, the conclusions are the same as before: In order for the marginal physical product of labor to rise as a consequence of accumulation, there must be an increase in the relative price of labor-intensive goods.

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MONETARY RESERVES AND CREDIT CONTROL

By JAMES M. HENDERSON*

Federal Reserve policy “. . . operates largely by regulating the volume of member-bank reserves, which constitute the base for bank credit and the money supply” [8, p. 10]. The Federal Reserve authorities have encountered a number of difficulties, but, on the whole, have been able to limit the rate of growth of member-bank reserves, and to a lesser extent member-bank credit. Member-bank credit, however, is only one of many types of credit. The Federal Reserve has little control over credit which is not based upon the issuance of new money. Non-monetary credit has increased at a rapid rate since the accord.

The recent growth of credit extended by financial institutions other than commercial banks appears to be an intensification of a long-term trend. Goldsmith's pioneering study shows a relative decline of commercial-bank credit between 1900 and 1949 [3]. Gurley and Shaw have considered some of the theoretical and policy implications of the rapid growth of “financial intermediaries” in a path-breaking analysis, and have suggested a “financial control” over the assets created by these institutions [4]. The nature of such control, however, is not indicated other than by a suggestion that the assets held by particular types of institutions might be directly regulated. There does not appear to be any unified control system which could serve to regulate such diverse institutions as savings and loan associations and private pension funds, both of which are included within the general category of “financial intermediaries.”

The proposal presented here is far from global. It is limited to an extension of legal reserve requirements and Federal Reserve control to those institutions issuing liabilities that may be classified as savings deposits. The credit extended by such institutions represents a substantial proportion of the volume of institutional credit outside of Federal Reserve control and has been growing at a very rapid rate. In fact, credit based upon savings deposits now exceeds credit based upon demand deposits. Savings and demand deposits are close asset substitutes, and a lack of control or an inadequate control over the institutions issuing savings deposits has raised serious problems regarding both the effectiveness and the equity of Federal Reserve policy.

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The following two sections contain a general discussion of the nature of credit and the means of its control. The effect of monetary reserves is the principal subject of Section I. Section II is devoted to a discussion of the relation between asset choice and the volume of credit. Experience since the Treasury-Federal Reserve accord of March 1951 is examined in the light of this discussion in Section III. The proposal for savings-bank regulation is presented in Section IV, and some of its consequences and problems in Section V.

I. *Monetary Reserves*

Liabilities, Assets, and Credit. Credit extension is a transaction in which a borrower exchanges a financial liability—i.e., a promise to pay a specified number of dollars—for goods or money. Outstanding credit is most broadly defined as the money value of all outstanding financial liabilities, or equivalently, the money value of all financial assets. But since our primary concern is the spending on goods and services generated by credit extension, a more restrictive definition is used. Outstanding credit is defined as the money value of the financial liabilities of final spenders, i.e., households, nonfinancial business enterprises, and governments, which we shall collectively call *the public*. This definition excludes money and the other liabilities of financial institutions, but includes the liabilities of the public which are held as assets by financial institutions.

The public contains creditors as well as debtors. As a result, many credit transactions take place between different members of the public with the liabilities of one member becoming the assets of another. Examples of such transactions are provided by the purchase of a new corporate bond by an individual, the shipment of goods on credit from a wholesaler to a retailer, and the purchase of new government securities by a business firm. Credit also flows to the public from financial institutions in exchange for bank loans, mortgages, securities, and other liabilities. The Gurley-Shaw term *direct debt* is used to denote public liabilities which are held as assets by the public. Total credit is the sum of direct debt and institutional credit.

The financial institutions occupy an intermediate position in the credit process. They issue financial liabilities and use the purchasing power thus obtained to acquire the financial liabilities of others, which they hold as assets. Demand deposits are unique among the private institutional liabilities in that they serve as a widely accepted means of payment; i.e., they are money. Savings deposits are fixed in money value and possess other properties in common with demand deposits, but do not serve as a means of payment and therefore are not money.

- Because of this basic difference between demand and savings deposits,

a commercial bank which issues both is treated as two separate institutions.

Federal Funds Absorption. Monies issued by the federal government are designated by the term *federal funds*. These include deposits at the Federal Reserve Banks as well as currency issued by the Federal Reserve Banks and the Treasury.¹ The publicly held money supply depends upon the quantity and distribution of federal funds. Currency is money, and the quantity of commercial-bank demand deposits is linked to the commercial banks' holdings of Federal Reserve deposits.

The commercial banks must freely exchange demand deposits for currency in order to maintain the former's money quality. The banks play a passive role in this respect. The public and the other financial institutions determine the distribution of their money holdings between currency and demand deposits, and the surplus or deficiency of currency flows to or from the commercial banks. As a result of maintaining demand deposits as money, the commercial banks are the residual holders of the outstanding supply of federal funds.

Member banks hold federal funds in the form of both currency and Federal Reserve Bank deposits. For simplification it is assumed that their legal reserve requirements refer to their total funds holdings without distinction between the two types. It is also assumed that there is a single legal reserve ratio for all member banks of α_m dollars of federal funds for each dollar of demand deposits subject to the requirement.

Commercial banks also hold *domestic balance reserves* in the form of each other's demand deposits in order to facilitate their operations, and to a lesser extent, to provide an additional reserve against withdrawals. Assume that each holds β_m dollars of domestic balances as assets against each dollar of its demand-deposit liabilities which are subject to the reserve requirement. The interbank deposit liabilities of correspondent banks are subject to the reserve requirement. The banks that hold domestic balances as assets, however, are allowed to deduct the amounts of these balances from their total demand-deposit liabilities before computing reserve requirements.

The *federal-funds absorption ratio* for a financial liability is defined as the amount of federal funds which directly and indirectly support a one-dollar public holding of the liability. The ratio for a member-bank demand deposit equals α_m . The bank which issues the deposit to the public holds $(1 - \beta_m)\alpha_m$ dollars of federal funds, and correspondent banks hold the remaining $\beta_m\alpha_m$ dollars.²

¹ Some definitions of federal funds used elsewhere include only Federal Reserve Bank deposits.

² The first correspondent bank issues a new interbank deposit liability for β_m dollars, in-

Savings banks receive currency and demand deposits from the public in exchange for their nonmonetary liabilities, extract customary monetary reserves, and exchange the remaining money for the liabilities of borrowing members of the public. As a result of the redemption passivity of the commercial banks on demand-deposit account, the savings banks are able to apportion their reserves between federal funds and domestic balances in any manner they choose without affecting the outstanding value of *their* liabilities.

Assume that a savings bank holds reserves of α_s dollars of federal funds and β_s dollars of domestic balances in a member bank against each dollar of its savings-deposit liabilities. Its federal-funds absorption ratio is the sum of the federal-funds reserves that it holds and those that are tied up in the member banks to support its domestic-balance reserves:

$$(1) \quad r_s = \alpha_s + \beta_s r_m$$

where $r_m = \alpha_m$ is the member-bank absorption ratio. Equation (1) can be used for the computation of absorption ratios for all types of savings banks and any other financial institution which holds monetary reserves.

Differential Reserve Holdings. Average reserve ratios and the implied federal-funds absorption ratios for the end of 1957 are listed in Table 1. Member and nonmember commercial banks are listed separately. Demand and savings deposit activities are also separated for each group of commercial banks.³ The absorption ratios for nonmember banks were computed from equation (1) on the assumption that their domestic balances were held at member banks.

Since monetary reserves bear zero rates of return, the monetary reserve ratios—the sums of the federal-funds and domestic-balance ratios—provide an indication of the relative sacrifice respectively made

creases its domestic-balance reserve by β_m^2 , and its federal-funds reserve by $(1 - \beta_m)\alpha_m\beta_m$. The correspondent of this bank in turn increases its domestic-balance and federal-funds reserves by β_m^3 and $(1 - \beta_m)\alpha_m\beta_m^2$, respectively. The federal funds supporting the successive domestic-balance reserves form the geometric progression:

$$(1 - \beta_m)\alpha_m\beta_m, \quad (1 - \beta_m)\alpha_m\beta_m^2, \quad (1 - \beta_m)\alpha_m\beta_m^3, \dots$$

which has the limiting sum $\alpha_m\beta_m$.

³ The distribution of reserve holdings between the two commercial-bank activities must necessarily be arbitrary in some respects. The Federal Reserve Bank deposits of member banks were distributed by assigning required reserves to savings deposits, and allocating the remainder, including all excess reserves, to demand deposits. Vault cash and domestic balances of both member and nonmember banks were distributed on the assumption that the amounts held against each dollar of demand deposits were three times as great as the amounts held against each dollar of savings deposits.

in supporting the various deposits.⁴ The monetary reserve ratios for member banks are somewhat higher than those for nonmember banks. The major difference between the monetary reserves of the two types of banks, however, is in form not size. Member-bank reserves are mainly in the form of federal funds; nonmember reserves are mainly in the form of domestic balances. As a result, the nonmember banks have much lower absorption ratios. A nonmember demand deposit absorbs less than half the amount of federal funds absorbed by a member-bank demand deposit. The savings banks other than member banks also hold most of their reserves in the form of domestic balances, and consequently their absorption ratios are low relative to their monetary reserve ratios.

TABLE 1—AVERAGE RESERVE RATIOS: DECEMBER 31, 1957

Type of Liability and Institution	Monetary Reserve Ratios			Absorption Ratio
	Federal Funds	Domestic Balances	Total	
Demand deposits:				
Member banks	.183	.064	.247	.183
Nonmember banks	.035	.199	.234	.071
Savings deposits:				
Member banks	.058	.026	.084	.063
Nonmember banks	.012	.066	.078	.024
Mutual savings banks	.004	.018	.022	.007
Savings and loan associations	.010	.041	.051	.018

Sources: *Federal Reserve Bulletin*; [9] [10].

Monetary reserve ratios for savings deposits are much lower than those for demand deposits. This differential rests on the assumption that savings deposits are less subject to sudden withdrawals and therefore require a smaller safety margin. Although this assumption may be true, it ignores the role of reserves in the process of credit control. Since Federal Reserve authorities regulate credit by regulating the amount of federal funds outstanding, the absorption ratios are the significant figures for credit policy. Since these are not uniform for all assets, the volume of credit which can be supported by a given amount of federal funds depends upon the public's distribution of its asset holdings.

II. Asset Choice and Credit Control

Asset Exchanges. Financial, as well as commodity, transactions generally take place through the media of demand deposits and currency,

⁴ The banks bear an additional earnings sacrifice in so far as they hold low interest-yielding government securities as nonmonetary secondary reserves.

with demand deposits being far the more important. Since demand deposits are the dominant medium of exchange and the commercial banks are the residual holders of federal funds, all public financial transactions can be treated as one or more exchanges between demand deposits and other financial assets. If an individual uses currency to increase his savings deposits, he is assumed to exchange currency for a demand deposit and then exchange the demand deposit for a savings deposit. If he decreases his savings deposits to obtain a direct-debt asset, he is assumed to exchange a savings deposit for a demand deposit and the demand deposit for a direct-debt asset. Once the effects of exchanges between demand deposits and the other financial assets have been determined, the effects of all other asset exchanges can be computed as sums of these.

As a first step, consider the changes in aggregate credit and publicly held demand deposits which would occur if a member of the public exchanged a one-dollar member-bank demand deposit for a savings deposit. It is initially assumed that the aggregate of federal funds remains unchanged, and that the member banks are always "fully loaned-up," i.e., create the maximum amount of demand deposits allowed by their holdings of federal funds. The resultant changes in the balance sheets of the member banks as a whole and the savings bank under investigation are outlined in Table 2. The asset and liability changes are considered in a three-stage sequence for expository convenience. Each entry consists of an amount and a capital letter denoting the sector on whose balance sheet an offsetting entry is made. P, S, M, and F respectively denote the public, the savings bank, the member banks, and the Federal Reserve Banks.

The initial exchange of deposits is described in the first stage. The member banks merely transfer an existing deposit from an individual account to the savings bank's account. The savings bank records its newly acquired demand deposit as an asset and increases its deposit liabilities by an equal amount. The savings bank's disposition of the demand deposit is recorded in Stage 2. It draws α_s dollars worth of federal funds from the member banks and retains β_s dollars as a domestic balance in order to maintain its customary reserves. The remaining $(1 - \alpha_s - \beta_s)$ dollars are exchanged for the liabilities of public borrowers.

The contraction of member-bank deposits and credit which results from their loss of federal funds is described in Stage 3. Since each dollar of federal funds supports $1/\alpha_m$ dollars of member-bank demand deposits held outside of the system, a loss of α_s requires a deposit contraction of α_s/α_m dollars. The federal funds withdrawal by the savings bank reduces demand deposits by α_s dollars. The remaining $(\alpha_s/\alpha_m - \alpha_s)$

TABLE 2—BALANCE SHEET EFFECTS OF EXCHANGING A ONE-DOLLAR DEMAND DEPOSIT FOR A SAVINGS DEPOSIT

Stage	Member Banks		Savings Bank	
	Assets	Liabilities	Assets	Liabilities
1		P - 1 S + 1	M + 1	P + 1
2	F - α_s	S - (1 - β_s) P + (1 - α_s - β_s)	M - (1 - β_s) F + α_s P + (1 - α_s - β_s)	
3	P - $\left(\frac{\alpha_s}{\alpha_m} - \alpha_s\right)$ M - $\beta_m \frac{\alpha_s}{\alpha_m}$	P - $\left(\frac{\alpha_s}{\alpha_m} - \alpha_s\right)$ P - $\beta_m \frac{\alpha_s}{\alpha_m}$		
Totals	- (1 + β_m) $\frac{\alpha_s}{\alpha_m}$	- (1 + β_m) $\frac{\alpha_s}{\alpha_m}$	+ 1	+ 1

Sector identification symbols: P(ublic), S(avings bank), M(ember banks), F(ederal Reserve Banks).

Symbols representing monetary-reserve assets per dollar of own liability: α_m , federal funds held by member banks; β_m , domestic balances held by member banks; α_s , federal funds held by savings bank; β_s , domestic balances held by savings bank.

dollar reduction falls upon publicly held demand deposits, and is achieved by an equal reduction of member-bank credit. The contraction also results in a reduction of member-bank domestic balances by $\beta_m (\alpha_s/\alpha_m)$ dollars.

The net change in outstanding credit resulting from this transfer—called its *credit effect* and denoted by ρ_s —is the algebraic sum of the savings-bank increase and the member-bank reduction:

$$\begin{aligned}\rho_s &= (1 - \alpha_s - \beta_s) - \left(\frac{\alpha_s}{\alpha_m} - \alpha_s\right) \\ &= 1 - \frac{\alpha_s + \beta_s \alpha_m}{\alpha_m};\end{aligned}$$

or substituting from equation (1),

$$(2) \quad \rho_s = 1 - \frac{r_s}{r_m}.$$

The credit effect depends upon the absorption ratios and is positive if the savings bank's ratio is smaller than the member-bank ratio.

The individual who makes the asset exchange sacrifices a one-dollar demand deposit to obtain a one-dollar savings deposit, but the public as a whole does not necessarily make a one-for-one exchange. The net change of publicly held demand deposits can be determined from the information given in Table 2. The net change—called the *demand-deposit effect* and denoted by μ_s —is the algebraic sum of the initial one-dollar sacrifice, the amount passed on to borrowers by the savings bank, and the amount contracted by the member banks:

$$\begin{aligned}\mu_s &= -1 + (1 - \alpha_s - \beta_s) - \left(\frac{\alpha_s}{\alpha_m} - \alpha_s \right) \\ &= -\frac{\alpha_s + \beta_s \alpha_m}{\alpha_m};\end{aligned}$$

or substituting from equation (1),

$$(3) \quad \mu_s = -\frac{r_s}{r_m}.$$

The demand-deposit effect is always negative, but is less than unity in absolute value if $r_s < r_m$. A one-dollar demand deposit passes into the savings bank, (r_s/r_m) of it is lost to the public as a result of reserve adjustments, and the remaining $(1 - r_s/r_m)$ is passed on to borrowers in the form of new credit.⁵

Equations (2) and (3) may be extended to cover exchanges of demand deposits for currency or direct debt. Since currency is federal funds, its absorption ratio is unity, and its credit and demand-deposit effects are $(1 - 1/r_m)$ and $-1/r_m$ respectively. Both are negative, since $0 < r_m < 1$. A withdrawal of currency into public circulation reduces member-bank holdings of federal funds by an equal amount, and results in the familiar multiple contraction of demand deposits and member-bank credit.

Since the issue of new direct debt is an intrapublic transaction which

⁵ The credit and demand-deposit effects cover only the immediate changes resulting from a demand-savings deposit exchange. Additional changes will occur if the recipients of the new savings-bank loans, or persons to whom they make payments, exchange a portion of their newly acquired demand deposits for other assets. The effects of subsequent exchanges can be computed from formulas developed in the text if their extent and timing are known. These effects are probably small relative to the immediate effects, and will most likely result in a further credit increase. Exchanges for new direct-debt assets and additional savings deposits would increase, and exchanges for currency would decrease, credit. If the negative effects of currency outflows are offset by Federal Reserve action, as is postulated later, the final result would be a further credit increase.

requires no reserve adjustments by the banks, direct debt has a zero absorption ratio. Its credit and demand-deposit effects are unity and zero respectively. The effects are independent of the member-bank absorption ratio. An existing demand deposit is passed from one member of the public to another. This transaction increases outstanding credit by one dollar, but leaves the demand-deposit holdings of the public as a whole unchanged. These effects are applicable only for new direct debt. If an outstanding liability is exchanged among members of the public, the distribution, but not the amounts, of outstanding credit and publicly held demand deposits will change. If a member of the public sells an outstanding liability to a bank, credit remains unchanged, but publicly held demand deposits increase by one dollar.

TABLE 3—EFFECTS OF EXCHANGING MEMBER-BANK DEMAND DEPOSITS FOR OTHER LIABILITIES

Asset Acquired	Credit Effect	Demand-Deposit Effect
Nonmember demand deposit	+ .61	+ .61
Savings deposits:		
Member banks	+ .66	— .34
Nonmember banks	+ .87	— .13
Mutual savings banks	+ .96	— .04
Savings and loan associations	+ .90	— .10
Currency	—4.46	—5.46
Direct debt	+1.00	0

Source: Computed from the year-end average absorption ratios for 1957 contained in Table 1.

Numerical values for the credit and demand-deposit effects of exchanging member-bank demand deposits for other liabilities have been computed from the average year-end absorption ratios for 1957 and are presented in Table 3. The effects for exchanges of the other liabilities for demand deposits are equal to, but of opposite sign from, those listed in Table 3. The exchange of a member-bank demand deposit for a nonmember demand deposit would result in an expansion of both credit and demand deposits.⁶ Since the absorption ratios for savings deposits are substantially smaller than the ratio for member-bank demand deposits, the public as a whole can increase its savings deposits with a relatively small sacrifice of demand deposits. Only ten cents of demand deposits must be sacrificed to obtain an additional one-dollar savings and loan deposit.

⁶ The credit effect for nonmember demand deposits is computed from equation (2). Since the liabilities of nonmember banks are demand deposits, their demand-deposit effect is one dollar greater than the amount given by equation (3).

The credit and demand-deposit effects for an asset exchange in which neither asset is a demand deposit are sums of sequential direct effects through the medium of member-bank demand deposits. One asset is exchanged for a demand deposit and the demand deposit is exchanged for the other. Consider the exchange of a member-bank savings deposit for a savings and loan deposit. Using the figures in Table 3, the credit effect is $(-.66 + .9 = .24)$ and the demand-deposit effect is $(+.34 - .1 = .24)$. The effects for such exchanges are always equal in amount, and positive or negative as the absorption ratio for the asset sacrificed is higher or lower than the ratio for the asset acquired.⁷

The credit and demand-deposit effect concepts are easily extended to cases in which the amount of federal funds is variable. The exchange ratios between member-bank demand deposits and other assets may be potential rather than actual. If federal funds are increasing in amount, the public can obtain a new one-dollar savings and loan deposit at approximately the same cost in terms of federal funds as a new .1 dollar-demand deposit. Equivalently, a new dollar of federal funds could support \$55.56, or $(1/r_s)$, of new savings and loan deposits, but only \$5.46, or $(1/r_m)$, of new member-bank demand deposits.

The Limits of Credit Control. Outstanding credit, denoted by L_n , can be expressed as a function of the absorption ratios and public holdings of demand deposits and nonmonetary assets:

$$(4) \quad L_n = \sum_{i=1}^{n-1} (1 - r_i) L_{in} + L_{nn}$$

where L_{in} ($i = 1, \dots, n-1$) is the value of the liabilities of the i th financial institution held as assets of the public and L_{nn} is the public's holdings of direct debt. The public's holdings of currency and other financial assets in turn are related to the volume of federal funds:

$$(5) \quad L_{n+1} = \sum_{i=1}^{n-1} r_i L_{in} + L_{n+1,n}$$

where the subscript $n+1$ denotes federal funds.

The Federal Reserve authorities exercise an indirect control over credit. They regulate the amount of federal funds [equation (5)] which serves as a constraint upon outstanding credit [equation (4)], but with few exceptions do not control credit directly. The minimum reserve ratio for member banks serves to tighten the relation between the federal-funds holdings and credit extensions of member banks. The relation for other institutions is far less rigid. They can and do allow their absorption ratios to fall in tight-credit situations.

⁷ In general, the credit and demand-deposit effects of exchanging asset i for asset j as determined from equations (2) and (3) both equal $(r_i - r_j)/r_m$.

Differential absorption ratios are sufficient to cause major credit-control problems even if the ratios are rigidly maintained. The amount of credit supported by a given amount of federal funds, or the change in credit brought about by a change in the amount of funds depends upon the asset choices of the public. If the public reacts to a tight-credit policy by increasing its proportional holdings of the relatively low funds-absorbing assets, it can obtain more credit from each dollar of funds, and a given variation in the supply of funds will have a correspondingly smaller credit-restriction effect.

The relation of the supply of funds to the volume of credit is loose, but nevertheless, there is always some volume of funds which will allow the Federal Reserve authorities to achieve any level of credit restriction that they desire. A restrictive policy will increase interest rates and reduce the demand for credit, but will affect mainly the availability and supply of credit.⁸ A restriction upon the supply of funds will work upon the public's need for money balances and its desire for a high proportion of liquid assets. As the money supply becomes tighter, the public will become more reluctant to increase its nonmonetary asset holdings at the expense of money balances. The fact that nearly any credit-restriction goal can be achieved is not in doubt, but the amount of funds-restriction necessary to achieve a particular goal is quite uncertain. It might be very large under a system of differential reserves.

The use of credit policy in an aggressive manner is constrained by Federal Reserve responsibilities to the Treasury and the member banks. The responsibility to the Treasury is much less of a constraint today than before the signing of the accord, but it nevertheless still exists. The obligation was changed from that of maintaining government security prices to "maintaining orderly conditions in the government security market" by the accord, and was further weakened to "correcting disorderly conditions" in 1953 [7, pp. 86-90]. Although the term "disorderly conditions" has never been clearly defined, it can be assumed that either a major Treasury refunding failure or a sharp decline in the prices of outstanding government securities would qualify as a "disorderly condition." Therefore, the Federal Reserve cannot be an aggressive seller of governments during periods of Treasury refunding. Furthermore, large-scale open-market sales during a period of general expansion, when commercial banks and other financial institutions are moving out of governments relative to other assets, would most likely lead to a sharp decline in government security prices and thereby create "disorder."

⁸ It is assumed that the interest-rate mechanism works imperfectly, and that a tight credit situation is characterized by an excess demand for credit.

The Federal Reserve must also fulfill a number of responsibilities to its member banks. Roosa has distinguished the *defensive* and *dynamic* responsibilities of the Federal Reserve [6]. On the defensive side it provides currency and a system for check collection, and compensates for outside factors which affect member-bank reserves in order to prevent sharp fluctuations of member-bank credit. Its dynamic responsibility is to use its powers to implement a general credit-control policy.

Though conceptually separable, the defensive and dynamic are not independent. Both responsibilities are met simultaneously on a day-to-day basis, mainly through open-market operations. During the postaccord period the Federal Reserve authorities have generally been able to meet their defensive responsibilities, and at the same time, limit the rate of expansion of member-bank reserves. This is about as far as they can or will go. A sharp contraction of member-bank reserves would represent a violation of responsibilities which have not been violated in the past. Reserve requirements have been increased only at times when member banks held large amounts of excess reserves, and open-market sales have not been used to obtain sharp reductions in bank credit.

III. *Postaccord Developments*

The Hold-Tight Policy. The Federal Reserve pursued a goal of credit ease from May 1953 until January 1955 in order to stimulate spending during the 1953-54 recession. Its policy was one of restraint during the remaining 61 of the 80 months from the signing of the accord in March 1951 until November 1957. Ease was again the goal from November 1957 until the fall of 1958 when the System shifted back to restraint.

Discount rates have been changed many times since the accord in order to follow interest-rate changes and indicate Federal Reserve sentiment, but a regulation of the rate of increase of member-bank reserves has been the major means of policy implementation. Open-market policy has been used to "hold-tight" member-bank reserves during periods of restraint, but has not been used to contract them in the vigorous manner suggested in most textbooks. Reserve-ratio reductions were combined with open-market operations to ease the reserve positions of member banks during 1953, 1954 and 1958.

A numerical description of the hold-tight policy is given in Table 4, which lists the absorptions of federal funds by financial institutions and the public for the seven-year period from the end of 1950 to the end of 1957. The absorption figures for financial institutions other than the demand-deposit departments of member banks include both their holdings of vault cash and the federal funds which support their domestic-balance reserves. The figures for member-bank demand deposits cover

TABLE 4—FEDERAL FUNDS ABSORBED BY FINANCIAL INSTITUTIONS AND THE PUBLIC: DECEMBER 31, 1950-1957
(millions of dollars)

Holder	1950	1951	1952	1953	1954	1955	1956	1957
Demand deposits:								
Member banks	15,971	18,045	17,859	17,604	16,610	16,866	17,139	16,931
Nonmember banks	1,148	1,385	1,358	1,335	1,247	1,250	1,386	1,360
Savings deposits:								
Member banks	1,875	2,325	2,462	2,631	2,420	2,493	2,666	2,934
Nonmember banks	191	223	222	230	224	222	246	270
Mutual savings banks	210	241	235	243	240	237	240	247
Savings and loan associations	322	386	453	512	664	703	708	731
Life insurance and pension funds	900	970	1,020	1,070	1,000	1,050	1,030	1,050
Public	23,455	24,325	25,443	26,504	26,129	26,519	26,598	26,526
Totals	44,072	47,900	49,052	50,129	48,534	49,340	50,013	50,049
Annual changes:								
Member banks (demand deposits)	—	+2,074	— 186	— 255	— 994	+256	+273	—208
Other financial	—	+ 884	+ 220	+ 271	— 226	+160	+321	+316
Public	—	+ 870	+1,118	+1,061	— 375	+390	+ 79	— 72
Total change		+3,828	+1,152	+1,077	—1,595	+806	+673	+ 36

Sources: *Federal Reserve Bulletin*; Federal Deposit Insurance Corporation, *Annual Reports*; Federal Home Loan Bank Board, *Combined Financial Statements*; and "Flow of Funds Tables" in *Federal Reserve Bulletin* (August, 1959).

only those funds which support publicly held deposits excluding Treasury deposits. The figures for the public are merely the amounts of its holdings of currency and coin.

Looking at the annual changes in the lower part of Table 4, the hold-tight policy is first discernible in 1952 when member-bank holdings of funds against demand deposits declined by \$186 million. Ignoring 1954, these holdings show year-end variations of less than \$300 million from 1952 through 1957. The large decline of reserves in 1954 together with moderate changes in the other years since 1951 suggests a policy of absolute restriction. However, reductions of the legal-reserve ratios in 1953 and 1954 respectively released \$1,190 and \$1,583 million from legal-reserve status in the member banks. Effective reserves, therefore, have increased by a moderate amount in the period as a whole.

The relatively small variations in member-bank demand-deposit reserves were achieved with much larger variations in the total supply of funds. Other financial institutions increased their absorptions in every year except 1954. The Federal Reserve also compensated for currency drains to the public of more than a billion dollars in both 1952 and 1953, and a drain of \$390 million in 1955.

In so far as the Federal Reserve compensates for funds withdrawn from the member banks, the demand-deposit cost of currency for the public as a whole is zero rather than the \$5.46 listed in Table 3. The demand-deposit cost of a savings deposit also is reduced since the public now loses only the amount of the demand deposit diverted to the savings bank's domestic-balance reserve. The demand-deposit cost of a one-dollar savings and loan deposit is thus reduced from .1 to .04 dollars.

Table 5 contains average absorption ratios for the eight year-end dates. The ratio for member-bank demand deposits increased as a result of legal-reserve ratio increases in early 1951, declined as the banks reached a very tight reserve position at the end of 1952, increased slightly in 1953 as released reserves were used to increase liquidity, and declined in response to legal-reserve reductions in 1954. The subsequent year-end ratios remained very close to the 1954 level. Changes in the ratio for member-bank savings deposits reflect an increase of the legal-reserve ratio in 1951, and a decrease in 1954.

The absorption ratios for the other savings banks varied in response to variations in the member-bank demand-deposit ratio, but also exhibited independent variations. The ratios for mutual savings banks and savings and loan associations moved steadily downward in response to the tight-credit situation after the end of 1954. The non-member banks allowed their absorption ratios to decline during the 1955 boom, but restored them in 1956.

Public Reactions. The hold-tight policy was successful in limiting the rate of growth of demand deposits, but had limited effect upon the public's holdings of other financial assets.⁹ Table 6 shows that postal savings and government security holdings grew less rapidly than demand deposits from the end of 1950 to the end of 1953 and from the end of 1953 to the end of 1957. Currency holdings also grew less rapidly during the latter period. All other assets increased more rapidly during both periods. The highest growth rates were exhibited by savings and loan and credit union deposits which increased 8.4 and 10 times as fast

TABLE 5—AVERAGE ABSORPTION RATIOS: DECEMBER 31, 1950-1957

Liability	1950	1951	1952	1953	1954	1955	1956	1957
Demand deposits:								
Member banks	.202	.216	.202	.203	.184	.182	.183	.183
Nonmember banks	.078	.087	.080	.077	.070	.068	.072	.071
Savings deposits:								
Member banks	.063	.075	.074	.073	.061	.062	.063	.063
Nonmember banks	.026	.029	.026	.026	.023	.022	.025	.024
Mutual savings banks	.011	.012	.011	.010	.009	.009	.008	.007
Savings and loan associations	.023	.024	.023	.023	.024	.021	.019	.018

Sources: See Table 4.

as demand deposits. Life insurance and pension funds and other direct debt grew rapidly relative to demand deposits, but savings deposits showed the most rapid growth rates among the general asset categories during both periods. At the end of 1957 the public's savings-deposit holdings exceeded their demand-deposit holdings by \$14.6 billion. By the end of 1958 this differential had increased to more than \$22 billion despite an unusually large increase of demand deposits during 1958.

IV. *The Case for Uniform Reserves*

A Proposal. Changes in public asset holdings and changes in outstanding credit are generally different sides of the same coin. Credit expansion can be regulated only if it is possible to regulate asset expansion. Federal Reserve control is limited to member-bank deposits, principally their demand deposits. The demand-deposit control has both a direct and an indirect effect. The limitation of member-bank credit resulting from a limitation of demand deposits represents the direct effect. The indirect effect is realized in so far as individuals be-

⁹ The liabilities of the federal government are included in the public liability (outstanding credit) totals, but government asset holdings are not included in the public asset totals. Government assets are very small relative to government liabilities, and are generally the result of legislation rather than asset choice.

come more reluctant to exchange their limited supplies of demand deposits for other assets. The combination of these effects does not appear overly promising as a basis for future anti-inflation policy. The responsibilities of the Federal Reserve to the Treasury and member banks limit the direct effect, and the experiences outlined in Table 6 cast doubt upon the possibility of a sufficiently compensating indirect effect.

The credit gap resulting from differential reserves can be closed by extending Federal Reserve control to all deposits in order to equalize absorption ratios. Reserve requirements for savings deposits could be

TABLE 6—PUBLIC HOLDINGS OF FINANCIAL ASSETS: DECEMBER 31, 1950-57

Type of Asset	1950	1953	1957	1950-53	1953-57
	(billions of dollars)			(percentage increase)	
Demand deposits:	99.0	108.9	119.8	10.0	10.0
Member banks	84.9	92.3	101.3	8.7	9.8
Nonmember banks	14.1	16.6	18.5	17.7	11.4
Savings deposits:	74.1	95.0	134.4	28.2	41.5
Member banks	29.2	34.9	45.0	19.5	28.9
Nonmember banks	7.2	8.8	11.2	22.2	27.3
Mutual savings banks	20.0	24.4	31.7	22.0	29.9
Savings and loan associations	14.0	22.8	41.9	62.9	83.8
Credit unions	0.8	1.7	3.4	112.5	100.0
Postal savings	2.9	2.4	1.3	-17.2	-45.8
Life insurance and pension funds	80.9	102.8	137.7	27.1	33.9
Currency	23.5	26.5	26.5	12.8	0
Direct debt:	202.4	228.5	284.4	12.9	24.5
Federal government securities	96.5	101.4	105.3	5.1	3.8
Other	105.9	127.1	179.1	20.0	40.9
Totals	479.9	561.7	702.8	17.0	25.1

Sources: See Table 4.

set in the manner currently used for member-bank demand deposits, i.e., a specified ratio between federal-funds holdings and total deposit liabilities less domestic-balance assets. Equal absorption ratios for demand and savings deposits would then be achieved by setting equal reserve requirements.¹⁰ The establishment of uniform reserves would increase the effectiveness of anti-inflationary credit policy without recourse to direct controls and without creating a policy so severe that its

¹⁰ If savings banks are allowed to offset their domestic-balance holdings, equation (1) becomes

$$r_s = \alpha_s(1 - \beta_s) + \beta_s r_m$$

where α_s now is the savings-bank reserve requirement. The absorption ratio for member-bank demand deposits as a whole remains

$$r_m = \alpha_m. \quad \text{Letting } r_s = r_m, \text{ yields } \alpha_s = \alpha_m.$$

application might result in financial disaster.¹¹ The case for this proposal is considered on the grounds of direct effect, uncontrolled asset expansion, equity, and convenience.

Direct Effect. The often repeated statement that savings institutions "play a middleman role in conveying to their ultimate users loan funds brought into being by others" [2, p. 120] is true, but frequently serves to obscure a consideration of inflationary credit expansion. The inflationary effects of spending based upon one type of credit are just as great as those based upon another. Effects do not differ because one type of spending is based upon a more active use of the existing money supply and another is based upon an expansion of the money supply.

Money which is placed in a savings deposit may represent savings from current income, but this does not mean that the subsequent spending of this money is not inflationary. The spending-savings relationship works both ways. The public increases its holdings of savings deposits as the money value of its gross savings increases. The money channeled into the savings banks is passed on to borrowers whose spending in turn increases the money value of income and the money value of the public's gross savings. The credit generated by this sequence is largely outside of Federal Reserve control, and is limited only by spending time lags and the public's reluctance to increase its holdings of savings deposits relative to demand deposits. Conditions during the postaccord period have been such that the public has been willing to accept large relative increases in its savings-deposit holdings.

If the absorption ratios for all deposits were equalized, credit effects would equal zero for all deposit transfers [see equation (2)]. Furthermore, the savings banks could no longer increase credit by allowing their reserve ratios to decline during boom periods. A new dollar of federal funds would support a given amount of credit regardless of its ab-

¹¹ Cf. L. S. Ritter [5]. Ritter has suggested that gaps in credit policy which are reflected by increases in income velocity may be beneficial because: "Changes in velocity . . . provide the needed safety valve, tempering and graduating the impact of monetary policy and thereby enabling the central bank to apply more restraint than it might otherwise risk" [5, p. 127]. Savings deposit expansion, of course, is one of the means by which income velocity has increased in recent years.

It is almost impossible to dispute the general proposition that gaps in the credit control mechanism can serve to compensate for a lack of omniscience on the part of the Federal Reserve authorities. The extent of the gaps necessary for this purpose, however, is not so certain. At the extreme is a credit policy with so many gaps that it has no effect. Current credit policy rests upon a declining base and contains a number of gaps. The central bank must apply vigorous restraint to the money supply and commercial-bank lending in order to have a sizeable effect upon total credit. The declining base and gaps also lead to uncertainty as to the magnitude of the credit effects resulting from a specific action. An adoption of uniform reserves would enlarge the control base, close a very important gap, and thereby allow the central bank to pursue its goals with less vigor and more certainty. A sufficiently large number of safety valves would nevertheless remain.

sorption among the various banks. Furthermore, the public as a whole would have to sacrifice, either actually or potentially, one dollar of demand deposits for each new dollar of savings deposits that it acquired; i.e., the demand-deposit effects of equation (3) would equal unity.

The direct effect of the uniform reserve plan would be based upon a doubling of the volume of credit under the uniform control of the Federal Reserve, and an elimination of credit expansion through deposit transfers and adversely variable reserve ratios. The Federal Reserve authorities could determine the rate of deposit and credit expansion for the commercial and savings banks as a whole by varying the quantity of federal funds. The distribution between demand and savings deposits would be determined by the asset choices of the public.

Uncontrolled Asset Expansion. Creditors generally desire to hold more liquid assets than borrowers desire to issue. The savings banks as well as the commercial banks have profited from the public's desire for liquidity. Savings deposits are fixed in money value, almost always paid on demand, and are, therefore, highly liquid in the eyes of the public. The problems of *time* deposits during the 1930's are becoming a dim memory. Savings deposits do not serve as a means of payment, but have become very close substitutes for the asset functions of demand deposits. Balances classified as precautionary or speculative are probably more frequently held in the form of savings deposits than in the form of demand deposits.

Savings and loan association deposits, which were in relative disfavor after the experiences of the 1930's, have undergone major qualitative changes in the eyes of the public [Cf. 1]. The savings and loan associations have carried out major promotional campaigns in an attempt to convince the public that their liabilities are no different from any other savings deposit, and furthermore, they pay the highest interest rate.¹² The success of their campaigns is witnessed by their growth rate.

Savings deposits have provided highly liquid substitutes for demand deposits for the public's expanding asset portfolios. Life insurance companies are similar to savings banks as grantors of credit, but the assets which they issue to the public are rather remote substitutes for demand deposits. Pension funds, which have been growing much more rapidly than life insurance companies, provide even more remote substitutes. Treasury bills appear to be the major substitute for demand deposits among the direct-debt assets, but by the nature of their market they can be held only by institutions. Other direct-debt assets do not appear to be close substitutes.

¹² In 1958 the savings and loan associations paid an average return of 3.38 per cent on their deposits, contrasted with 2.21 per cent for insured commercial banks and 3.09 per cent for mutual savings banks. See [10, p. 11] and [9, pp. 203 and 221].

The uniform reserve plan would reduce the credit effects of demand-savings deposit exchanges to zero. Positive credit effects would still be realized if individuals exchanged either type of deposit for new direct-debt assets, life insurance reserves, pension fund reserves, or certain other uncontrolled assets. In general, however, the public cannot obtain highly liquid substitutes for demand and savings deposits among the uncontrolled assets. Therefore, total deposit control would not open a new liquidity gap comparable to the gap now provided by savings deposits, and the public is unlikely to respond, to a regulation of the rate of increase of total deposits by accelerating the rate of increase in its holdings of uncontrolled assets. On the contrary, the liquidity squeeze provided by total deposit control may increase the reluctance of many individuals to exchange their limited supplies of deposits for less liquid uncontrolled assets, and thereby spread the base for credit control by indirectly reducing the rates of increase for uncontrolled assets. Life insurance and pension fund assets, which are frequently accumulated automatically, are probably less susceptible to this indirect control than are many of the direct-debt assets.

Equity. The idea that the primary purpose of reserves is the provision of funds to meet unexpected withdrawals has been discarded as a reason for the regulation of member-bank deposits. In the interests of credit control the Federal Reserve requires member banks to hold reserves which are larger than necessary to meet normal withdrawal demands, but stands ready to provide additional funds in cases of unusually heavy withdrawals. As the final source of liquidity for the economy as a whole, the Federal Reserve is required to aid the savings banks in the event of financial crisis. At present the savings banks share little of the responsibility for credit control, but gain most of the benefits provided by the Federal Reserve and other government agencies. The federal savings and loan associations provide a striking example of the inequities in the present system of diverse regulation. The Federal Home Loan Bank Board and the eleven Federal Home Loan Banks provide loans and other Federal Reserve-type services, but do not impose Federal Reserve-type regulations.

Equal reserve requirements for demand and savings deposits are not unknown in this country. The National Banking Act required them, and twelve states currently have uniform requirements [12, pp. 468-69]. The savings-deposit differential was included in the Federal Reserve Act in order to allow member banks to compete for savings deposits with outside institutions [11, p. 957]. This would no longer be a problem if the competing institutions were brought under Federal Reserve control.

Convenience. The uniform reserve plan provides a convenient means

for increasing the effectiveness of credit control because it requires only an extension of the familiar indirect methods already employed by the Federal Reserve. Open-market operations would be used to regulate the federal-funds holdings of the controlled institutions as a whole. All controlled institutions would be able to use the discount window to meet temporary reserve deficiencies.

The regulation of credit based upon nondeposit assets appears possible only if extensive direct controls are instituted. Information gained from past experience with direct controls is not encouraging. They proved difficult to formulate, difficult to enforce, and expensive to administer.

V. *Some Consequences and Problems*

Consequences. A shift to total deposit control would remove the virtual prohibition upon demand deposit growth during periods of credit restraint, and thereby reduce the relative growth advantages of savings deposits. The opportunity cost of holding demand deposits would also be reduced. The savings banks would be required to increase their total monetary reserve ratios, i.e., the proportion of their deposits on which they earn no interest, and therefore would be able to pay somewhat lower rates of return than under the current system.

The savings-deposit gap has become increasingly important as savings deposits have become more and more like demand deposits. The adoption of the uniform reserve plan would not reverse recent growth trends. It would reduce the relative advantages of savings deposits, but savings deposit growth would undoubtedly continue to exceed demand deposit growth if the current narrow differences between the two types of deposits remained unchanged. Savings deposit growth would be further stimulated if the differences continued to narrow.

Many savings banks are trying to devise a means to make their deposits subject to checks drawn outside of their premises. If they are successful, there will be two types of demand deposits, one which pays interest and one which does not. The uniform reserve plan would allow a control of credit effects in this case, but in itself would provide no solution for the impossible position of the commercial banks. However, the existence of a unified Federal Reserve control over all deposits, which would accompany uniform reserves, would provide a means for a regulation of differences between the two types of deposits.

Problems. If uniform reserves were required immediately for all deposits outstanding on the date that the plan was put into effect, the money markets would be thrown into a state of complete disorder as the savings bank sold securities and converted domestic balances in order to increase their funds holdings. There are many possibilities for easing a

transition to uniform reserves. If requirements were set lower than current demand-deposit requirements, the savings banks would need to raise a smaller amount of funds, and the commercial banks would have an excess-reserve margin to cover the conversion of savings-bank deposits to funds. The uniform reserve plan would allow a control of credit effects in this case, but in itself would provide no solution for the impossible position in which the commercial banks would be placed if savings banks paid interest on deposits subject to check. However, placing all deposits under the administrative control of the Federal Reserve would facilitate the regulation of many savings-bank practices.

Problems of equity arise once operating differences between individual banks are recognized, but these are not new problems. Country member banks currently maintain larger domestic-balance reserves than reserve city and central reserve city banks. Equal reserve requirements would force the country banks to maintain much larger total monetary reserve ratios than the other classes of banks, and thereby suffer a much larger relative earnings sacrifice. The lower reserve requirements for country banks are mainly justifiable on equity grounds. Similar equity differentials would have to be established for savings banks. However, these differentials would be small relative to the country-bank differential. Current domestic-balance ratios for savings banks are below the country-bank ratio, and would fall further below if the uniform reserve plan were adopted.

Concluding Remarks. The uniform reserve plan is not presented here with the intention of rousing the readers of this paper into writing their congressmen to urge its adoption. It will not provide a panacea. A number of its consequences and problems require more complete investigation and additional information. Available information, however, suggests promise for the plan, and it is presented as a possibility which deserves further consideration. New information, which is becoming available at an increasing rate, may either strengthen or weaken the case for uniform reserves.

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THE "OUTER-SEVEN" AND EUROPEAN INTEGRATION

*By MORDECHAI E. KREININ**

The beginning of 1959 witnessed the first round of tariff reductions and quota increases within the European Economic Community¹ (henceforth referred to as the EEC). While the first removal of trade barriers under the Rome Treaty has been small, its impact upon business practices has been considerable. In anticipation of future intra-EEC tariff cuts, various steps taken by the business community within "the Six" discriminate against nonmember nations. Distributors inside the Common Market, for example, began dropping the products of British and other outside agencies in favor of products of the Six; industrial agreements for joint production and selling were concluded between firms in the Community cutting across national frontiers, but excluding nonmembers; and finally, American investors have recently begun to prefer EEC locations over sites in the United Kingdom.²

In response to these initial effects of the Community, and in view of the failure of the negotiation for an all-European Free Trade Area [15, Ch. 4] (of which the EEC was to be part), a rival scheme was devised known as the European Free Trade Area Association [14] (henceforth referred to as the EFTAA). Its members, frequently described as the "Outer Seven," are the United Kingdom, Sweden, Norway, Denmark, Austria, Switzerland and Portugal. The new scheme is not seen as a permanent arrangement. Rather it is supposed to provide a vigorous spur towards a broader all-European agreement [3]. In addition it will give the seven countries a certain compensation for the losses they are suffering from the establishment of the Common Market. The pressure on the EEC to negotiate a broader agreement is to be exerted through discrimination against products of the Six in the markets of the Seven.

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¹ The six members of the Community are: France, West Germany, Italy, Netherlands, Belgium and Luxemburg. Greece and Turkey have asked to be linked to the Community as Associates but not with full membership status [1].

² If an all-European Free Trade Area were established, many American investors would prefer British locations, thus gaining free access both to Europe and to the Commonwealth nations.

Although the new scheme is designed to bring Europe closer to an over-all agreement, it may produce precisely the opposite result. Fears have already been expressed in various quarters that establishment of the EFTAA may push the agreement with the Six farther away, thus "deepening and completing the division within Europe" [2, p. 715]. So far, these apprehensions have been mostly intuitive. It is a main purpose of this paper to examine empirically the extent to which they may be justified (Section II). But first we compare the two prospective unions with reference to their impact on the pattern of resource utilization in the world (Section I).

I. Comparative Welfare Implications of the Two European Unions

Both the EEC and the EFTAA provide for the eventual abolition of most trade restrictions within their respective areas. Nevertheless there are important differences between them. The Common Market, a customs union, will eventually have a common tariff against outsiders. On the other hand, each of the Outer Seven may maintain its own tariff rates, thus permitting Great Britain to retain its imperial preference system. While the EEC is viewed as a permanent cohesive union, which may eventually lead to a political union,³ the EFTAA is planned as a looser organization with much more modest and temporary goals. Since the Outer Seven plan to use their union as a stimulus for a wider agreement in Europe, their integration schedule corresponds roughly to that of the EEC. It will be somewhat faster than that of the Common Market, with the first step of a 20 per cent tariff cut to be taken in July 1960. That the EFTAA is considerably smaller, and economically weaker than the EEC is clear from Table 1. Its population is about half, and its GNP is 60 per cent that of the EEC. One nation, the United Kingdom, occupies a predominant position among the Outer Seven. It produces two-thirds of the total EFTAA GNP, and accounts for 57 per cent of the area's total trade.⁴ There is no such single predominant nation among the six Common Market countries.

³ "... the common market is primarily political in purpose, and its economic objectives, while of great importance, are nevertheless secondary. The men who were responsible for the Treaty of Rome . . . were inspired primarily by the desire to progress toward political integration, ultimately toward some kind of European federation, some form of a United States of Europe . . . one can say quite categorically that without the political objective there would have been no Common Market and that the six nations which joined in the Treaty of Rome would never have entered into such a revolutionary adjustment of their trade policies if the treaty had contained no political content; if it had been merely a free trade area or a customs union." From a statement by George W. Ball, attorney for the Common Market, before the Joint Economic Committee, United States Congress [5, p. 992].

⁴ The predominant position of British trade in the EFTAA is further demonstrated by the fact that, for commodities in which the United Kingdom has a comparative advantage, the entire area's exports exceed its imports. This is not the case for commodities in which other EFTAA members have a comparative advantage (see Section II).

Both unions occupy a major position in world trade, although the EFTAA falls short of the EEC (Table 1) in the proportion of world exports and imports for which it accounts. Their formation is likely to have a significant impact on the pattern of world trade. For this reason it is instructive to focus the comparison of the two unions on their relative impact on the world's welfare.⁵ Available data make possible inter-union comparison on the basis of three welfare criteria suggested by Jacob Viner, and a fourth one added by J. E. Meade.

In his pathbreaking work on the theory of customs unions, Viner [17, pp. 41-55] bases the criteria for judging the economic advantage of a customs union on the distinction between the favorable trade-creation

TABLE 1—THE EUROPEAN ECONOMIC COMMUNITY AND THE EUROPEAN FREE TRADE AREA ASSOCIATION—SOME COMPARISONS (1957)

	EEC	EFTAA	U.K.
Population (millions)	165	88	52
Gross National Product (\$ billions)	157	95	62
Total exports (\$ billions)	22.4	16.3	9.3
Total imports (\$ billions)	24.8	20.1	11.4
Proportion of world exports (per cent)	22.7	16.4	
Proportion of world imports (per cent)	23.3	18.8	
Intratrade (\$ billions)	7.0	3.2	
Intratrade as per cent of exports	31.3	19.6	
Intratrade as per cent of imports	28.4	15.9	

Sources: (1) OEEC, *Statistical Bulletins, General Statistics*, No. 4, July 1959; (2) OEEC, *The Network of Intra-European Trade, Trade by Product in 1957*.

effect and the unfavorable trade-diversion effect. Trade creation takes place when, as a result of the removal of trade barriers within a customs union, a member country replaces its own high-cost (formerly protected) production by imports from another member of the union. Trade diversion occurs when, as a result of the removal of trade barriers within a customs union, a member country replaces its imports from a low-cost nonunion source, by a higher-cost source within the union.⁶ The shift is caused by the newly created tariff discrimination favoring union members. The relative magnitude of the two effects would determine the welfare impact of the customs union.

⁵ The word welfare is used loosely here to denote economic efficiency. The discussion ignores the possible impact of regional integration on the distribution of income within and between countries.

⁶ This classification is not exhaustive. There can be trade displacement from a high-cost source outside the union to a lower-cost source inside. This can occur, for example, when production costs within the union decline because of economies of scale brought about by the creation of the union. Such a shift is not adverse to welfare.

On the basis of this distinction Viner offers the following conclusions [17, p. 54]:

A customs union is more likely to operate in the free trade direction. . . .

- (1) the larger the economic area of the customs union and therefore the greater the potential scope for internal division of labor (the limiting case is a customs union embracing the entire world); (2) the lower the "average" tariff level on imports from outside the customs union area as compared to what that level would be in the absence of the customs union;
- (3) the greater the difference in unit costs for protected industries of the same kind as between the different parts of the customs union.

It is clear from Table 1 that, with respect to the first criterion, the EEC is likely to be more favorable (or less unfavorable) to world welfare than the EFTAA. Not only is it larger in population and production, but the proportion of its total trade transacted within the area is significantly larger (by 11 to 12 percentage points) than the parallel proportion in the EFTAA, thus leaving less room for trade diversion.

Viner's second criterion which depends on the effect of integration on tariff rates yields no conclusive results, because average tariff incidences are likely to decline in both unions as a result of integration. Official pronouncements in Washington (in connection with the 1958 extension of the Reciprocal Trade Agreements Act) and in Europe indicate a strong probability that the common EEC rates will be reduced through GATT negotiations conducted with the Community as a whole.⁷ Indeed, the Community's 10 per cent tariff reduction on January 1, 1959 was extended in part also to third countries. In the EFTAA case, even if nominal rates in individual countries remain intact, one can expect some reduction in effective rates. Unless import controls through certificates of origin are strictly enforced, the lowest tariff that prevails for each commodity will tend to become the effective rate for the entire free-trade area. The rules of origin adopted by the EFTAA require that 50 per cent of the value of a commodity be produced in a member country in order for it to acquire duty-free status. That still leaves some leeway for imports to enter high-duty countries through low-duty countries, if they are embodied in commodities which are processed in the EFTAA and form less than 50 per cent of the value of such commodities. (This

⁷ Cf. statement by T. Scitovsky [5, p. 1040]. The basic rule of taking the unweighted average tariff rate of the four constituent customs areas as the common EEC rate may actually result in a small increase of tariffs in the Community. France and Italy will have to lower their tariffs by about the same amount that Germany and Benelux will raise them. Since the first two countries account for 40 per cent of EEC imports and the latter two for 60 per cent, the unweighted average would tend to be somewhat above the average weighted by imports. Assuming however that GATT rules will be followed, this potential increase will be compensated for somewhere in the customs union.

possibility is limited by the small differentials in tariff rates of the major EFTAA nations.) It is, however, impossible to predict the relative magnitude of tariff cuts in the two unions.

Data on production costs which could be used in applying Viner's third criterion are extremely scanty. But such fragmentary evidence as exists suggests that cost differentials are larger within the EEC than within the EFTAA. First, one would expect this to be the case because production is more evenly distributed among the six Common Market nations than among the seven EFTAA nations. Two-thirds of total EFTAA GNP in 1956 was produced by one country (United Kingdom) within which trade is free and costs are presumably equal. Another 20 per cent was produced in the three Scandinavian countries, between which there is free factor mobility (and low tariffs), resulting in a strong tendency toward equalization of production costs. In the EEC, on the other hand, the distribution of 1956 GNP among the four constituent customs areas was as follows: France, 37 per cent; Germany, 33 per cent; Italy, 17 per cent; and Benelux, 13 per cent. Thus, there appears to be more room for cost differentials (weighted by GNP) in the EEC than in the EFTAA.

Furthermore, some inferences can be drawn from comparative tariff rates, if those are taken as an indication of the level of protection necessary to maintain high-cost industries. Table 2 presents average levels of ad valorem import duties for ten West European countries. There appears to be considerably greater dispersion of tariff rates (weighted either by imports or by GNP) among EEC nations than among those of the EFTAA. Disaggregation of the data leaves this result unaltered. On the basis of a study of tariff rates imposed on 570 comparable commodity groups (the five digit SITC) by West European nations,⁸ the United Nations' Economic Commission for Europe [15, Ch. 4, p. 15] lists France and Italy as having the highest tariffs in Western Europe; Benelux and the Scandinavian countries (and now Germany), the lowest; while the United Kingdom is ranked between the two extremes (but closer to the lower end).⁹ Two points become apparent from examination of this study. First, tariff rates in the Outer Seven are—on the average—lower than rates in the six EEC nations. Second, unlike the EFTAA, the EEC includes both very high- and very low-duty countries. Tariff variations within the EEC are larger than within the EFTAA, leading one to expect larger cost differentials in that area.¹⁰

⁸ The calculations on which the study is based were made by GATT.

⁹ This is consistent with specialists' estimates quoted by the Department of Commerce statement: "The Nature and Significance of International Comparisons of Tariff Levels" [4, p. 229].

¹⁰ This argument would be strengthened if Greece and Turkey were to attain associate status with the EEC.

Finally, unlike the EFTAA, the EEC will have free factor mobility in addition to a common commodity market, and is thus likely to attain a higher degree of cost equality.

In a more general way, the fact that EEC tariff rates have been higher than those in the EFTAA implies that the EEC nations have had a better opportunity to develop industries for which they lacked comparative advantage [9, Ch. 3]. Thus the Common Market is more likely to lead to the elimination of such industries. It may therefore be concluded

TABLE 2—AVERAGE AD VALOREM IMPORT DUTIES

EEC Nations				EFTAA Nations			
Country	Tariff Rate	Per Cent of EEC		Country	Tariff Rate	Per Cent of EFTAA ^a	
		Imports (1957)	GNP (1956)			Imports (1957)	GNP (1956)
Italy	25%	40%	54%	United Kingdom	12%	57%	66%
France	24%			Scandinavian Countries (Sweden, Norway, Denmark)	10%	25%	20%
Germany ^b	10%	60%	46%	Switzerland	9-10%	10%	8%
Benelux	9%						

^a Data are missing for Austria and Portugal. While both are relatively high-tariff countries, they account for only 6 per cent of EFTAA GNP and their omission would not affect the results significantly.

^b Germany's tariff rates declined substantially in the past few years. In 1952 her average ad valorem rate was 34 per cent.

Sources: (1) Tariff rates in EFTAA except for Switzerland: P. J. Verdoorn [16, p. 489]. (2) Tariff rates in the EEC: G. W. Ball [5, p. 997]. (3) Imports and GNP data: OEEC, *Statistical Bulletins*, General Statistics, July 1958. (4) The tariff rate for Switzerland, relating to 1949, was obtained privately from Hans Wyss, a Swiss economist at the IBRD.

that the EEC would result in more trade creation than the EFTAA, making it more favorable to welfare.

A fourth criterion, offered by Meade, tends to favor the EEC over the EFTAA. This criterion is designed to cope with the following problems left unresolved by Viner: In most cases of customs union formation, both trade diversion and trade creation are present. How can they be weighed as to their relative importance? Moreover, if demand is not of zero elasticity, the removal of a tariff within the union will not only cause a shift in the pattern of trade, but will also result in a price reduction and, therefore, in an increase in the quantity taken. How is that increase to be evaluated? Likewise, if elasticity of supply is short of infinity, an increase in demand on the part of one country for a product of another

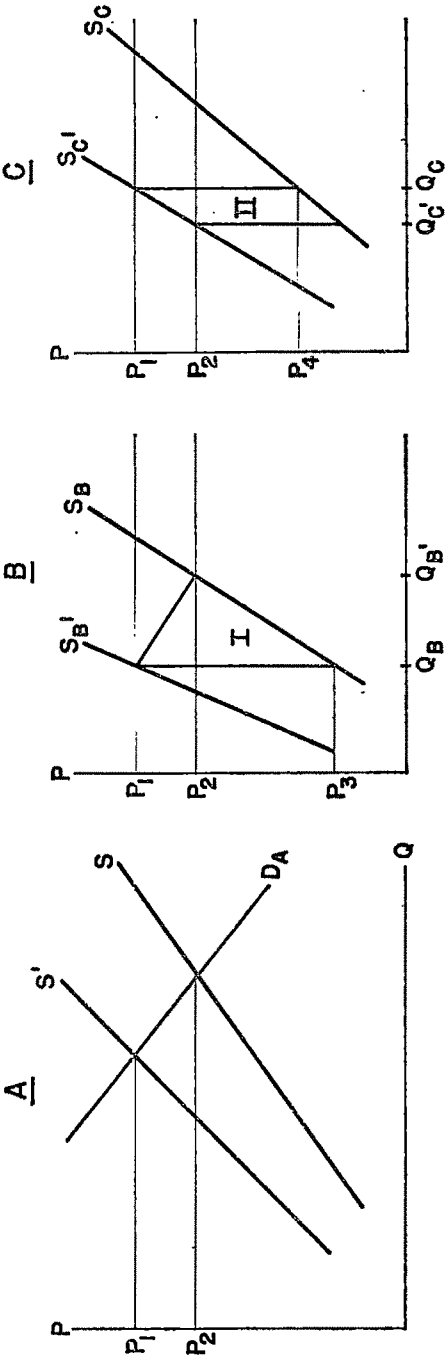


Figure 1

country within a newly formed customs union may change cost of production per unit of output. What would be the economic implications of that change?

Using a quantitative welfare approach, Meade's criterion [9, Ch. 3-5] [10, Ch. 7] is based on the divergence between the marginal cost of producing a product and its marginal value to the consumer in the importing country. The divergence is created by the import duty, and is measurable in terms of the rate of tariff yield. If a seller is willing to part with his product for \$2, while a buyer is willing to pay \$3 for it (the tax being 50 per cent), then—assuming that the marginal utility of money is the same for both—the seller gives up less in terms of utility than the buyer receives. The transaction therefore results in a rise of their combined welfare, and an increase in the number of such transactions will be favorable to welfare.

Using this criterion, trade diversion and creation can be described diagrammatically as follows (Figure 1): Let countries B and C be the suppliers of a certain product to country A, with production costs in C lower than in B. S_B and S_C are the export supply curves of B and C (to country A) respectively, and S'_B , S'_C are the same supply schedules taking into account A's duty. S' is the total export supply of both countries B and C in country A, given the duty. Price P_1 is established and countries B and C will export to A quantities Q_B and Q_C respectively.

Producers in B and C, while receiving price P_1 , are willing to supply the given quantities for prices P_3 and P_4 respectively. The distance P_1P_3 indicates the amount by which welfare would rise if exports from B to A were increased by one unit. Consumers in A would get a unit which is worth P_1 to them, while sellers in B would give up a unit which is worth only P_3 to them. By the same token, the distance P_1P_4 shows the amount by which welfare would decline if exports from C to A were reduced by one unit.

When A and B establish a customs union to the exclusion of C, B's export supply schedule shifts to S_B , while C's supply curve remains S'_C . The total import supply curve in A becomes S . It results in a new, lower price P_2 , in an increase to Q'_B of B's export to A, and in a decline to Q'_C of C's export to A. Triangle I measures the trade-creation effect,¹¹ and area II the import-diversion effect. The more elastic is D_A , the larger will be the trade-creation effect, and the smaller will be the import-diversion effect. Likewise, the more elastic is S_B the larger the trade-creation effect. A similar diagram can be drawn to depict export diversion.

¹¹ The triangle is constructed as follows: a straight vertical line connects S'_B at height P_1 with S_B , while the upper line, parallel to D_A , connects S'_B with S_B at heights P_1 and P_2 respectively.

In shifting towards a new general equilibrium, the following effects of a customs union must be considered: trade creation within the union, trade diversion between member and nonmember countries, and changes in the direction of trade involving only nonmember nations.¹² Any shift in trade from countries between which there exists a small divergence between marginal cost and marginal value (i.e., low import duty) to countries between which the divergence is large, would have a favorable effect on welfare in proportion to the volume of trade in the commodity. The welfare effect would be unfavorable for shifts in the opposite direction. The quantitative impact of each shift can be measured in terms of the areas involved, as in Figure 1. A comparison between the "favora-

TABLE 3—PERCENTAGE DISTRIBUTION OF INTRA-UNION TRADE AMONG COMMODITY GROUPS (1957)

Commodity Group	SITC	Percentage Distribution	
		Outer Seven	Common Market
Food, beverages & tobacco	(0, 1)	15.9%	14.0%
Crude materials	(2)	17.8	10.2
Mineral fuels	(3)	8.4	11.2
Oils & fats	(4)	0.7	0.6
Chemicals	(5)	6.4	6.6
Machinery	(7)	21.5	21.7
Manufactures	(6, 8)	29.3	36.3
		100.0%	100.0%
Total Intra-Union Trade (\$ millions)		3,186.7	6,992.9

Source: OEEC, *The Network of Intra-European Trade, Trade by Product in 1957*.

ble" and "unfavorable" areas would yield an estimate of the total welfare implications of the customs union.

The size of the areas for each commodity depends on the quantity of trade shifted as well as on the tariff rates which are associated with each shift. The 1952 U.N.-GATT study [15, Ch. 4, pp. 12-16] showed that European tariff rates tend to be highest on finished manufactures and lowest on crude materials. At the same time, Table 3 shows that manufactures account for a larger proportion of intra-EEC trade than of intra-EFTAA trade, while the reverse is true for crude materials. Relatively speaking, therefore, commodities with high-tariff incidence weigh more heavily in intra-EEC trade than in intra-EFTAA trade. This, coupled

¹² These changes represent Meade's primary and secondary repercussions. His tertiary repercussions, involving the reaction of individual countries to changes in their balance-of-payments position caused by the formation of a customs union, are excluded from this discussion.

with the fact that the Common Market nations tend in general to have higher tariff rates than their EFTAA counterparts, shows that each "unit" of trade creation in the EEC would tend to have a higher weight (measured by the tariff rate along the coordinate of the central part of Figure 1) than a similar unit in the EFTAA. By the same token, the fact that manufactures weigh more heavily in intra-EEC trade than in intra-EFTAA trade, suggests that S_B would be more elastic in the EEC, leading one to expect a greater magnitude of trade creation in that union.¹³ Although based on aggregate rather than on detailed statistics,¹⁴ these observations suggest that the favorable Area I would be larger in the EEC than in the EFTAA.¹⁵

Finally, on the basis of his welfare criterion Meade concludes that the abolition of import quotas within a customs union would invariably lead to an increase in welfare. The argument was effectively summarized by Sannwald and Stohler [12, pp. 66-67] as follows:

Let country A import steel from B and C and assume that there are quantitative restrictions on the imports from both countries. . . . If these restrictions are removed on the imports of steel from B but not on those from C . . . there will be no decrease in steel exports of C to A as long as A's quota effectively restricts the demand for steel from C. If this demand diminishes it means that the quota has become ineffective and could be removed, so that a possible reduction in the trade leaves economic welfare undisturbed [i.e., at this point there is no divergence between marginal cost and marginal value, namely the trade reduction has a zero "weight"]. . . . The primary expansion of the trade between the partners would incontestably increase welfare and there would be no repercussions except in markets in which there are no or ineffective quotas. In any case, these reactions do not affect economic welfare.

Regional integration can affect quotas imposed on intra-union trade as well as those imposed on imports from outside countries. Both the

¹³ The number of trade units created depends also on the elasticity of demand for imports in the integrating nations (D_A in Figure 1). Since manufactures, machinery and chemicals account for the same proportions of *total* imports in the two unions, there are no a priori grounds for expecting demand elasticities to be higher in one union than in the other.

¹⁴ To get detailed estimates of the size of the "areas" involved, one would need the changes in the quantity of each commodity traded and the tariff rate associated with that change. The first datum could be approximated if we knew the elasticities of demand and supply for each product. The second datum would have to be derived from official statistics. Existing tariff rates, however, apply usually to much more refined commodity classes than the commodity trade data. The need for comparable groupings would involve both a further breakdown of the commodity trade statistics (e.g., to the 570 items in the five-digit SITC) and calculations of approximate tariff incidence levied on each item by each of the trading nations.

¹⁵ With regard to import and export diversion, the evidence concerning the relative "weights" and the relative quantities of trade diverted is mixed and does not lend itself to an a priori statement about the probable comparative effects of the two unions.

EFTAA and the EEC call for abolition of quotas within their respective areas. While it is impossible to construct a precise measure of the relative potential for quota increase within the two unions, a rough comparison shows that such increases may be larger in the EEC than in the EFTAA. The comparison is based on the percentage liberalization of intra-OEEC trade achieved by each member country late in 1958. These percentages ranged from 81 (Norway) to 94 (U.K.) per cent in the EFTAA, and from 90 (France¹⁶) to 99 (Italy) per cent in the EEC [11, p. 180]. For each country in the two unions, the difference between 100 per cent and the percentage liberalization attained prior to integration was multiplied by the dollar value of its 1957 imports from other union members. The resulting product, which may be taken as an indication of the potential increase in intra-union quotas, is 20 per cent larger in the EEC than in the EFTAA.¹⁷ (It should be emphasized that these rough calculations abstract from numerous problems involved in interpreting the liberalization percentages. They therefore cannot be taken as conclusive evidence). Moreover, the leverage for extending quota benefits to outside nations is also likely to be greater in the EEC than in the EFTAA. The initial 20 per cent increase in quotas within the Community on January 1, 1959 was extended in part to nonmember countries, thus bringing about more liberalization than can be expected of the EFTAA. The extension also attests to the EEC commitment to pursue a (common) liberal external trade policy. This may not be the case in the EFTAA where each member will follow an independent policy toward third countries (this statement is confined to quota increase which can be attributed to the integration movement).

On balance, the evidence assembled here demonstrates that the Common Market is more likely than the Outer Seven to have a favorable impact on the utilization of resources in the world. Moreover, the EFTAA seems to have very few of the prerequisites for a favorable welfare effect. A tentative (qualitative) judgment can be made that integration of the Outer Seven may reduce rather than increase international specialization.¹⁸ (This view seems consistent with, but does

¹⁶ In 1957 France suspended her earlier liberalization of quotas on imports from OEEC member countries. However at the end of 1958, with the devaluation of the franc, 90 per cent liberalization was restored. While this restoration coincided with the introduction of the Common Market, there was no direct link between the two events.

¹⁷ On the average, the percentages relating to EEC nations were somewhat smaller than those of the EFTAA countries. But the difference was more than counterbalanced by the larger amount of intra-EEC trade.

¹⁸ A fifth welfare criterion, adopted by both Viner and Meade, would lend further support to this judgment. It states that the more competitive the nations composing a customs union, the more likely is the union to be favorable to welfare. As shown in Section II, the economies of the Outer Seven are highly complementary, and therefore are not likely to

not depend on, a quantitative estimate constructed by Scitovsky [13, pp. 64-76] that a customs union embracing *all* of Western Europe is likely to be neutral to welfare.¹⁹)

This is not to deny the probable existence of other favorable effects since not all possible repercussions were considered here.²⁰ One example is the possible increase in internal competition which would reduce domestic divergences between marginal costs and marginal values caused by monopoly power. The relative effect of the two unions on such internal divergences would depend on the degree of monopoly existing prior to integration and on their ability to enforce proper rules of competition. A second factor is the relative effect of the two unions on international investments. To the extent that regional integration brings about capital flows from capital-rich to capital-poor countries, it is beneficial to welfare since it tends to equalize factor prices. To date the Six seem to have exceeded the Outer Seven in attracting U.S. investments, in infusing capital into their depressed areas, and in promoting outflow of capital to underdeveloped countries. But this position may be reversed once the EFTAA becomes viable (and would rate further investigation at a later date). Other considerations include the greater opportunities for economies of scale provided by integration and a possible favorable terms-of-trade effect (Meade's tertiary repercussions).

Nevertheless, in light of its possible adverse effect on international specialization, and in view of the need to avoid political division within Western Europe, it is important that the EFTAA should not remain a permanent organization. Whether or not the Outer Seven are likely to help attain the goal of an all-European union is the question examined next.

II. *The Outer Seven and an All-European Free Trade Area*

We shall first explore the degree of complementarity of the seven nations composing the EFTAA; and subsequently, the degree of rivalry between the two unions. The data used are from the OEEC *Statistical Bulletins* on foreign trade (Series IV) relating to 1957. These bulletins provide a breakdown of each member country's imports and exports into 260 commodity groups (the three-digit Standard International Trade Classification, with a further breakdown for many groups to the

increase international specialization. This criterion, however, was strongly criticized by Makower and Morton [8].

¹⁹ This estimate was rejected by H. G. Johnson in a review of Scitovsky's book [6].

²⁰ Excluded from the discussion in this paper are the overseas territories of the West European nations. This exclusion resulted in part from uncertainty regarding the eventual status of some territories and their form of association with the two unions.

five-digit classification). The export/import ratio for each commodity was used as an indication of the comparative advantage of a country in producing that commodity;²¹ the higher the ratio, the more likely is the country to have a comparative advantage. Some weight was also given to the absolute amounts of exports and imports. Of the EFTAA nations data are available for only six: Switzerland does not provide a breakdown of its trade by commodity components. Selection of the products for which each nation has a comparative advantage reveals

TABLE 4—PERCENTAGES OF EFTAA^a EXPORTS AND IMPORTS ACCOUNTED FOR BY COMMODITIES IN WHICH ONE, TWO OR THREE OR MORE COUNTRIES HAVE A COMPARATIVE ADVANTAGE (1957)

	Per Cent of Outer Seven	
	Exports	Imports
One Country	76%	58%
United Kingdom	55	22
Denmark	6	11
Norway	3	6
Sweden	5	7
Austria	4	5
Portugal	3	7
Two Countries	15	12
Three or More	4	2
No Advantage	4	30
	100%	100%

^a Switzerland is excluded.

Source: Based on calculations of data derived from the 1957 OEEC *Statistical Bulletins, Foreign Trade*, Ser. IV, 1957.

that 76 per cent of the total EFTAA exports consist of commodities in which only one of the six countries has a comparative advantage (Table 4). By far the largest share is that of the United Kingdom; that share (55 per cent) consists mainly of chemicals, machinery and manufactures (items 5, 6, 7, 8 in the first-digit SITC). Commodities for which Denmark has a comparative advantage consist mostly of food items (number 0 in the first-digit SITC). Sweden's 5 per cent are mainly crude materials (item 2), while for the remaining countries the commodities in the case of which they have a comparative advantage represent all major categories. Commodities for which two of the six nations have a comparative advantage consist of foods, machinery and manufactures. Finally, only 4 per cent of the EFTAA exports consist of commodities in which three or more members have comparative advantage.

²¹ A similar procedure was followed by H. H. Leisner [7].

These figures indicate the extreme complementarity of the economies composing the EFTAA. They show that, from the point of view of domestic political pressures, it should not be difficult to establish the EFTAA. This is not to claim that problems do not exist. Such a thorny issue is the Danish demand for British concessions in the case of certain agricultural products. But if we disregard the agricultural field (and the EFTAA may very well exclude agriculture), it seems that domestic pressures should be at a minimum.

TABLE 5—DEGREE OF COMPETITION AMONG THE TWO EUROPEAN UNIONS

	EEC			EFTAA		
	Proportion of Total		Ratio of % of Exports to % of Imports	Proportion of Total		Ratio of % of Exports to % of Imports
	Exports	Imports		Exports	Imports	
1. Commodities in which both ECM and Outer 7 have no comparative advantage	13%	55%	0.24	13%	48%	0.27
2. Commodities in which ECM and Outer 7 are competitive	65%	28%	2.30	67%	28%	2.42
3. Commodities in which ECM has comparative advantage over Outer 7	18%	9%	1.93	8%	18%	0.46
4. Commodities in which Outer 7 has comparative advantage over ECM	3%	7%	0.48	14%	6%	2.19
Total ^a	100%	100%		100%	100%	

^a Totals deviate from 100% because of rounding.

Source: OEEC, *Statistical Bulletins, Foreign Trade*, Ser. IV, 1957.

As a second step, using the export/import ratio for each commodity for the union as a whole, we listed the products for which: (a) both unions have a comparative advantage and therefore would tend to be competitive; (b) neither of the two unions has a comparative advantage; and (c) only one union has a comparative advantage. Table 5, which summarizes the results of these calculations, shows that in products accounting for 65 per cent of Common Market exports and 67 per cent of EFTAA exports both unions have a comparative advantage. The ratio of total exports to total imports of these commodities is 2.4 in the Common Market and 2.4 in the EFTAA. On the other hand, 13 per cent of EEC exports and the same proportion of EFTAA exports

TABLE 6—COMMODITY COMPOSITION OF THE TWO UNIONS' EXPORTS

	SITC Number	Commodities for Which the Two Unions				Commodities in Which EEC Has Advantage over EFTAA		Commodities in Which EFTAA Has Advantage Over EEC	
		Are Competitive		Have No Comparative Advantage		EEC Exports	EFTAA Exports	EEC Exports	EFTAA Exports
		EEC Exports	EFTAA Exports	EEC Exports	EFTAA Exports				
Food, beverages and tobacco	0, 1	2.3%	2.4%	3.6%	4.0%	4.3%	3.1%	0.9%	1.8%
Crude materials	2	0.6	0.4	4.0	4.2	0.0	0.0	1.0	6.7
Mineral fuels	3	0.1	0.1	2.3	0.8	5.7	2.4	0.0	0.0
Oils and fats	4	0.2	0.2	0.5	0.5	0.0	0.0	0.0	0.0
Chemicals	5	5.1	5.3	0.0	0.0	3.5	1.6	0.0	0.0
Machinery	7	24.9	31.6	0.0	0.0	0.0	0.0	0.1	1.4
Manufactures	6, 8	30.9	25.7	2.4	3.2	4.3	1.4	1.4	3.9
Miscellaneous	9	0.9	1.6	0.3	0.2	0.0	0.0	0.0	0.0
Total		65.0%	67.3%	13.1%	12.9%	17.8%	8.5%	3.4%	13.8%

Source: See Table 5

consist of commodities in which neither of the two unions has a comparative advantage, with a total export/import ratio of .24 and .27 respectively. The remaining products, accounting for about one-fifth of the exports of the two unions, are those in the production of which only one union has a comparative advantage. These figures demonstrate that once the two rival schemes are completed they will be highly competitive with each other. It would thus be in the interest of European producers to strongly oppose unification of the two unions. Not only will the EFTAA impose two adjustment processes on Europe's economies in their path towards all-European integration, but, because of the competitive nature of the two unions, integration between them will be very difficult to achieve.

This analysis is subject to the limitation that the classification of internationally traded commodities into 260 groups may not be sufficiently refined to prove inter-union rivalry (a limitation which does not apply to the case of complementarity). But no finer breakdown is available. Also, the rivalry conclusion would hold even if we allowed for a considerable margin of error. At the risk of further aggregation the point made in the last paragraph can be strengthened. Table 6 presents a distribution of the eight "export percentages" of Table 5 among the major commodity categories. The overwhelming majority of the products in which the two unions are competitive are manufactures, machinery and chemicals. But Table 7, which shows the interunion trade relationships, demonstrates that it is precisely in these categories that the two unions' imports from each other weigh heavily. This means that producers within each one of them have much to lose from economic integration with the other. On the other hand, with regard to their export trade, it is in food, crude materials, minerals and oils that

TABLE 7—EEC-EFTAA TRADE RELATIONSHIPS (1957)

	EEC		EFTAA	
	Exports to EFTAA as Per Cent of All Exports	Imports from EFTAA as Per Cent of All Imports	Exports to EEC as Per Cent of All Exports	Imports from EEC as Per Cent of All Imports
All commodities	22.9	14.9	22.9	25.5
Food, beverages & tobacco	26.9	9.9	28.1	12.5
Crude materials	27.2	14.1	53.8	8.3
Mineral fuels	32.5	2.6	22.3	21.8
Oils & fats	27.2	7.9	34.5	13.3
Chemicals	21.8	22.6	19.4	47.0
Machinery	21.3	26.6	15.2	51.7
Manufactures	20.8	23.7	20.7	41.7

Sources: (1) OEEC *Statistical Bulletin*, July 1959; (2) OEEC *The Network of Intra-European Trade, Trade by Product, 1957*.

interunion exports carry most weight. These are items in which the two unions are least competitive (either neither of them has a comparative advantage or only one of the two does), and (disregarding food) on which European tariffs are very low to begin with. Their producers therefore do not stand to gain much from integration of the two unions. We can conclude that once the two unions are established, there is likely to arise substantial political pressure within each of them against further integration, with a much weaker counterpressure favoring such integration. Thus unification of the Outer Seven would probably lessen rather than increase the feasibility of an all-European free trade area. Unless a link between the two unions is established early in the transition period, the creation of the EFTAA is likely to deepen the division within Europe.

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WESTERN DEVELOPMENT OF A SPARSELY POPULATED COUNTRY: THE RHODESIAS

By STEPHEN ENKE*

There are still some parts of the world which are not depressed by overpopulation and where economic development involves a twentieth-century form of pioneering. In these areas there is some indigenous population, but it is sparse and of very primitive culture, so that development requires immigrants of Western culture. In the last century this was true of New Zealand, British Columbia, and what is now the Union of South Africa, despite the sporadic opposition of Maori, Siwash, and Bantu respectively. Modern examples are Queensland, parts of north-west Canada, and the Rhodesias. To some extent, all these regions have similar characteristics, and so it may be useful to make a case study of the Federation of Rhodesia and Nyasaland.¹

The following are some of the main requirements for economic development in this kind of country: First, more people having the education and skills, ability to organize, and knowledge of government that is part of Western civilization; in practice this means encouragement of selective immigration, but "Westerners"² insist on all sorts of amenities which in return require a great deal of social overhead capital. Second, profitable export opportunities, to attract needed immigrants and capital; but specialization on a few exports means that a wide range of goods must be imported in volume, and high import propensities in-

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¹ This was constituted in 1953 and includes the self-governing territory of Southern Rhodesia and the Colonial Office-administered territories of Northern Rhodesia and Nyasaland. The author wishes to acknowledge the assistance of the following, none of whom is necessarily responsible for the views expressed here: R. J. Randall, Economic Section, Prime Minister's Office; B. J. P. Woods, statistician, Bank of Rhodesia and Nyasaland; Duncan Anderson, chairman, Federal Power Board; Osborne Bellington, assistant director, Central Statistics Office; Colin Kirkpatrick, Board of Directors, Rhodesian Selection Trust; William Taylor, senior lecturer, University College; Clyde Sanger, assistant editor, *Central African Examiner*; F. H. Hartley, director, Municipal Native Affairs Department, Salisbury; Vernon Brailsford, federal director of information; J. A. Scholtz, chief of studies, Roan Antelope Mine; A. Torrance, African personnel manager, Mufilira Mine; R. S. Arnot, chief engineer, Rhodesia Congo Border Power Corporation; the staff of the Federal Archives, and many others in the Federation.

² "Westerners" refers to people who are descended from Western Europeans, belong to their culture, and are white; in Africa they are called "Europeans." I shall use both terms in this sense.

crease the export burden of serving an external debt. Third, a continued and rather steady inflow of capital for several decades, for extra exports can only slowly be substituted for foreign capital; any large interim reduction in capital inflows would drastically reduce domestic economic activity. Fourth, participation by the indigenous population in the market economy of the Westerners; there will be an inadequate labor supply and racial unrest if natives cannot share in the material benefits of economic development. In the following sections, the problems of development in the Federation will be examined in relation to each of these general requirements in turn.

Most of the points can be made by description and the presentation of data. However a model is needed to explain why such an economy, with its high import propensities, must increase its exports by several times any increment in external debt service *and* is so dependent upon continued capital inflows to sustain personal incomes. The macro-economic model developed here for the Federation's economy treats exports and net capital inflows as basic injections and imports as a leakage. Various parameters, linking each of these elements with one another and with personal income, are estimated.

I. Economic Development by Western Immigrants

Economic development of the Federation is of recent origin. There was no permanent white settlement until 1890 when the armed Pioneer Column entered Southern Rhodesia. Seventy years ago the site of Salisbury—today's federal capital and a city with an impressive skyline—was a bare and uninhabited plain. Some of today's "copper-belt" towns in Northern Rhodesia did not exist twenty-five years ago.

Since 1953 the Federation's economy has been experiencing one of the highest rates of gross investment in modern times. This has equaled at least one-third the gross domestic product and over half the aggregate personal consumption. Three-fourths of this gross investment is estimated to be net of depreciation. Gross domestic product has increased from £300 million in 1954 to £361 million in 1958, with an intervening peak of £379 million in 1957.³

This extremely rapid development has been closely and inevitably associated with immigration of Westerners. These people, apart from some that migrate between the copper mines and the Union, have come to settle. Most of them comprise young families from Great Britain. Some of the attractions are higher real wages, greater freedom from government controls, lower taxes, a better climate, and availability of

³ Unless otherwise specified, the data that follow refer to the entire Federation, which includes Nyasaland; but economically the latter is of minor importance, the main economy being in the more settled Rhodesias.

African domestic help. During the peak year of 1957 such immigration amounted to 8 per cent of the Western population. In the two Rhodesias, out of a total population of roughly 5 million, about 1 in 20 is now "European" (i.e. white). Nyasaland, with a more primitive economy, has a very small white population. The population density of the Federation is only about 10 per square mile.

Economic development in the Federation, at the present stage of the native African's culture, is inconceivable without the government, management, skills, capital, and social values of Westerners. The indigenous natives, despite rapid advancement, are still only a few generations from cannibalism and slavery. Their agriculture has no tradition of permanent cultivation or systematic irrigation. A majority of the men have only a limited usefulness as laborers at rough work, although a minority are now acquiring skills, especially when not blocked by European trade unions. Technical knowledge and managerial procedures, so important a part of today's industry and commerce, will be quite beyond the average native for some time to come. Socially, it is unfortunate that economic progress can only occur through the immigration of managers, technicians, business men, farmers, and shopkeepers of an alien culture and different race. But this is an economic necessity if there are to be schools and other social services for the natives—largely financed by European taxpayers—and if the region is not to retrogress economically.

The governments concerned are anxious that immigration should continue despite the recent political unrest in Nyasaland. They do not wish it to slacken for lack of amenities adequate for Europeans. Unfortunately, in the case of countries of small and primitive native population, such immigrants require a lot of new capital just to live. Whole cities and transportation systems have to be built for them and they require schools and hospitals. In recent years housing alone has probably accounted for about 20 per cent of total gross investment.

In most countries a majority of Western immigrants are lower middle class and possess very limited savings. If they are employees of some large Anglo-American mining concern, there will probably be a company township to receive them. However, where enterprise is small or agricultural, there will have to be bond issues for roads, utilities, schools, etc. Not only are immigrants necessary and expensive, but typically they do not bring with them enough personal savings to finance the housing and social overhead capital that they demand.⁴

⁴ The Federation has many ties with the United States. This is partly because its two major exports—copper and tobacco—are also products of the United States but not of Great Britain, and so technical know-how had to be imported from the United States. The pioneer life, too, is in many ways reminiscent of North America a century ago.

II. *The Importance of Current and Future Exports*

Profitable exports are a condition of rapid economic development in areas of primitive culture and sparse population. In this respect the Rhodesias are no exception. Their export industries serve not only to attract immigrants, both directly and indirectly, but they explain the availability of capital from abroad for industrial and agricultural investment and for social overhead projects. Exports constitute the foundation of such economies during their early development. It is only later that this extreme degree of specialization is mitigated and the import propensity of the economy is lessened.

Few Major Exports

At present the industries earning substantial export credits are few in number, and likely to remain so; even if certain domestic light industries do develop in time, high transportation costs are likely to confine their products to the home market.

Exports exceeding £1 million in 1958, listed in order of value, are: electrolytic copper, blister copper, unmanufactured tobacco, raw asbestos, chrome ore, tea, maize, zinc bar and ingots, cobalt metal, apparel, ferro chrome, and lead bar and ingots.⁵ Copper, over half of which in weight and value is electrolytic, accounted for £115 million, or 63 per cent of all exports during 1956, but less in subsequent years. Tobacco has accounted for another 15 to 20 per cent in most years.

Unfortunately, copper has a notoriously fluctuating price. For electrolytic copper the average export price per short ton was £291 in 1956 and £172 in 1958. Tobacco exports are subject to unanticipated percentage changes in output of almost comparable magnitude. And all the Federation's exports of any consequence are always subject to price declines associated with business recessions abroad.⁶

Specialization Makes Debt Service Unexpectedly Onerous

Because there are profitable export lines, production is extremely specialized, and there is a marked dependence upon imports. Specialization is of course typical of any export economy during early stages of development. However, as in most "dualistic" countries, this is complicated by the relatively small number of Westerners and their relatively high incomes. On one hand, there are not enough Westerners to

⁵ Other exports, in descending order of value in the same year were: cattle hides, coal and coke, cotton piece goods, maize, radios, cigarettes, meat, and footwear, all mostly exported to neighboring African countries.

⁶ The distribution of exports is about 50 per cent to the United Kingdom, under 10 per cent to the United States, followed by the Union of South Africa, the Federal Republic of Germany, and India.

justify domestic production of the things on which they spend most of their incomes. On the other, the not inconsiderable total purchasing power of the black Africans is concentrated on a few goods, so that it does become economical to make some of them locally. For example, bed sheets (for whites) will not be home produced, but simple cooking utensils (mostly for blacks) will be.

The Federation, except for certain sumptuary goods for the black African market, imports a very high proportion of the producer goods and consumer goods that it uses. Hence any increase in gross domestic product is likely to lead to induced imports. Not only will the extra copper and tobacco output for example require more imports—e.g., capital equipment and petroleum products to produce them—but resultant increases in wages and local profits will be spent on cars, furniture, clothes, etc.

Hence, to earn a given value of foreign exchange for debt service, there must be a much greater increase in gross exports. For instance, if £100 of extra exports tend to be associated with about £75 of extra induced imports, the economy will have to produce extra exports valued at £240 yearly to service annually a £1000 debt nominally requiring interest and principal payment of £60.

A developing country such as Rhodesia must obtain most of its capital initially from abroad, because the small immigrant population brings inadequate capital with it. And, taking the economy as a whole, it must be able through exports to pay a return to nonresidents on borrowed capital. This is so whether or not the capital they have lent has been invested in producing exports. Fortunately, for many decades, the economy need make no net capital repayments so long as foreign investors are obtaining a return and feel that their investments are safe; in fact, over short periods of time, a return on past foreign investments may be currently financed by continued capital inflows.

During recent years the Federation's "export rate of return" on total capital inflows has been roughly 10 per cent. This is obtained by dividing the increase in exports from 1954 to 1958 (line 1, Table 1) by net capital inflows during 1954 through 1957. Arithmetically, it is 9 per cent if only net long-term capital movements are considered, and 8 per cent if short-term capital movements are included. While the actual increase in exports may seem respectable it must also cover extra *induced* imports.

Estimating Necessary Gross Export Ratios

The extra exports needed to finance one pound sterling of extra debt service are here considered to be the reciprocal of one minus the propensity to import. The propensity to import is taken to be the ratio of an-

nual imports to what are here termed annual gross injections. Gross injections are defined as the sum of exports and gross investment. If relations are stable between producer-goods imports and gross injections, between consumer-goods imports and personal income, and between personal income and gross injections, we can perhaps surmise something about the imports that will be induced by the export component of gross injections.

Pragmatically, the existence of stability among these relations may

TABLE 1—NATIONAL INCOME AND TRADE MAGNITUDES AND COEFFICIENTS
(pounds in millions)

	1954	1955	1956	1957	1958
Categories:					
1. Exports, ¹ E'	116.7	125.2	124.9	142.9	134.5
2. Gross investment, K	86.0	113.1	144.2	151.6	128.3
3. Gross injection, I_g	202.7	238.3	269.1	294.6	262.8
4. Personal income, ² Y_p	187.9	208.5	236.6	255.5	268.7
5. Imported producer goods, M_p	73.7	80.7	99.4	109.5	NC
6. Imported consumer goods, M_c	78.2	91.5	95.6	107.4	NC
7. Total imports, M	151.9	172.2	195.0	216.9	196.0
8. Current transfers (net)	+2.8	+3.1	+3.8	+2.8	+4.4
9. Undistributed profits, etc. ³	21.1	33.0	32.7	2.1	2.3
10. Current Account	-11.3	-10.9	-33.6	-70.0	-54.8
Coefficients:					
11. M_p/I_g	.37	.34	.38	.37	}74
12. M_c/I_g	.37	.36	.37	.37	
13. Y_p/I_g	.94	.87	.85	.87	
14. M_c/Y_p	.42	.42	.41	.43	NC ⁴

¹ Goods and services gross, income payments net, minus undistributed profits of companies.

² Income earned from labor and property in the Federation.

³ Undistributed profits of companies and statutory bodies, mostly held in London.

⁴ NC, not comparable.

Line (3) = (1) + (2)

(10) = (1) - (7) + (8) + (9)

Source: Annual federal *Economic Report*.

be as important as the validity of their explanation, and so it is reassuring that Table 1 indicates that most ratios vary little from year to year: producer-goods imports and consumer-goods imports tend to be .36 and .37 respectively of gross injections each year. Thus extra exports of £100 will perhaps induce extra imports of £73 and so improve the merchandise trade balance by only £27. The ratio of necessary gross exports to debt service is then $1/(1 - .73)$, or a little less than 4. The algebra is given in the appendix.⁷

⁷ In obtaining these coefficients, and because of the manner in which the large Northern Rhodesian mining companies operate, certain adjustments had to be made in the data. These copper companies, although legally registered in the Federation, are in effect "foreign"

Some Practical Consequences

Countries that have a high rate of induced imports should take this into consideration in assessing the ability of the home economy to service external debt. So should the agencies that lend to these countries. Exports may have to increase by an amount several times greater than is commonly supposed.

The Kariba project to dam the Zambesi, thereby producing electric power, may or may not be a case in point. The immediate effect on merchandise trade is slight, some imports of electric power from the Congo perhaps being eliminated. However, as a result of more abundant power, more copper may be exported in future years. Also coal may be released for export. Cheap and plentiful power may stimulate some new industries replacing imports. New borrowing to expand railroad capacity may be obviated. Nevertheless, if now-contracted interest and principal payments are to be met without depressing the balance of the economy, by around 1970 additional exports somehow attributable to Kariba and amounting to about £25 million a year may be needed. This "economic" requirement, for reasons explained above, is about four times as great as the legally contracted debt service.

In a developing economy such as the Federation's, the expansion of which depends in part on providing European immigrants with the facilities they expect, many recent loans have been spent in improvements that do not stimulate exports directly at all. And so it is to be hoped that they will in turn lead to other investments that will produce exports and substitutes for imports. The crucial test is whether, concurrently with all these new outlays that are financed by nonresidents, the merchandise and service accounts will in time become sufficiently less unfavorable to meet at least interest charges plus the cost of the consumer- and producer-goods imports induced by these exports. It appears that the export rate of return—on all loans from abroad together—needs to be about four times the financial rate of return required by most lenders abroad.

companies. They credit most of their sales proceeds to London bank accounts and remit to the Federation only those funds needed there for payroll, local material purchases, taxes, etc. Dividends and interest are paid from London to security holders most of whom live outside the Federation. When they undertake large investments in the Federation, the capital funds come from outside the local economy. Accordingly, as an approximate adjustment in all tables, the undistributed profits of these companies have been subtracted from reported merchandise exports, and conversely their investments in the Federation have been treated as capital inflows. There are of course many other small countries in which a few companies with large direct investments operate in this manner. In such cases the published balances of payments must be interpreted carefully for the reasons given here. One important consequence is that the local economy is *partially* insulated from fluctuations in the prices of these large companies' exports, even though high copper prices mean increased output, royalties, local taxes, wages and bonuses.

III. *Dependence Upon Continued Capital Inflows*

The Federation, like many another immature creditor country before it, is dependent upon a continued inflow of capital if economic activity is to be maintained. Since imports are a leakage from the payments circuit, a high propensity to import makes for a "leaky" economy that requires offsetting injections of merchandise exports and foreign capital.

Moreover, the Rhodesias have lately emerged from a period of frenzied growth often experienced in newly settled areas. The coming of federation, high prices for copper until 1957, and a comparatively high rate of European immigration combined to instigate, and until recently to maintain, a boom in city real estate. The Kariba project, costing £80 million, stimulated both enthusiasm and the economy. Today optimism is less extreme, and people appreciate the vulnerability of the local economy to reductions in export proceeds and loans from abroad.

What capital inflows will be needed during the next few years for per capita incomes to continue at something like their recent levels?⁸ Any attempt at an answer must be based on obviously rough estimates and some crude assumptions (see Table 2). First, relying upon the relations recorded in Table 1, necessary gross injections are set at 1.11 (the reciprocal of .9) times the required aggregate personal income.

Second, an estimate of future export proceeds is made (see line 3 of Table 2).⁹ Extrapolated export proceeds fall short of necessary gross injections. One part of the difference will have to be net capital inflows. But the other part will be unleaked gross injections of previous years. (See the appendix for definitions and algebra).

It is supposed that the Federation's economy is a circulating monetary system, subject to leakages and injections. The leakages each year are imports—considered to be .75 of gross injections—and external debt service. The gross injections each year are exports, capital inflows from abroad, and unleaked gross investment that remains from the injections of previous years.¹⁰ In other words, it is assumed that old gross

⁸ Recent per capita annual incomes have been around £500 for Europeans (including Asiatics) and £12 for Africans, the large difference being partly explained by most of the latter being in the subsistence economy. Allowances should be made for increases in population during the next few years. Aggregate personal income must then be £271 million in 1960, £288 million in 1961, and £299 million in 1962 if per capita incomes for both races are to be maintained.

⁹ During 1954 through 1958, exports (less undistributed profits of the copper companies) climbed slowly from £117 million to £135 million. A similar annual percentage rate of physical increase, but with copper at a more normal price of £240 a long ton (as compared with £172 in 1958), would bring such adjusted exports to about £193 million by 1962. These increases may come primarily from a continued shift in production from blister to electrolytic copper; increased acreages, yields and qualities of tobacco; and greater output of asbestos and chrome. In Table 2 projected exports are adjusted by not more than £2 million in any one year so that required long-term capital inflows will be a constant magnitude.

¹⁰ Together, excluding exports, they comprise gross investment (see Section II).

injections continue to circulate each year as induced injections until leaked away, which occurs very rapidly with high import propensities. The new injections each year are exports and capital inflows. Given the annual gross injections estimated as necessary to provide the required aggregate personal income, and given the other various injections and leakages, one can estimate the level of lending from abroad that is needed.

This approach really ignores domestic injections and leakages that are independent of external injections and leakages. The domestically financed part of gross investment, in any one year, is held to be "induced" in the sense that unleaked net injections from the outside world continue to circulate. The Rhodesian economy is so dominated by its external transactions that it seems permissible to disregard any leakage from unspent domestic savings.¹¹ And as a practical matter most small countries have more and better information regarding their external transactions—and publish a fairly detailed annual balance of payments—than they do regarding domestic saving and domestic autonomous investment. Hence the method of analysis used here may be valid and necessary for small countries deeply involved in the world economy.

Table 2, calculated in this way, suggests that the minimum annual foreign lending is about £65 million (Table 2, line 2).¹² This is about the same as in the past.¹³ Despite the assumed growth in exports, at least the past rate of borrowing from abroad still appears necessary, partly because of growing population. Long-term capital inflows of this magnitude, as Table 2 also indicates, would prevent pressure on the short-term account.

These minimum capital inflow requirements are not inconsiderable. After all, £65 million annually in net loans is the equivalent of an entire Kariba project every 15 months; and the fact that corporations, individual investors, and international lending agencies have made loans at almost this rate in the past provides no assurance that they will continue to do so. A halving of this lending rate, according to our assumptions, would occasion a fall in personal income of about one-tenth in even the first year. So it would seem that international lending institutions have a responsibility not to encourage a temporary rate of world lending to a small economy if this lending rate is not likely to be sus-

¹¹ Or one can assume that they are offset by domestic investment that is "autonomous," in the sense that it is independent of external injections and leakages.

¹² "Minimum" because only formerly contracted interest is excluded from exports, E'.

¹³ During 1954 through 1958, the annual net inflow of capital averaged about £56 million, of which £50 million might be termed autonomous (long-term) and £6 million induced (short-term). The balance of gross investment, about £71 million annually, was officially reported to have come from domestic sources. However this is here treated as unleaked injections of previous years (line 4, table 2).

TABLE 2—MINIMUM FOREIGN CAPITAL REQUIREMENTS, COMPATIBLE WITH EXPORT PROJECTIONS, TO MAINTAIN PER CAPITA PERSONAL INCOMES AT RECENT LEVELS THROUGH 1962^a

(millions of pounds)

	1960	1961	1962
Changes in the Circuit of Payments			
1. Necessary gross injections, I_0	305	319	332
2. Required long-term capital inflows, C	65	65	65
3. Exports, E'	167	181	193
4. Unleaked previous gross injections, J_{t-1}	73	73	74
5. Foreign interest payments, D'^b	2	4	6
6. Foreign debt repayment, D''^b	1	2	3
7. Imports, M	229	239	249
8. Unleaked gross investment, J	73	74	74
Changes in International Balance of Payments			
9. Merchandise and services	-62	-58	-56
10. Interest paid abroad ^b	-2	-4	-6
11. Current account	-64	-62	-62
12. Long-term capital movement ^b	+64	+63	+62
13. Short-term capital movement	+0	-1	+0

Derivations: (See also footnote 19)

- | | |
|---|------------------------------|
| (1) See Table footnote | (9) Lines (3) - (7) |
| (2) Approximate projection: such that lines (2) + (3) + (4) = (1) | (10) Minus line (5) |
| (3) Projection, adjusted (see fn. 9) | (11) Lines (9) + (10) |
| (4) Line (8) of year before | (12) Lines (2) - (6) |
| (5) £2 million × no. years | (13) Minus lines (11) + (12) |
| (6) £1 million × no. years | |
| (7) .75 of line (1) | |
| (8) Lines (1) - [(5) + (6) + (7)] | |

^a The population assumed for 1960 through 1962 in millions is respectively .365, .385 and .405 for Europeans and Asiatics and 7.690, 7.900 and 8.078 for Africans. Per capita incomes are assumed to be £500 and £12 respectively for the entire European and African populations. Necessary gross injections are set at 1.11 times required aggregate personal income.

^b Excluding payments arising from past borrowings from abroad.

tained. The political effects of sudden deflation may be particularly unfortunate in the case of a multiracial country.

IV. *Native Participation in the Market Economy*

Economic development in a dualistic economy is associated with a slow penetration by the Westerners' market economy into the subsistence economy of the indigenous population. Indeed, in the Federation, one measure of economic development is the extent to which the natives become part of the money and market economy of the Western immigrants and settlers. And another measure is the extent to which the participating natives benefit thereby. In both respects the Rhodesian record over the decades has been remarkable.¹⁴

¹⁴ Especially when one realizes how recent is "civilization" and that, over much of what is

Traditionally, the African man places a high value on irregular work involving danger and excitement. In peacetime the men have concentrated on hunting game and grazing cattle. Hence the cultivation and grinding of mealies, the fetching of water and wood, in addition of course to cooking, mending, and child-rearing, are the chores of the "weaker sex." Women, as the main providers, still have considerable value, whether as wives to be bought or as daughters to be sold.¹⁵ Almost until the first world war, although the white settlers obtained some native labor, the local population had little need or use for the Europeans.¹⁶

An important factor in overcoming the barriers between the two cultures and economies has been the natives' strong desire for European goods, partly for their use value, but also as prestige items. The men now want "city" clothes and shoes, hunting knives, flashlights and bicycles. The women want yardage goods, simple cooking utensils and gewgaws. Sugar and soap, matches and cigarettes, and inevitably "soft" drinks, are frequently bought. In the native reserves, and for agricultural workers on European cultivated land, these are among the more important purchases by the African from the money economy.¹⁷

To purchase these limited items and pay taxes an African must acquire money. If on a native reserve, he will occasionally sell a cow or bag of maize "outside." However, to an increasing extent, some of the men seek work in the towns, down the mines, and on the farms, leaving their wives in the reserves to tend cattle, grow mealies, and raise children. In the towns, unless he has some special skill, the native will earn some rations and about 30 shillings a week; on a European farm he will get less, but have a lodging or be given time to build a hut; but on the copper belt he will cost his employer 20 shillings a work day, which explains the very low labor turnover there.

A growing tendency is for some natives to settle, more or less permanently, in the African townships located near the European cities. In this case the family may move from the reserve, and some sort of municipal housing will usually be rented, if available. The family, without real income from farming, is now entirely dependent on the money economy. To some extent this trend is being accelerated by the increase

now Southern Rhodesia, the Matabele tribesmen were hunting the Mashona people just over seventy years ago.

¹⁵ The old tribal practice of paying a bride price (*lobola*) was only part of the marriage negotiations; today the price may be paid in cash rather than cattle.

¹⁶ Early railroad construction in Rhodesia could never depend on local native labor.

¹⁷ The native hut can be built in a few days and contains no purchased materials. Beer, until recently the only alcoholic beverage permitted, is home-brewed. A native working for a European gets a ration of mealies, meat, salt, and sometimes nuts and oil.

in the native population on the limited acreage originally set aside as native reserves.

At the time of the last census, in 1956, the "economically active population" in the European market economy was put at 110,000 Europeans, 3,000 Asiatics, and 1,037,000 Africans. In terms of the estimated populations by race, this placed 42 of every 100 Europeans and 14 out of every 100 Africans in the European market economy for purposes of earning income. Considering the high proportion of African children, and the rarity of native women working for a wage, this means that at least one out of three adult male Africans had left the subsistence economy.

The most important industries as employers of African labor were, in descending order, agriculture and forestry, construction, manufacturing, mining and quarrying, commerce and finance, government administrative services, and medical and educational services, these categories accounting for 85 per cent of all African wage-earners.

Additional estimates concerning African participation in the European-market economy of the Federation are given in Table 3. From 1954 to 1958 there has been an increase of 17 per cent in those economically active in the market economy; this compares with an estimated 13 per cent increase in native population. The average income of these participating Africans rose from £65 to £87. This increase, of one-third in four years, is in marked contrast to the almost static per capita return to the European population during the same period. It indicates the extent to which the Federation's economy, despite restrictions on job advancement in most of the European trade unions, is increasingly producing for the African.¹⁸

Africans now earn, as wages or from small businesses, about £2 for every £3 earned by Europeans and others. As consumers, seldom paying income taxes, the African sector commands goods and services approaching £100 million a year. This purchasing power is gaining Africans practical equality as effectively as legislation can provide it.

V. *Conclusions*

Some economic lessons from Rhodesian experience may be applicable to other underdeveloped areas having sparse and primitive native populations. First, a country that is dependent for economic development on a rapid increase of Western population must invest in all sorts of expensive amenities that only indirectly earn exports. Second, a high rate of induced import leakage sharply increases the export rate of return that the economy as a whole must be able to earn on borrowing from

¹⁸ The estimated per capita annual income obtained by Africans from the European-market economy increased from £9.5 in 1954 to £13 in 1958.

TABLE 3—COMPARATIVE POPULATION, PARTICIPATION AND EARNINGS IN THE EUROPEAN MARKET ECONOMY, BY RACE, 1954-1958

	1954	1955	1956	1957	1958
1. Estimated Population (in thousands)					
a. European, Asian and colored	240	257	281	305	321
b. African	6,630	6,810	6,980	7,140	7,460
2. Estimated Economically Active Persons ^a (in thousands)					
a. European, Asian and colored	105	112	120	128	134
b. African	960	998	1,037	1,079	1,120
3. Wages and Salaries (in million pounds) ^b					
a. European, Asian and colored	91.5	104.0	118.1	125.0	130.4
b. African	54.5	61.6	69.6	79.0	83.9
4. Income from Unincorporated Enterprises (in million pounds)					
a. European, Asian and colored	25.7	25.9	28.8	28.3	28.0
b. African	8.6	9.2	10.4	12.0	13.6
5. Estimated Income per Economically Active Person (in pounds)					
a. European, Asian and colored	1,112	1,151	1,218	1,198	1,179
b. African	66	71	77	85	87
6. Estimated Income per Capita from the European Economy (in pounds)					
a. European, Asian and colored	486	504	522	504	495
b. African	9.5	10.4	11.5	12.8	13.0

^a Economically active persons are those participating in the European market economy rather than in the native subsistence economy.

^b Includes wages in kind, such as rations, but not unsold subsistence income now valued at about £70 millions.

Source: Recomputed from data in the federal *Economic Report*, 1957, 1958 and 1959.

abroad, if it is to attract more capital and immigrants. Third, a constant inflow of capital is of great importance to a relatively small economy. Fourth, in a multiracial society, largely dependent on native labor, the indigenous population must increasingly participate in the market economy and benefit from it.

APPENDIX

The following is a more formal statement of the model first described in Section II and further developed in Section III.

The symbols used, all being annual values, are:

E' , exports, adjusted as explained in footnote 7;

D , extra debt service, including interest and principal repayment;

M_p , producer goods imports;

M_c , consumer goods imports;

M , imports;

K , gross investment;

• I_g , gross injections;

Y_p , aggregate personal income;
 L , leakages to the outside from the economy;
 J , unleaked previous gross injections;
 C , capital funds inflow.

An asterisk indicates a stipulated or required value.

In Section II the relationship of necessary extra gross exports, that is the ratio of E'^* to D_1 was stated to be

$$\frac{1}{1 - (a + b)},$$

where a is M_p/I_g (actually .36) and b is M_e/I_g (actually .37). $I_g = E' + K$. M_e is held to be .37 of I_g because Y_p is .9 of I_g and M_e is .41 of Y_p .

In Section III, there is a stipulated personal income Y_p^* , held to be .9 of the thereby necessary gross injection I_g^* . There are certain leakages, L , and injections, I_g , in each time period. $L = M + D$ and $I_g = E' + K$. But $K = C + J$. And $J = I_{gt-1} - L_{t-1}$, thus $I_{gt} = E'_t + C_t + I_{gt-1} - L_{t-1}$. The value ascribed in this section to $a + b$ is .75 and hence $M = .75I_g$. It is assumed that any unspent domestic saving by some is offset by autonomous domestic investment by others. Given I_g^* , E' , D , and the import propensity, the task is to find the necessary capital inflow, C^* .

A MODEL OF PRICE FLEXIBILITY

By JOSEPH V. YANCE*

While the accepted theory of price determination is that of Marshall, in which price is mutually determined by supply and demand, the empirical observations of price behavior that were stimulated by Means' work [7], particularly those of Ruggles [10] seem to show that in fact demand has very little effect on industrial prices. Most price changes can be accounted for by changes in direct costs for labor and materials. Here we expand on this notion and show that there is a delay between cost changes and the corresponding price changes. In particular, we show that a distributed delay describes the data rather well. One interesting feature of the model is that a natural measure of price flexibility emerges which meets important objections to Means' measure.

Furthermore, the model seems to answer some of the everyday questions that arise concerning the pricing process. In 1957-58 prices were continuing to rise even though demand was dropping. This was thought to be rather paradoxical, and yet if there are delays in passing on increased costs, this sort of inflation could very well be the result of cost increases in the business system that take time to work themselves out.

Another practical area to which the model has relevance is the accountant's concern with the effect of various types of inventory evaluation methods on profits, taxes, and so on. For the most part, in accounting texts, prices are assumed to go up at the same time that costs go up, or at the time they are reflected in cost of goods sold, but the present model furnishes a more realistic assumption.

In measuring delays of this type, Chenery and others (cited below) have developed statistical tools. An extension of these methods developed here is to show the consistency of results using time intervals of different length, something that is of interest particularly in using statistical results for simulation or whenever there is a need to change

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• Forrester and in part by general funds of the School of Industrial Management.

from a time interval of one length to another. A second extension is to measure the difference in the delay of the price response to an upward, as opposed to a downward, cost change.

I. The Model

We assume that in equilibrium the price of a commodity will be a linear function of the price of raw materials and the wage rate. Furthermore, taking this same linear function of the current cost elements, we can define a hypothetical entity for the present time, the "normal price"

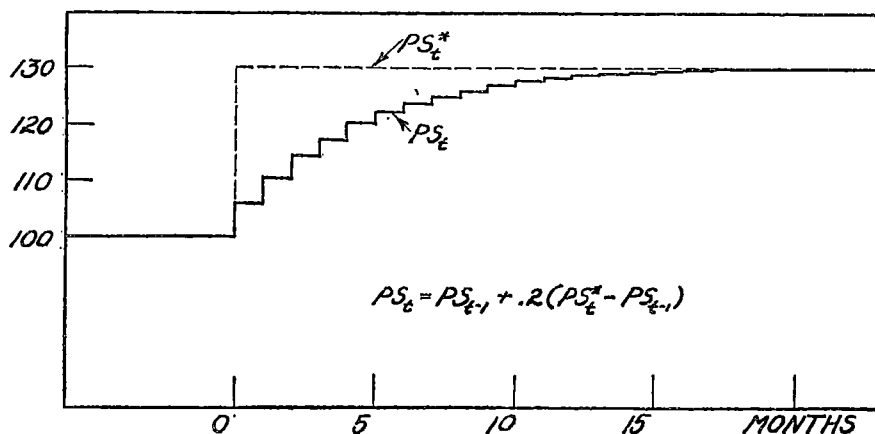


FIGURE 1. RESPONSE OF SHOE PRICES TO STEP INCREASE IN COSTS, PS_t^*

of the commodity. This is the price to which the actual price would tend if the costs remained at their current levels. Thus we define:

$$(1) \quad P_t^* = b_0 + b_1 PR_t + b_2 W_t.$$

P_t^* —current normal or equilibrium price for finished goods.

PR_t —current price of raw materials.

W_t —current wage rate.

In addition, we postulate a mechanism for the way in which an industry changes its price to approach the normal price. The dynamic mechanism proposed here is that the price change this period will make up part of the difference between last period's price and the current normal price, i.e.,

$$(2) \quad P_t = P_{t-1} + a(P_t^* - P_{t-1})$$

P_t —actual price this period.

This mechanism represents a distributed delay¹ in the sense that if normal price moves from one level to a higher level, the actual price will gradually approach it. An illustration of the response for shoe manufacturing to a hypothetical increase in normal price is shown in Figure 1. The periods are months, and the value of $a = .2$ was obtained in a manner to be described below.

The most awkward question that economists raise about the model is its interpretation in terms of classical theory. The present model obviously makes a host of assumptions relative to classical theory, but the most critical point would seem to be what is assumed to happen at capacity. The model assumes that the margin between price and costs will not change significantly even then, whereas in classical theory price is the equilibrating mechanism and would rise at capacity. In real life I believe that orders are likely to back up and the increased delivery delay serves as an additional, and perhaps more important, equilibrating mechanism than price. In commodity markets we wouldn't expect this to be true; there the margins would be likely to increase as demand increased, but not in manufacturing.²

Not only would the pricing behavior implied by the model restrict its applicability to manufacturing as opposed to commodity markets, but the continuity of price changes implied by the model would seem off-hand to make it inapplicable to industries like steel and aluminum where prices change rather discretely.

Perhaps the most useful practical implication of the model, in view of the current interest in price behavior, is the measure of price flexibility that emerges. There is only one constant, a . It is obvious that the larger the a , the faster will the model approach the normal price, and therefore a large a implies more flexible prices.

A more specific connection can be shown between the magnitude of a and a measure of price flexibility, however. In terms of the current model, a natural measure of price flexibility is the average delay between a cost change and the price changes that it causes. If we consider a single cost change, part of the resulting price change will occur the first month, a smaller part the next month, and so on. We can calculate the average number of months that the various proportions of the complete price change are delayed. In the Appendix we show that this average delay is:

¹The mechanism described is the simplest type of distributed delay, an exponential or first-order delay, and has been used to describe a number of economic phenomena: the response of investment to demand (Chenery [21]); the response of dividends to profits (Lintner [51]); and the response of agricultural supply to prices (Nerlove [9]).

²In the only study that I know of along these lines, Goris and Koyck [4] examined steel prices to see if margins between costs and price varied with demand, but the study did not reveal any conclusive evidence that they did.

$$\bar{D} = \left(\frac{1}{a} - 1 \right) \text{ periods.}$$

For example, with an a of .2, the average delay in the resulting price changes is four months.

In addition, it is a property of this model that even when the cost series to which the model is responding is an actual series, we can pinpoint the effect of a given cost rise and show that the effect occurs with the same average delay.³ Thus this measure of price flexibility is independent of cost fluctuations, something that is not true of Means' tallies of price changes per unit of time, a shortcoming to which one can logically object.⁴

A second advantage of this measure of price flexibility is that we can meaningfully deal with price indices and average wage rates. The Means-B.L.S. approach is restricted to studying individual commodities, since their measures are not sensitive enough to deal with price indices. And yet what is needed, it would seem, are broad measures for an industry, rather than specific measures for "2 Shoes, childrens, Goodyear, elk or kip upper," "1 Escalator," etc., as in [12].

II. *Application of the Model*

The model has been applied to the U.S. tanning and shoe manufacturing industries. The model seems to explain the price changes rather well, and the results indicate that tanners adjust their prices in response to costs more rapidly than shoe manufacturers do. The average delay in tanning is one month and in shoe manufacturing about four months.

Not only are the lengths of the delays plausible from what is known about pricing in these industries, but the distributed nature of the delays is plausible. Prices do seem to take some time to make a full adjustment, and even though particular manufacturers make discrete price jumps, the aggregate effect is of a more or less continuous response. This is what the model attempts to describe; not the fact that price changes occur after one month or after four months, but that in the aggregate there is a continuous adjustment which averages one or four months.

The way an individual firm's behavior enters into these aggregate responses is suggested by some other observations. Richard Maffei of M.I.T., in connection with consulting work for a tanner, has observed that when hide prices go up, management gets ready for continued high

³ This is shown in the Appendix.

⁴ It is interesting to note, however, that in spite of the difference between the measures, the conclusions are the same with respect to the greater price flexibility of industries closer to commodity markets (e.g., tanning vs. shoe manufacturing).

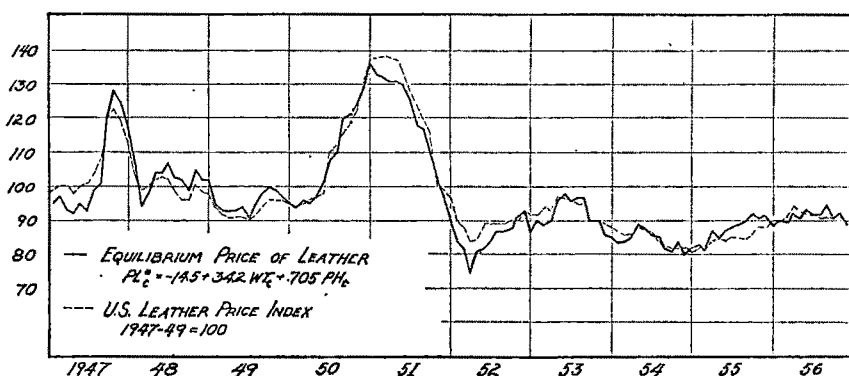


FIGURE 2. EQUILIBRIUM AND ACTUAL LEATHER PRICES (SHOW EFFECT OF SHORT DELAY)

prices over the next few weeks, and if they continue high, will raise leather prices. How long they wait will determine their price flexibility.

A report similar in feeling was published in the *New York Times* [1]:

Prices of hides have about doubled in the last four months, and the consumer, as a result, can expect higher retail prices for shoes next fall. Several footwear manufacturers have announced price increases of about 8% on fall lines. Some have not raised prices yet, but have indicated that an increase will come unless prices recede.

Another effect of the pricing delays is shown in Figures 2 and 3. A mechanism like this gives a price series that not only is a delayed, but is a smoothed and damped version of the cost series. Furthermore, with a smaller a the price series will be more smoothed and delayed. This is seen by comparing tanning with an $a = .48$ and shoe manufacturing with an a of .19. The solid lines in Figures 2 and 3 are the normal

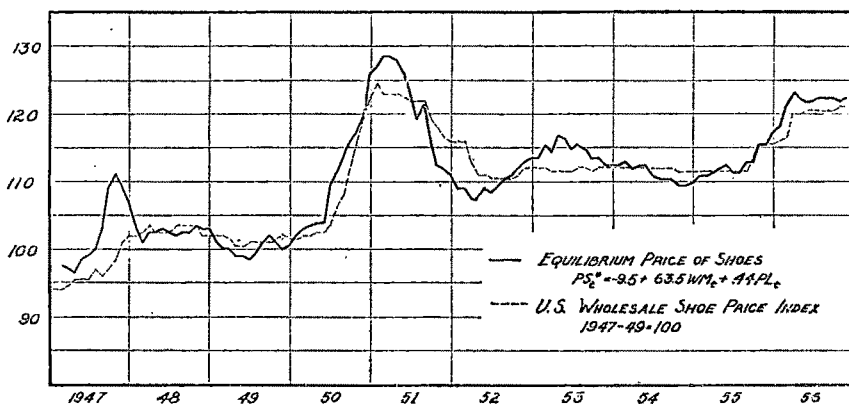


FIGURE 3. EQUILIBRIUM AND ACTUAL SHOE PRICES (SHOWS EFFECT OF LONG DELAY)

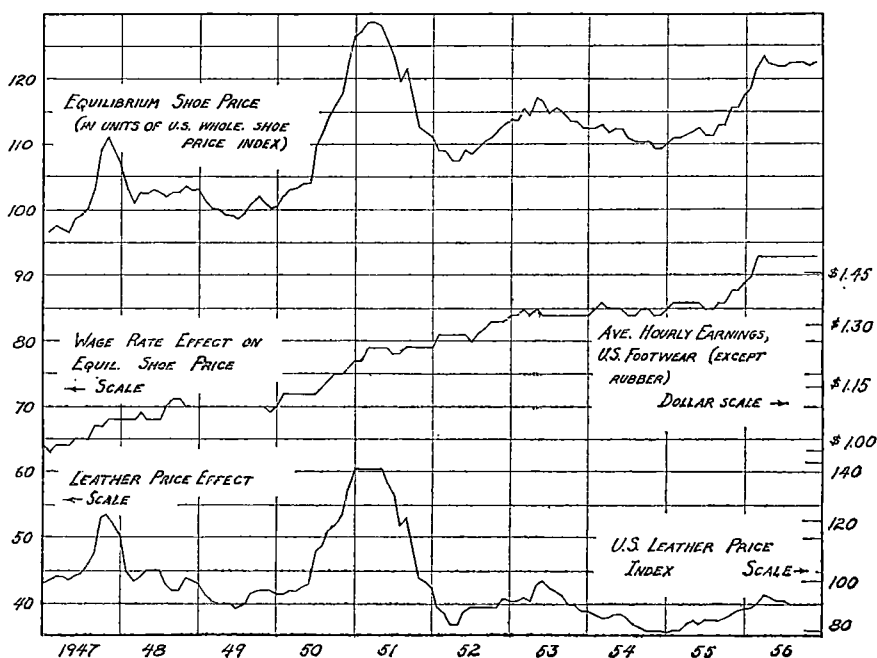


FIGURE 4. EQUILIBRIUM SHOE PRICE AND ITS COMPONENTS, LEATHER PRICE AND WAGE RATE

prices, i.e., the linear combination of costs to which the actual price will tend. It will be seen that for tanning the actual price, the dashed line, though somewhat damped and smoothed, follows its normal price much more than shoe manufacturing follows its. Note that we are illustrating not the goodness of fit of the model—we are not plotting the actual price against model-generated price—but the effect of the various delays.

Figure 4 illustrates how the separate cost components look. The series for wage rates and raw material prices have been scaled according to weights that come out of the regression analysis to be described.⁵

III. Method of Estimation

We shall now describe the statistical procedures used to obtain estimates of the response coefficient a , in (2), and the constants of the normal price equation, (1):

⁵ All series are from January 1947 through December 1956. All regressions are done on these observations, so that if, for example, a regression uses independent variables lagged one period, there are 119 observations. The price indices are all from the B.L.S. as they appeared in [8]. They include the Index of Hide and Skin Prices, Index of Leather Prices, and Wholesale Index of Shoe Prices. For all three series, 1947-49 = 100. The wage rates are average hourly earnings of production or non-supervisory workers for *Footwear (except rubber)* and *Leather: Tanned, Curried, and Finished*, expressed in dollars, and taken from [11].

$$(1) \quad P_t^* = b_0 + b_1 PR_t + b_2 W_t$$

$$(2) \quad P_t = P_{t-1} + a(P_t^* - P_{t-1})$$

The variable P_t^* is a hypothetical variable, the normal price, and therefore cannot be measured directly. We could obtain estimation equations without this variable by substituting equation (1) into (2), getting an equation for P_t in terms of P_{t-1} , PR_t and W_t . Although this would lead to the desired estimates, it is not a satisfactory method because, in time series like P_t , there is apt to be a high correlation between successive values of prices; we would overstate the explanatory value of the model. Instead, equation (2) can be written in terms of the change in price, at the same time substituting for P_t^* its value from (1):

$$(3) \quad \Delta P_t = a[(b_0 + b_1 PR_t + b_2 W_t) - P_{t-1}]$$

$$= ab_0 + ab_1 PR_t + ab_2 W_t - aP_{t-1}$$

The term ΔP_t is not necessarily correlated with P_{t-1} so that (3) is a firmer starting point. From equation (3) it is rather obvious how the desired estimates can be made: the coefficient of P_{t-1} gives us an estimate of a ; we then divided this into the estimated constant and the other estimated coefficients to obtain estimates of b_0 , b_1 and b_2 . In addition, from the estimate of a we can calculate the estimate of the average delay.

In discussing the empirical estimates, it should be noted that the measures of goodness of fit which are used are the squares of the multiple correlation coefficient, and secondly that they refer to an explanation of the change in price rather than to the absolute level of price. Since the results are stated in this ultra-conservative way, an R^2 of .50 to .75 is quite satisfactory.

A. Basic Estimates

We shall show how this procedure was followed with the tanning and shoe manufacturing data, first giving the original regressions and then the estimates of interest to be inferred from these. It is worth going through the manipulations once in detail to facilitate the reading of the next section, where the results using different time intervals are compared.

In the notation,

PH_t —Price index for hides, i.e., raw material to the tanner.

PL_t —Price index for leather; finished goods price of the tanner in the tanners' price response; raw materials price of manufacturing in its price response.

PS_t —Price index for shoes; finished goods price for shoe manufacturer.

WT_t —Average hourly earnings in tanning.

WM_t —Average hourly earnings in manufacturing.

The original regressions are:

$$(4a) \quad \Delta PL_t = -7.01 + .340PH_t + 16.5WT_t - .483PL_{t-1} \quad R^2 = .48$$

$n = 119$

$$(4b) \quad \Delta PS_t = -1.76 + .081PL_t + 11.7WM_t - .186PS_{t-1} \quad R^2 = .41$$

$n = 119$

The response parameters are the coefficients of the lagged price; for tanning,

$$a = .483,$$

and for shoe manufacturing,

$$a = .186.$$

Therefore the average delays are, respectively,

$$\overline{DT} = \left(\frac{1}{.483} - 1 \right) = 1.07 \text{ months}$$

$$\overline{DM} = \left(\frac{1}{.186} - 1 \right) = 4.38 \text{ months}$$

These are the results referred to above, an average delay of about one month in tanning and four months in manufacturing.

The three terms to the right of the equality sign in (4a) and (4b) represent the normal price equation multiplied by the response parameter. So the normal prices are:

$$(5a) \quad PL_t^* = \frac{1}{.483} [-7.01 + .340PH_t + 16.5WT_t]$$

$$= -14.5 + .70PH_t + 34.2WT_t$$

$$(5b) \quad PS_t^* = \frac{1}{.186} [-1.76 + .081PL_t + 11.7WM_t]$$

$$= -9.46 + .436PL_t + 62.9WM_t$$

To get some appreciation of the magnitudes, note that the wage rates are in terms of dollars and are in the region of one to two dollars. The price indices are in the region of 100. A graphical example of the results is presented below.

B. *Estimates with Various Time Intervals*

Among the more interesting aspects of the present work, at least from the standpoint of technique, are the results obtained using different time intervals. Which time interval to use has long been a matter of concern to model builders, one of the early discussions being Lundberg's treatment of "the question of the 'right' period" [6, p. 47]; but it seems now to be regarded as less a philosophical and more an operational question. In simulating models of the present type, J. W. Forrester, in his work at M.I.T., has obtained satisfactory results with time periods of different length, making an approximation that amounts to the same thing as saying that interest compounded semiannually at 2 per cent for half a year is approximately the same as interest compounded annually at 4 per cent a year.

Something similar was done in connection with the present estimation. In the econometric work on exponential delays, this question has never seemed to come up; but it did so in the present case and because the initial R^2 's were not as good as might be expected it seemed that the trouble might be that the delay, particularly for shoe manufacturing, was rather long, relative to the monthly intervals of the data. When longer data intervals were tried not only did the model give a better fit, but the results using different periods were amazingly consistent.⁶

The procedure used was to average the data over two- or three-month periods. These are not moving averages; the periods are nonoverlapping. One period is January-February, the next March-April, and so on. The time subscripts in the model then refer to these data intervals. For one-month intervals ΔP_t refers to the change in price from January to February; for two-month intervals it refers to the change from January-February to March-April, and so on for the other variables. For tanning we used an interval of two months, in addition to the initial study based on a monthly interval; and for shoe manufacturing results were obtained for two-month and for three-month intervals.

The consistency between the results using different time intervals was tested by going back to the implications of the formal model and seeing first whether the implied average delays were invariant. When the data intervals vary, the response parameters should vary. If, for example, an industry made a $\frac{1}{4}$ of the adjustment toward the normal price in one month, it should make a greater proportion of the adjustment when time is measured in units of two or three months. In Tables 1b and 2b, this is seen to be the case. But do the average delays, calculated in

⁶The suggestion came from Sidney Alexander, who had done work which showed that the change in wheat prices from week to week appears to be nonautocorrelated, but changes over longer periods do show persistence in trends.

the way specified above, remain invariant? By and large they do, particularly for shoe manufacturing; for tanning, the estimates are 1.07 and .86 months, and for manufacturing 4.38, 4.13, and 3.78 months.

A more striking illustration of the usefulness of the formal model in demonstrating consistency shows up when the coefficients of the other variables are compared. In the original regressions, the coefficients change quite a bit with different data intervals. For example, the successive coefficients for WM_t are 11.7, 21.1, and 28.4 (Table 2a) for one-, two-, and three-month intervals. However, they *should* change because by hypothesis these coefficients represent the product of a constant from the normal price equation and the response parameter. The important question is, do the normal price equations remain invariant; and we find that they do. These are shown in Table 1c and 2c. We find, for example, that the estimates of the coefficients of WM_t in the normal price equations are 62.9, 63.5, and 64.

TABLE 1—SUMMARY OF RESULTS FOR TANNING

a. Original Regressions				
Data Interval				
1 mo.	$\Delta PL_t = -7.01 + .340PH_t + 16.5WT_t - .483PL_{t-1}$ (3.42) (.034) (2.19) (.048)			$R^2 = .48$ $n = 119$
2 mo.	$\Delta PL_t = -12.94 + .503PH_t + 25.2WT_t - .701PL_t$ (5.33) (.039) (2.93) (.054)			$R^2 = .77$ $n = 59$

b. Estimates of Average Delay		
Data Interval	Parameter a	Average Delay, in Months
1 mo.	.483	$\left(\frac{1}{.483} - 1\right) = 1.07$
2 mo.	.701	$\left(\frac{1}{.701} - 1\right)2 = .86$

c. Estimates of Normal Price	
1 mo.	$PL_t^* = \frac{1}{.483} [-7.01 + .340PH_t + 16.5WT_t] = -14.5 + .70PH_t + 34.2WT_t$
2 mo.	$PL_t^* = \frac{1}{.701} [-12.94 + .503PH_t + 25.2WT_t] = -18.5 + .72PH_t + 36.0WT_t$

Not only are the results consistent, but with longer time intervals the R^2 's improve considerably, going from .4 or .5 for the one-month intervals to .7 or .8 for the longer intervals. There appears to be a bias

TABLE 2—SUMMARY OF RESULTS FOR SHOE MANUFACTURING

a. Original Regressions				
Data Interval				
1 mo.	$\Delta PS_t = -1.76 + .081PL_t + 11.7WM_t - .186PS_{t-1}$	$R^2 = .41$		
	(1.17) (.0093) (1.62) (.027)	$n = 119$		
2 mo.	$\Delta PS_t = -3.50 + .147PL_t + 21.1WM_t - .332PS_{t-1}$	$R^2 = .61$		
	(2.36) (.016) (2.86) (.043)	$n = 59$		
3 mo.	$\Delta PS_t = -5.17 + .198PL_t + 28.4WM_t - .443PS_{t-1}$	$R^2 = .75$		
	(3.52) (.021) (3.75) (.056)	$n = 39$		

b. Estimates of Average Delay		
Data Interval	Parameter a	Average Delay, in Months
1 mo.	.186	$\left(\frac{1}{.186} - 1\right) = 4.38$
2 mo.	.332	$\left(\frac{1}{.332} - 1\right)2 = 4.03$
3 mo.	.443	$\left(\frac{1}{.443} - 1\right)3 = 3.78$

c. Estimates of Normal Price	
Data Interval	
1 mo.	$PS_t^* = \frac{1}{.186} [-1.76 + .081PL_t + 11.7WM_t] = -9.46 + .436PL_t + 62.9WM_t$
2 mo.	$PS_t^* = \frac{1}{.332} [-3.50 + .147PL_t + 21.1WM_t] = -10.5 + .443PL_t + 63.5WM_t$
3 mo.	$PS_t^* = \frac{1}{.443} [-5.17 + .198PL_t + 28.4WM_t] = -11.7 + .447PL_t + 64WM_t$

in the results, in that as we extend the length of the interval, we underestimate the average delay so that the time intervals can not be increased indefinitely; but the general improvement in fit is so marked that one wonders what property of the data could account for it.

C. Autocorrelation of Residuals

The residuals turn out to be autocorrelated in general—except for the shoe manufacturing run using a three-month data interval. The autocorrelation may represent, in part, the effect of demand on profit margins; with strong demand, firms can get a higher markup over

direct costs or pass on cost increases faster. These demand effects are likely to persist from one month to the next, as shown in the simulation covering the Korean period presented below.

The Durbin-Watson measures of autocorrelation are shown in Table 3. If the measure is below the $d_L - d_U$ band of uncertainty there is certain autocorrelation (at the 95 per cent level); above the band there is no autocorrelation (at the 95 per cent level).

TABLE 3—AUTOCORRELATION MEASURES: SHOE MANUFACTURING AND TANNING
(Number of independent variables = $k' = 3$)

	Autocorrelation Measure (d)	Number of Observations (n)	Uncertainty Range (5 Per Cent Level)	
			d_L	d_U
	Shoe Manufacturing:			
1 mo.	1.19	119	1.61	1.74
2 mo.	1.26	59	1.48	1.69
3 mo.	1.83	39	1.33	1.66
	Tanning:			
1 mo.	1.07	119	1.61	1.74
2 mo.	.99	59	1.48	1.69

D. *Illustration of Model-Generated Prices*

An example of the results generated by the model is given in Figure 5. The solid line is the target that actual prices are assumed to be following, the normal shoe price. The dashed line shows the price that we would get with a distributed delay as given by the model—the way this was calculated will be described presently. The model-generated prices are seen to be fairly close to the actual prices, except when the price of leather and wages began to rise rapidly after the Korean outbreak; the price of shoes rose faster than the model would have predicted. This is probably the result of the sellers' market which then existed; it illustrates how the model may furnish a starting point for measuring the effect of demand on manufacturers' margins. Then, as leather prices fell and rose again in 1952, we see that the delay mechanism accounts very well for the fact that there was a negligible swing in shoe prices.

The data used for the graph are two-month averages. The method used for calculating the model-generated prices was a simulation, rather than an attempt to reproduce the data that went into the regression. The difference hinges on whether, in getting our model-generated prices of shoes, we use the actual lagged price (of shoes), as is common in most such graphs, or the lagged model-generated price, as used here.

The main stimulus to the model is the normal shoe price, calculated as a linear function of the price of leather and the wages of shoeworkers. To obtain a model-generated shoe price for the first period, January-February, 1950, we use in equation (2) the normal price for this period and the actual shoe price for November-December 1949. For the next model-generated price, we use the normal price for March-April and the model-generated shoe price for January-February. In terms of electrical mechanisms, the normal price is the independent input to the system and the actual November-December 1949 shoe price is the initial condition for shoe prices. This simulation method of generating shoe prices seems to show the behavior of the model more clearly than the standard method of presentation, particularly when the interest is in reproducing actual behavior rather than in prediction.

E. Test for Phase Differences

One of the interesting possibilities with behavior of this type is that there may be different responses up or down. Myron Gordon has conjectured that, broadly speaking, in a competitive effort to keep the good will of customers, firms would tend to pass on raw material cost increases slowly and decreases in costs rapidly. Similar phase differences have been proposed for investment or dividend behavior [5], and, in fact, Lintner argues that his dividend model shows a propensity to be more sluggish on the downswing. His model does not have the effect

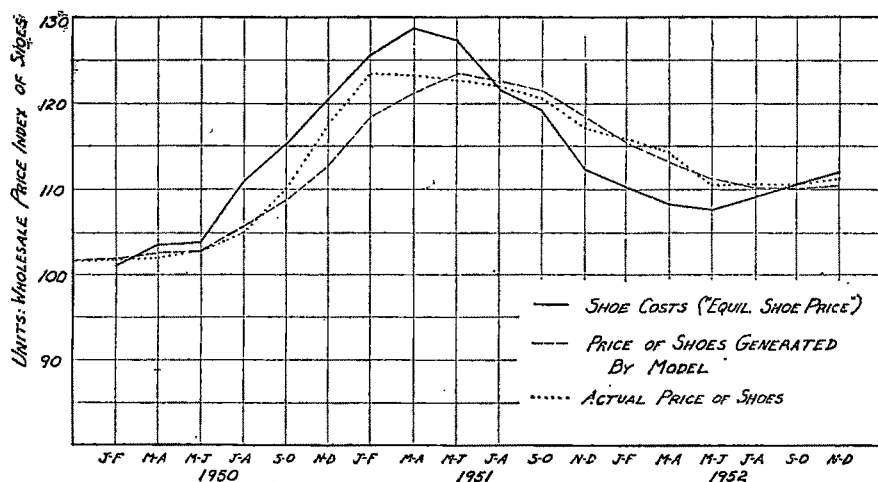


FIGURE 5. COMPARISON OF ACTUAL AND MODEL-GENERATED PRICE BEHAVIOR OF SHOE MANUFACTURERS

them, and conversely for shoe manufacturers. The response parameters are close to those estimated previously, and the R^2 's are about the same.

IV. Summary

We have explored the suggestion that many or most of the price changes that occur are due to changes in the direct labor and material costs. We have assumed that there is a delay between a change in costs and a change in prices, that this delay is not a fixed interval, but is distributed over time, and we have proposed a model in which this distribution over time is assumed to be of a specific form. The model is consistent with the view of many economists that it is primarily increases in costs that push up prices. Under such a view, we would expect, for example, that if steel wages go up, steel prices will go up.

But the model does more than restate the cost-push view because in its detailed mechanism, the delay in raising prices can account for another aspect of recent inflation trends, that prices have continued to rise even though demand has slackened and wages have stopped rising. If a sequence of industries has delayed price responses, it will take some time for an initial price rise to work its way through the system. It is a property of distributed delays of this type that if two or more are put in a sequence, the average delay of the final price in the chain in its response to the initial cost increase is the sum of the average delays of the individual industries. Thus with a pricing delay of one month in tanning and four months in shoe manufacturing, the retailer will face increases in prices with an average delay of five months. This, interestingly enough, is the order of magnitude given in the *New York Times* story.

APPENDIX

I. Derivation of Average Delay

Here we show the calculation for the average delay assuming a step increase in costs (normal price). The average delay is derived in terms of the delay between a change in costs and the resulting change in prices. The original model can easily be expressed in terms of changes in prices and costs; the original equation is:

$$P_t = P_{t-1} + a(P_t^* - P_{t-1}).$$

Taking a difference, we get:

$$\Delta P_t = \Delta P_{t-1} + a(\Delta P_t^* - \Delta P_{t-1}) = (1 - a)\Delta P_{t-1} + a\Delta P_t^*.$$

We can now calculate the price response. With a step increase in P_t^* in period 0,

$$\begin{aligned}\Delta P_t^* &= P_t^* - P_{t-1}^* = \bar{P} \quad \text{for } t = 0 \\ &= 0 \quad \text{for } t > 0.\end{aligned}$$

By the equation above, the price change is:

$$\Delta P_0 = a\bar{P},$$

$$\Delta P_1 = a(1 - a)\bar{P},$$

and so on, as shown in Table 5, column 3.

The delay between the initial cost rise and each segment of the price rise is shown in column 4. To calculate the average delay, we multiply, for each period, the price rise times the delay, sum the products over the period, and divide by the sum of the price rises (the sum of column 3):

$$\bar{D} = \frac{0a\bar{P} + a(1 - a)\bar{P} + 2a(1 - a)^2\bar{P} + \dots}{a\bar{P} + a(1 - a)\bar{P} + a(1 - a)^2\bar{P} + \dots}$$

TABLE 5—RESPONSE TO A STEP INCREASE IN COSTS

Period	(1) P_t^* (normal price)	(2) ΔP_t^*	(3) ΔP_t	(4) Delay between ΔP_0^* and ΔP_t
-1	0	0	0	
0	$\frac{0}{\bar{P}}$	$\frac{0}{\bar{P}}$	$a\bar{P}$	0
1	$\frac{\bar{P}}{\bar{P}}$	0	$a(1 - a)\bar{P}$	1
2	$\frac{\bar{P}}{\bar{P}}$	0	$a(1 - a)^2\bar{P}$	2
⋮				
⋮				
t	\bar{P}	0	$a(1 - a)^t\bar{P}$	t
Sum		$\frac{0}{\bar{P}}$	$\frac{a\bar{P}}{\bar{P}}$	

To evaluate this, we first show that the denominator adds up to \bar{P} ; it is the property of the model that if the normal price increases, the actual price will eventually reach the normal price. The denominator

$$= a\bar{P}[1 + (1 - a) + (1 - a)^2 + \dots] = \frac{a\bar{P}}{1 - (1 - a)} = \bar{P}$$

Hence the expression for \bar{D} reduces to:

$$\bar{D} = a(1 - a)[1 + 2(1 - a) + 3(1 - a)^2 + \dots].$$

Letting $r = (1 - a)$, we see that the term inside the bracket is the derivative of a geometric series, $1/(1 - r)$. I.e.,

$$\frac{d}{dr} \left[\frac{1}{(1 - r)} \right] = \frac{d}{dr} [1 + r + r^2 + \dots] = 1 + 2r + 3r^2 + \dots$$

and

$$\frac{d}{dr} \left[\frac{1}{(1 - r)} \right] = \frac{1}{(1 - r)^2}.$$

Finally, therefore,

$$\bar{D} = a(1-a) \left[\frac{1}{[1-(1-a)]^2} \right] = \frac{1-a}{a} = \frac{1}{a} - 1$$

which was to be proven.

It might be mentioned that we get this average delay because we assume that a cost increase has an effect on prices during the first period of the cost increase. If we had stated the model in such a way that the first effect on prices occurred the period after the cost increase, the whole price response would be shifted down one period and the average delay would be $1/a$, that is, with the model

$$P_t = P_{t-1} + a(P_{t-1}^* - P_{t-1}).$$

Another point is that in some cases we use data intervals of two or three months. The same mathematical argument holds as above, except that for the case of two-month intervals, the first response comes with a delay of zero, the second with a delay of one period, or two months, etc. Hence, to convert the above expression in terms of intervals into terms of months, we multiply by the number of months per interval. Let

Δt = length of data interval, in months per interval. Then:

$$\bar{D} = \left(\frac{1}{a} - 1 \right) \Delta t \text{ months}$$

II. *Superposition*

In the text it was pointed out that the derivation of the average delay using the case of a step increase in the normal price was not in any way a limitation on the result. This can be shown by expanding the equation above:

$$\Delta P_t = \Delta P_{t-1} + a(\Delta P_t^* - \Delta P_{t-1})$$

to get, for the general case, the change in price in terms of previous cost changes:

$$\Delta P_t = a\Delta P_t^* + a(1-a)\Delta P_{t-1}^* + a(1-a)^2\Delta P_{t-2}^* + \dots$$

Any change in cost will have the same effect in this equation as it had in the special case of the step increase in costs. In the first period a change in costs of \bar{P} will add the term $a\bar{P}$ to the change in prices; in the next period it will add a term $a(1-a)\bar{P}$ to the change in prices, and so on. So even though all the past history of cost changes is relevant to the price changes in a given month, we can, in the model, isolate the effect of a given cost change on later price changes, and the average delay in the effect of these components will be the same as that which we have calculated above.

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THE ALLOCATION OF JOINT COSTS WITH DEMANDS AS PROBABILITY DISTRIBUTIONS

By A. A. WALTERS*

The development of the theory of the single-product firm has been very rapid over the past thirty years. But the elaboration of this theory to include multiple-product firms has made little progress since the famous Pigou-Taussig controversy in 1912-1913 [3]. Since most of the firms dealt with by the applied economist produce a number of commodities or services, he has much difficulty in applying the theory to the data (see, for example [1, p. 339]). One particularly acute difficulty arises from the allocation of joint costs. Theory suggests that we calculate the marginal cost of each product separately by holding the quantity of others constant while we increase the production of one product by a single unit. To find the most profitable mix of products we need to equate marginal cost and marginal revenue for each product.¹ There is no "allocation of joint costs" involved directly in this procedure; it has all been taken into account in calculating marginal cost.

To estimate costs we need to know how total costs change as outputs change. Now it is extremely rare that the cost investigator finds enough observations of outputs and total cost to enable him to estimate the relationship between total cost and the various levels of output. Indeed the number of parameters to be estimated for a two-product firm is usually at least twice as many as that for a single-product firm. To get good estimates of the parameters there must be a large number of firms each producing a different proportion of products. But in fact it is unlikely that there will be much variation in the output mix of a cross-section of firms. If each firm has similar technology, the same prices of output, labor and raw materials, there will be little difference in the product proportions.

These difficulties have led practical economists to try to estimate marginal cost more directly. Instead of trying to trace variations in total cost, they have attempted to determine directly the increase in costs that would occur if output of one commodity were expanded by one unit and production of other commodities remained constant. The accounts of a particular firm or of a number of firms are analyzed to find

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• ¹ Some of the difficulties in this procedure are discussed by P. J. D. Wiles [6, p. 98 et seq.].

what items of expenditure would be increased if output of one commodity was increased.² One may find in the accounts, or construct from technological relationships, those subcategories of costs which are connected with the commodity (or service) in question. These costs cause little difficulty. There are, however, always other items of cost which are joint and connected with the output of more than one product. Thus the costs of a round trip of a truck are joint costs incurred to provide both outgoing and incoming freight services. The degree of jointness of the costs varies from the case in which there are rigidly fixed proportions (which more or less corresponds to the trucking case) to that in which the proportions of the product are highly variable (as in oil refining).

Now the allocation of joint costs between the various products is obviously the main problem in this sort of analysis. Cost accountants customarily divide up joint costs according to which outputs "caused" the joint costs. If there are rigid proportions, the economist proclaims that this allocation is irrational. Many examples of this appear in the transportation industries. The truck completes a round trip from A to B and back again to A. Ignoring the cost of loading and unloading the vehicle, this approximates to a case of rigid proportions (x tons capacity out and x tons back); and it is urged that the costs of the round trip cannot be allocated, and indeed need not be allocated, to find profit-maximizing outputs. Marginal cost varies erratically; it is either the cost of the round trip or zero. The economist is correct when the quantity demanded can be controlled *with certainty*. Allocations of joint costs are irrational. But where the quantity demanded cannot be controlled with certainty, when it is subject to chance fluctuations, we must define a new operational concept—marginal expected costs. This value takes into account these chance variations.

We shall argue that, even in the case of rigid proportions, marginal expected costs do not vary erratically as output varies, and that we get smooth marginal expected cost series. The conditions under which this is true are: (1) where demands are in the form of a joint probability distribution for a given price; (2) the product is impossibly expensive to store; (3) the price is fixed at the beginning of the marketing period and is held throughout the whole period; (4) all demand is satisfied; and (5) excess output can be disposed of without cost. Under these conditions,³ it is rational to "allocate" joint costs for the purpose of profit maximization. Hence the allocations of accountants and traffic

² This is the method of "costing" used by the Interstate Commerce Commission. For a more recent and more sophisticated version of the method, see [2].

³ These assumptions were used in Walters [4] [5].

costing officers may often have a rational economic basis. Indeed it is shown that the figures presented by cost accountants will usually be better than those of the economist.

I. *Expected Demand and Expected Cost*

Before we attempt to deal with the multiproduct firm, it will be useful to set out the theory of the *single-product* firm when demand is subject to random variations. We can then elaborate the method easily to include the multiproduct firm. The single-product firm considered here fixes its price at the beginning of the marketing period and finds it impossibly expensive or very detrimental to goodwill to change price during that period. These conditions correspond to those of the transport industries. Stories of "Dutch auctions" and "horse bargaining" are occasionally told, but they seem to be exceptional. Given the price, the quantity demanded is not a *certain* figure, but varies from day to day. The businessman does not know the quantity which will be demanded on any specific day; but we do assume that he knows the *probability* of any quantity being demanded.

There is no single demand curve in this situation since, for any given price, the quantity demanded is a probability distribution. With a given price, the businessman can attach a probability or likelihood to the various quantities of output he may sell during the marketing period. However, we can conceive of a simple stable relationship between the price and the *expected* quantity demanded. As price is reduced the expected demand is increased. To simplify, we shall suppose that the probability distribution of output does not change as price changes except that the mean of the distribution moves in the opposite direction to price. Thus if the output is denoted by X , the probability distribution for a given price p_i is denoted by $f(X|p_i)$. We denote the expected quantity of output, the sum of outputs weighted by the probabilities, for a given price by $\bar{X}(p)$. Thus: $E\{Xf(X|p)\} = \bar{X}(p)$. We have in fact assumed that $f\{X - \bar{X}(p_i)|P_i\} = f\{X - \bar{X}(p_j)|P_j\}$ for any i and j . This means that, in particular, the dispersion remains constant as mean output increases. For the transport industries this is certainly unrealistic for large increments of traffic. But this assumption does simplify the analysis and so it will be retained throughout most of this paper.

The assumption that the output of the firm is impossibly expensive to store is not really crucial for the following results. It is retained in order to avoid going into problems of inventory costs, etc. On the other hand, the assumption that all demand must be satisfied at the going price is rather more important. The firm must somehow produce during the market clearing period the quantity demanded—however costly such

production may be. There are many possible variations of this assumption; for example, if the firm were unable to produce the full amount, the firm may buy goods or services from other firms to sell to its own customers. Circumstances such as these could easily be incorporated. The assumption that excess output can be disposed of without cost is only absolutely necessary for the rigid proportions case.

The probability distribution of the demands for the outputs of a *two-product* firm is given by: $f\{X_1, X_2 | \bar{X}_1(p), \bar{X}_2(p')\}$. The subscripts denote the two products and p and p' are the respective prices. In other words, we specify that the entrepreneur fixes the two prices— p for the price of product 1 and p' for the price of product 2. The *expected* or mean outputs demanded $\bar{X}_1(p)$ and $\bar{X}_2(p')$ are given once the prices are specified. The actual quantities demanded X_1 and X_2 are grouped around these expected outputs and the probability of X_1 of product 1 and X_2 of product 2 being required is denoted by $f\{X_1, X_2 | \bar{X}_1(p), \bar{X}_2(p')\}$ —the joint probability distribution. Again we assume that the probability distribution does not change as prices vary, except that the mean outputs change thus:

$$f\{X_1 - \bar{X}_1(p_i), X_2 - \bar{X}_2(p'_i) | p_i, p'_i\} \\ = f\{X_1 - \bar{X}_1(p_j), X_2 - \bar{X}_2(p'_j) | p_j, p'_j\}$$

for any i, j . That is to say the probability of outputs deviating from expected outputs by given amounts does not change as mean outputs change in response to variations in prices. There is no need to assume that the demands are not related, i.e., that an increase in X_1 does not change the demand schedule for X_2 , although the process of profit maximization is much simpler when this assumption is made.

The relationship between the market clearing period, i.e., the period in which demand must be satisfied, and the conventional short period should be specified. In the freight transport industries the market clearing period is perhaps less, but not very much less, than the short period. If the market clearing period were less than the short period, we should have to develop market-clearing-period cost curves. This would require a complicated and tedious description of the relation between the market-clearing-period cost curve and the ordinary short-run curve. To avoid all this we shall assume in the following analysis that the market clearing period is *equal* to the short period. This simplifies the development.

We want to find the prices which will maximize the profits of the firm. In the ordinary deterministic theory of the firm output is produced such that marginal revenue is equal to marginal cost for each output. With a probabilistic demand the concept of marginal cost is replaced by marginal *expected* costs. This is defined for output X , as the difference

between expected costs with expected output \bar{X}_1+1 units and expected costs with expected output \bar{X}_1 units when, in both situations, the quantity \bar{X}_2 is held constant.

In order to develop this more formally, we must calculate expected costs. If $C(X_1, X_2)$ is the cost function, we define expected costs as:

$$E\{C(X_1, X_2) \mid \bar{X}_1, \bar{X}_2\} = \int_0^\infty \int_0^\infty f\{X_1, X_2 \mid \bar{X}_1, \bar{X}_2\} C(X_1, X_2) dX_1 dX_2.$$

This expectation is for *given* expected outputs of \bar{X}_1, \bar{X}_2 ; that is to say, for given prices p and p' . Now consider the consequences of a small reduction in the price of the first commodity so that the expected output increases from \bar{X}_1 to \bar{X}_1+h . The difference between the expected costs (divided by h) when expected output is \bar{X}_1+h and when it is \bar{X}_1 , is defined as the marginal expected cost of X_1 , or MEC_{X_1} . This is the relevant short-run cost which the entrepreneur takes into account when he considers reducing or increasing the price. Now it follows that:

$$\begin{aligned} MEC_{X_1} &= \int_0^\infty \int_0^\infty C(X_1, X_2) \\ &\quad \cdot \left\{ \frac{f(X_1, X_2 \mid \bar{X}_1+h, \bar{X}_2) - f(X_1, X_2 \mid \bar{X}_1, \bar{X}_2)}{h} \right\} dX_1 dX_2 \\ (1) \quad MEC_{X_1} &= \int_0^\infty \int_0^\infty C(X_1, X_2) \frac{\partial f}{\partial \bar{X}_1} dX_1 dX_2. \end{aligned}$$

In other words, the cost corresponding to a given output combination is weighted by the changes in the probability of that combination as mean output increases; the products are then summed over all output mixtures to give MEC_{X_1} .

II. A Simple Joint-Cost Case

The simplest version of the joint-cost case is where the two outputs are provided by one process of production resulting in fixed proportions. The trucking industry corresponds quite closely to this model. The cost of a round trip of a 14-ton capacity vehicle results in 14 tons capacity from A to B and the same on the return trip from B to A . The round-trip costs depend then on the larger of the two tonnages carried. The simplest cost function for this joint cost, assuming perfect divisibility, is then:

$$\begin{aligned} (2) \quad C &= \alpha X_1; & X_1 > X_2. \\ C &= \beta X_2; & X_2 > X_1. \end{aligned}$$

This joint-cost function could then be embedded in the total-cost function.

We can demonstrate some results graphically and get some insight into the allocation of joint costs by proceeding rather less formally. Let us represent the outputs X_1 and X_2 on the axes of the following diagram and draw the constant cost curves for each combination of X_1 and X_2 . Let us suppose that $\alpha = \beta$, i.e., the increase in cost due to a unit increase in X_1 when $X_1 > X_2$ is exactly the same as the increment in cost for a unit increase in X_2 when $X_2 > X_1$. The constant cost curves are then represented by bBb' , $d'Dd'$, etc., where $Ob = Ob'$, $Od = Od'$, etc., and the total cost is simply proportional to the ordinates Ob , Od , etc.

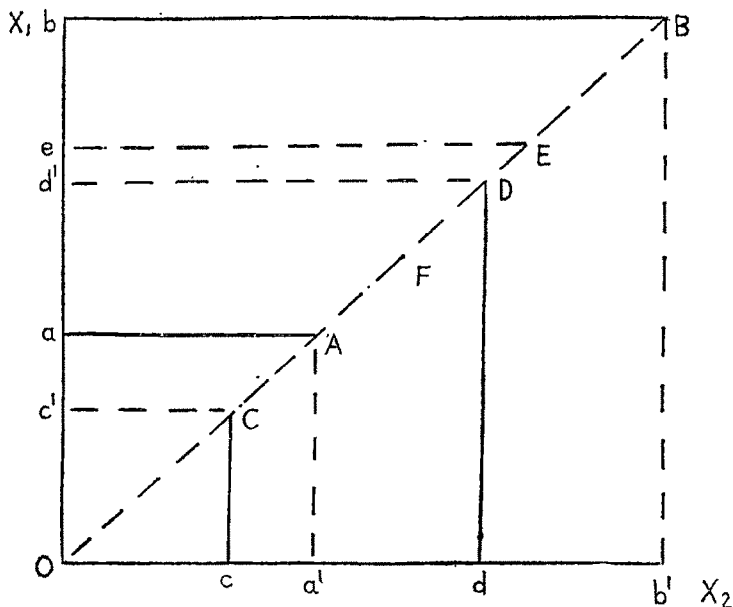


FIGURE 1

Now suppose that the demand for X_1 takes *only* two values, a and b , and that the demand for X_2 assumes only the two values c and d , given the prices of the two services. Further suppose that combinations of these demand quantities are equiprobable; thus the probability of (a of X_1 and c of X_2) is equal to $\frac{1}{4}$ which is the same as the probability of (b of X_1 and c of X_2), i.e., $P(a, c) = P(a, d) = P(b, c) = P(b, d) = \frac{1}{4}$. The output which must be produced for these demand combinations is simply the larger of the two values. Thus in the case of a of X_1 and c of X_2 , the production point is represented by A when Oa of X_1 is sold, Oc of X_2 is sold and ca' is thrown away. The point C cannot be a production point since the quantity of X_1 required is at least Oa . The point B is a production point both when the combination (b , d) and (b , c) are

required, so the probability of having to produce at B is $\frac{1}{4} + \frac{1}{4} = \frac{1}{2}$. The probability of having to produce at points A and D is of course $\frac{1}{4}$ each. To find expected costs we need to weight the total cost at production points A , D and B by the proportions $\frac{1}{4}$, $\frac{1}{4}$, and $\frac{1}{2}$ respectively. This can be done geometrically by bisecting the line between A and D at say F , then bisecting the line between F and B giving the point E . The value e on the X_1 (or X_2) axis then gives expected costs by multiplying by the cost per unit (i.e. α).

Now consider a small increment (h) in both a and b while c and d remain constant. The effect on expected costs is clearly to increase it by:

$$\alpha h \left(\frac{1}{4} + \frac{1}{2} \right) = \frac{3}{4} \alpha h.$$

Marginal expected costs are then equal to $\frac{3}{4}\alpha$.⁴ If $a < c$ such that the order of the points on the 45° line is $ACDB$, an increase of h in a and b would affect expected costs only through the point B . In fact if $a < c$, production will never take place at point A . Clearly marginal expected costs are then $\frac{1}{2}$. Two extreme cases can also be considered. First suppose that $a > d$, that is to say, A and B are moved up the 45° line until A is above D . Then clearly the marginal expected cost of X_1 is α . Secondly, suppose that the $c > b$; that is to say, the order of points on the diagonal from left to right is $ABCD$. In this case, marginal expected cost of X_1 is zero. With this simplest of all joint probability distributions, there are therefore four possible marginal expected costs of X :⁵

MEC		Order of Points on 45° Line
$= 0$	if $c > b$	$ABCD$
$= \frac{1}{2}\alpha$	if $a < c$	$ACDB$
$= \frac{3}{4}\alpha$	if $a > c$	$CADB$
$= \alpha$	if $a > d$	$CDAB$

This is, of course, a considerable generalization of the case with certain outputs where there are only two marginal costs, zero and α .

Instead of supposing that the demand for X_1 consists of *either* a or b , we can generalize the results by assuming that X_1 takes values continuously *between* a and b . Similarly, we assume that X_2 takes values continuously between c and d . The simplest continuous distribution is

⁴ Certain changes in the dispersion of the joint probability distribution do not affect the value of marginal expected costs. If, for example, c were increased but were still less than a , we should get the same figure.

⁵ There are $4!$ possible unrestricted orderings of points $ABCD$ along the diagonal and 6 orderings where $A < B$ and $C < D$. But three of these are isomorphisms of the other three, just as the last value in the table is an isomorphism of the first.

that in which the probability of each output combination over specified ranges of X_1 and X_2 is constant. If the range of X_1 is a to b , and the range of X_2 is c to d , then the marginal expected cost is given by:⁶

$$(3) \quad MEC_{X_1} = \frac{\alpha}{2(d-c)} [a + b - 2c]$$

provided that $b-a < d-c$ and $a > c$.

Secondly, when $a < c$ and $b < d$, we get:

$$(4) \quad MEC_{X_1} = \frac{(b-c)^2}{2(b-a)(d-c)}.$$

Thirdly, when $a < c$ and $b < d$, the formula is:

$$(5) \quad MEC_{X_1} = \frac{\alpha}{(b-a)(d-c)} \left[b(d+c) + ac - \frac{a^2 + d^2}{2} \right].$$

Let us examine first the special case where the level of back-haul traffic is much less predictable than the amount of outgoing traffic (X_1). To carry it to an extreme, let us suppose that $a=b$, i.e., the outgoing traffic is one certain volume. Clearly equation (3) is then applicable and:

$$(6) \quad MEC_{X_1} = \frac{\alpha}{2(d-c)} 2(a-c) = \alpha \frac{(a-c)}{(d-c)}.$$

The marginal expected cost curve corresponding to this formula is shown in Figure 2. MEC_{X_1} is zero up to $\bar{X}_1=c$, then it rises with slope $\alpha/d-c$ to α at $\bar{X}_1=d$.

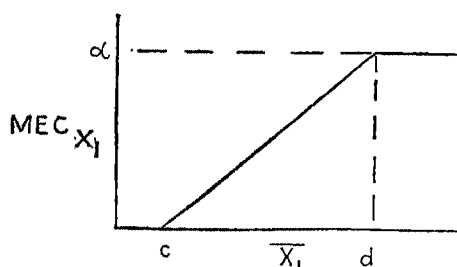


FIGURE 2

The next case we examine is where the mean of X_1 is equal to the mean of X_2 and $a=c$, $b=d$. All three of the above formulae (3), (4) and (5) give:

$$(7) \quad MEC_{X_1} = \frac{\alpha}{2}.$$

⁶ See appendix for proof.

This says that the marginal expected cost of X_1 is equal to one half the joint costs. Now this is just the allocation which would be suggested by a cost accountant in these circumstances. Most economists would object to this allocation as being both irrational and undesirable because they would have in mind *certain* outputs, not probability distributions. And with this cost function, marginal cost with certainty (MCC) is either zero or α , according to whether $X_1 < X_2$ or $X_2 < X_1$. The allocation rule of the accountant is much more relevant to the decisions made by the firm.

Unfortunately, it is only a coincidence that the accountant's procedure coincides exactly with the optimum economic allocation. Many varieties of apportionment conventions are employed by accountants. Probably the most common approach would be to allocate joint costs in proportion to the expected traffic flows. Thus the accountant would allocate

$$\frac{\bar{X}_1}{\bar{X}_1 + \bar{X}_2}$$

of the incremental cost to output 1 and the remainder to output 2. The average burden of joint cost per unit of output would be the same. If the accountant employed this rule, he would get the same results as equation (3) only when the mean outputs and the limits were the same.

If we write the ratio of the means as λ , i.e.

$$\lambda = \frac{\bar{X}_2}{\bar{X}_1} = \frac{c + d}{a + b}$$

we can show that the accounting rule will underestimate the joint-cost element in marginal expected cost when $\lambda > 1$ and when $a > c$ (i.e., the lower limit of X_2 is less than the lower limit of X_1); when $\lambda < 1$ and $a < c$ and $b < d$, then the accountant will overestimate the joint-cost allocation. The accountant's figures will in any case lie between the MEC_{X_1} and the marginal cost with certainty (MCC) of \bar{X}_1 (with \bar{X}_2 as the certain output of commodity 2).⁷ This curious result implies that the calculations of the cost accountants are nearer to the relevant figure than those of a naive economist who regards average outputs as a quantity to be produced each period and who ignores the random variations.

These results only apply to the special case of the probability distributions considered above. This is perhaps the most simple probability distribution one may postulate. Other more realistic distributions of the humped type would give somewhat different results. However, the conclusions do not differ in any fundamental way from those which we have

⁷ See appendix for proof.

found for the joint rectangular distribution. MEC_{X_1} will still fall between the two extreme values zero and α or zero and β . The biases involved in the cost accountant's allocation rules will, however, depend on the parameters of the probability distribution and, because we have to deal with truncated distributions, this is normally a complicated function. The marginal enlightenment is hardly worth the additional algebra involved. On the other hand, it is relatively easy to develop the cost function a little more realistically, such as:

$$C = \alpha_1 X_1 + \alpha_2 X_2; \quad X_1 > X_2$$

and

$$C = \beta_2 X_2 + \beta_1 X_1; \quad X_1 < X_2$$

where α_1 and β_2 are large compared with α_2 and β_1 . This would involve a little more elaboration of the above formula but the results would be basically similar.

A study of the cost of traffic sent by British Road Services vehicles originally stimulated the development of this analysis. The particular vehicle trips between Birmingham and Wales and the southwest of England were examined in some detail to find the technical make-up of each of the services to the different destinations. The method used was to measure the vehicle time spent, the input of loading dock labor, etc., on each of the services. The inputs were then valued to find the total variable cost of the round trip, out from Birmingham to the southwest and back again from the southwest to Birmingham.

The round-trip vehicle cost from Birmingham to Bristol was approximately 23 shillings per ton. There were only fragmentary data on the probability distribution of the traffic, and those data were not inconsistent with the rectangular distribution (i.e., equiprobable combinations of outgoing and incoming tonnages). The following were the estimates of the parameters of the distribution:

$$a = 1430 \quad b = 2010$$

$$c = 1350 \quad d = 1650$$

Then, using formula (5):

$$MEC = 23 \left[\frac{b(d - c) + ac - \frac{d^2 + a^2}{2}}{(d - c)(b - a)} \right]$$

$$= 18.4 \text{ shillings approximately.}$$

This compares with a cost accounting allocation of about 12.5 shillings; so there is a considerable difference.⁸

⁸ This cost study was included in a report submitted to the Ministry of Transport. The research was financed by Counterpart Funds (Conditional Aid).

III. *Concluding Remarks*

This paper has argued that in many industries where joint costs are important, the demand is not certain but has the form of a probability distribution. In the case of fixed proportions, the allocation of joint cost is both rational and necessary in order to find profit-maximizing price and output. With rectangular probability distributions, a rule, used quite commonly by cost accountants, estimates marginal expected cost *exactly* when the two outputs have the same expected value. But in general, the accountants' rule will produce biased results—overestimating when the mean of X_1 is lower than the mean of X_2 , and underestimating when $\bar{X}_2 < \bar{X}_1$. The application of the simple model to the cost study of British Road Services vehicles gave results which were quite different from those of the accounting allocation. The example shows how the economist may help accountants to improve their allocations, rather than condemn them as irrational.

APPENDIX

1. To calculate *MEC* from first principles when:

$$C = \alpha X_1 \quad X_1 > X_2$$

$$C = \alpha X_2 \quad X_2 > X_1$$

and

$$f(X_1, X_2) = \frac{1}{(b-a)(d-c)} = \frac{1}{k} \quad \text{where } 0 < a < X_1 < b,$$

and

$$0 < c < X_2 < d.$$

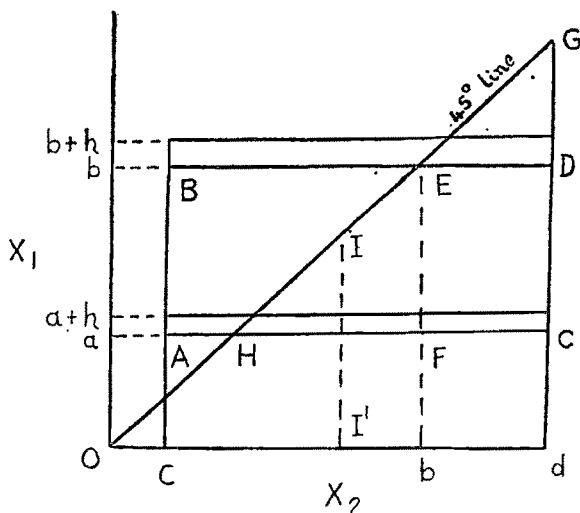


FIGURE 3

In Figure 3, we show the distribution of X_1 and X_2 for the case where $b-a < d-c$ and $a > c$. Each point on the rectangle bounded by a , b , c , and d is equiprobable.

We wish to find the increase in expected costs as a increases to $a+h$ and b to $b+h$. Expected costs are reduced by the rectangle represented by a to $a+h$ over the same range of X_2 . The increment in expected costs can be split into two parts by the diagonal at E as shown on the diagram. Similarly, the decrease in expected costs can be split in the same way at H .

The net increase in expected costs due to the increase in the limits BE from b to $b+h$ and AH from a to $a+h$ is:

$$\frac{\alpha h}{k} [b(b-c) - a(a-c)].$$

The increase in ED from b to $b+h$ increases expected costs by

$$\frac{\alpha h}{k} (d-b) \left(b + \frac{d-b}{2} \right)$$

and the decrease caused by HC increasing to $a+h$ is given by

$$\frac{\alpha h}{k} (d-a) \left(a + \frac{d-a}{2} \right).$$

The net result of these last two elements can be represented diagrammatically as the decrease in expected costs:

$$\begin{aligned} \frac{\alpha h}{k} [HF \times II'] &= \frac{\alpha h}{k} \left[(b-a) \left(a + \frac{b-a}{2} \right) \right] \\ &= \frac{\alpha h}{2k} (b-a)(b+a). \end{aligned}$$

The total effect is then:

Change in expected costs

$$\begin{aligned} &\simeq \frac{\alpha h}{k} \left\{ [b(b-c) - a(a-c)] - \frac{(b-a)(b+a)}{2} \right\} \\ &\simeq \frac{\alpha h}{k} \left\{ \frac{b^2 - a^2}{2} - c(b-a) \right\} \\ &\simeq \frac{\alpha h}{k} \frac{(b-a)}{2} [b+a-2c] \\ &\simeq \frac{\alpha h}{2(d-c)} (b+a-2c). \end{aligned}$$

And as

$h \rightarrow 0$

$$(i) \quad MEC_{X_1} = \frac{\alpha}{2(d-c)} [a + b - 2c].$$

Next consider the case when $a \leq c$ and $b \leq d$. By an argument similar to that above we get:

$$(ii) \quad \begin{aligned} MEC_{X_1} &= \frac{\alpha}{k} \left[b(b-c) + (d-b) \left(b + \frac{d-b}{2} \right) \right. \\ &\quad \left. - (d-c) \left(c + \frac{d-c}{2} \right) \right] \\ &= \frac{\alpha(b-c)^2}{2(b-a)(d-c)}. \end{aligned}$$

Next consider the case where $a \geq c$ and $b \geq d$, then:

$$(iii) \quad \begin{aligned} MEC_{X_1} &= \frac{\alpha}{k} \left[b(d-c) - a(a-c) - (d-a) \left(a + \frac{d-a}{2} \right) \right] \\ &= \frac{\alpha}{(b-a)(d-c)} \left[b(d-c) + ac - \frac{a^2 + d^2}{2} \right]. \end{aligned}$$

In the special case where $a=c$ and $b=d$, all the formulae (i), (ii), (iii) reduce to:

$$MEC_{X_1} = \frac{\alpha}{2}.$$

2. An accounting allocation according to average tonnage is:

$$\alpha \left[\frac{\bar{X}_1}{\bar{X}_1 + \bar{X}_2} \right] = \frac{\alpha}{1 + \lambda} \quad \text{where} \quad \lambda = \frac{c + d}{a + b}.$$

The bias (B) in the accounting allocation for formula (i) is given by:

$$\begin{aligned} B &= \alpha \left[\frac{1}{1 + \lambda} - \frac{1}{2(d-c)} (a + b - 2c) \right] \\ &= \alpha \left[\frac{1}{1 + \lambda} - \frac{1}{2\lambda(d-c)} \{c(1 - 2\lambda) + d\} \right] \\ &= \alpha \left[\frac{2\lambda(d-c) - (1 - \lambda)\{c(1 - 2\lambda) + d\}}{2\lambda(1 + \lambda)(d-c)} \right]. \end{aligned}$$

Since the denominator is always positive, the bias is zero when:

$$\begin{aligned} \text{i.e.,} \quad &2\lambda(d-c) - (1 + \lambda)\{c(1 - 2\lambda) + d\} = 0 \\ &2c\lambda^2 + (d-c)\lambda - (c+d) = 0 \end{aligned}$$

so that

$$\lambda = \frac{c - d \pm \sqrt{(d - c)^2 + 8c(c + d)}}{4c}.$$

But since $c - d$ is always negative, only the positive value under the square-root sign is of interest.

$$\begin{aligned}\lambda &= \frac{c - d \pm \sqrt{d^2 + 9c^2 + 6cd}}{4c} \\ &= \frac{c - d + (d + 3c)}{4c} \\ \underline{\underline{\lambda}} &= \underline{\underline{1}}.\end{aligned}$$

This means that only when $a=c$ and $d=b$ is the accounting allocation unbiased. This is the case where $MEC_x = \alpha/2$. Otherwise, the bias is negative.

Similar formulae can be worked out corresponding to (ii) and (iii) above, but they are rather more complicated.

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COMMUNICATIONS

Availability of Foreign Language Books in Economics

Few members of the American Economic Association apparently know that they can now easily borrow most recent foreign language books in economics. The Farmington Plan, a cooperative agreement established over ten years ago, made arrangements for American libraries to divide among themselves the obligation to import "at least one copy of each new foreign book and pamphlet that might reasonably be expected to be of interest to a research worker in the United States" [1, p. 3]. The books on economic theory procured under the Farmington Plan are in the library of the University of Chicago and those in all other branches of economics are mainly in the library of New York University. They can be readily obtained through interlibrary loans. The following classes of material are not among those obtainable: periodicals, numbered serials, dissertations, government documents, and the publications of Russia and her satellites or of Japan.

Before the inauguration of the Farmington Plan we depended on the somewhat hit-and-miss procedures of our research libraries for acquisitions of this kind. The systematic purchases in the overseas markets undertaken by the Farmington agents during the last decade have probably doubled the number of different books that otherwise would have been bought. The plan provides a further service by requiring that everything received be quickly recorded in the National Union Catalog at the Library of Congress, so that anybody can determine from an inquiry addressed to that library the correct location of a desired book. The plan obligates the institution having the books to make them available.

The founders of the Farmington Plan hoped that it would help American students to enlarge their acquaintance of the literature of other countries. Unfortunately no increased internationalization appears to have taken place in the area of economics. The writings of contemporary foreign economists have arrived in abundance, they are cataloged and ready to be studied; yet they stand on the shelves unread and unexamined. Sufficient direct testimony to their disuse comes from the librarians who look after the collections. An issue-by-issue inspection of the footnotes in the *American Economic Review* since 1948 will confirm their reports. Except for studies of the economies of particular nations (Russia principally) or those by economists from abroad, the number of articles that contain non-English references is small. To those who might think that foreign literature can be disregarded with some safety it may be pointed out that book reviewers praise the new foreign books generously. For instance, the September 1959 issue of the *American Economic Review* contains sixteen reviews of books in foreign languages (more than one-quarter of the total) which almost always commend the volumes and often describe them as "contributions," but has no article with so much as a single foreign-language allusion.

Even a wide knowledge of the greater accessibility of books and pamphlets provided by the Farmington Plan will not, by itself, broaden spectacularly our familiarity with foreign economics. For the linguistic barricade has prevented communication and it is not visibly falling in response to the many voices (and large amounts of federal money) which, in these days, call attention to the advisability of considering ideas originating outside our own language boundaries. Whatever aid the Farmington Plan books can give, however, is so easily within reach and so inexpensive in time and energy to utilize that it is a pity that it is overlooked.

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1. E. E. WILLIAMS, *Farmington Plan Handbook*. Bloomington, Indiana, 1953.

A Regional Countermeasure to National Wage Standardization

Economists have, almost without exception, discussed policy questions from the point of view of national governments. Political reality indicates that even within a single government's over-all policy program internally conflicting and offsetting measures are often encountered. Even more likely is it that, when two or more independently organized and overlapping governmental units enact economic policy measures simultaneously over the domain of the same economy, the separate governmental units may have sharply conflicting policy objectives as well as different constitutionally determined powers of action. In political federalism, conflicts between the national government and the state or regional government units seem certain to occur. In such a political setting, the economist's tools may be quite helpful in suggesting ways and means through which a separate governmental unit may take action to offset or negate the effects of policy measures taken by an overlapping jurisdiction. The possibility that action taken by the separate states may serve to offset national government policy which is contrary to the interest of the national economy, has not been adequately considered. This paper attempts to examine a particular case of this sort.

We assume a closed economy extending over a wide geographical area. The economic order is competitive, and the legal structure includes institutions which effectively enforce competitive norms except where specific exemption is granted. The national government also enforces the employment of a common currency unit throughout the economy. Suppose now that the national government imposes a legal minimum wage, higher than the lowest wage that would otherwise prevail. This wage is standardized for the whole economy and its coverage extends over a significant number of the employed workers, but not over all workers. No account is taken of differing labor-market conditions in the separate regions. We also assume specifically that there is a national policy of exempting labor organizations from the legal sanctions against restraint of trade. Labor organizations are encouraged to bargain collectively to obtain favorable wage contracts. These contracts are assumed to include standardized •

wage rates for similar jobs in different geographic regions of the economy.

Within this single national economy there are a number of regional governmental units possessing residual political powers. These states are prohibited by the Constitution from interfering directly with the flow of resources, goods, and services across state boundary lines. The states retain financial independence. They are empowered to tax individuals directly and indirectly to secure revenue for the performance of independent governmental functions or to provide transfers.

The states are classified into two groups the difference between which is defined in terms of the ratios of the productive factors. The origins of these differences in factor ratios need not concern us; they may be due to historical development, a different natural resource base, or to any other reason. The first group contains large amounts of unskilled and semiskilled labor relative to capital and skilled labor. The second group contains large amounts of capital and skilled labor relative to unskilled and semiskilled labor. We shall call the first group the "labor-surplus" states, and the second group the "capital-surplus" states, although the relative nature of the surplus in each case should be kept clearly in mind.

In this setting, it is clear that the effects of the two specific national policy measures mentioned, standardized minimum wages and standardized union-enforced wage rates, will cause real wages in covered industries in the labor-surplus states to be too high from the point of view of the usual market criteria for maximum efficiency. If the minimum wage is at all effective, some qualified workers in the labor-surplus states will not be able to find employment at the wage rates prevailing in the industries covered by the legislation. These workers will be forced to remain unemployed or to find employment in those occupational and industrial groupings not covered by either standardized wage contracts or minimum wage restrictions; or, perhaps more realistically, these workers will be forced to remain employed ("underemployed") in the uncovered areas of the regional economy. It follows that wages in the occupations and industries not covered by either the minimum wage legislation or by standardized union contracts will be lower than those that would prevail in the absence of the national policy measures outlined.

Workers unable to find employment in the covered or standardized industries in the labor-surplus states will find it advantageous to migrate to the capital-surplus states to the extent that labor can be absorbed there. Since the policies discussed are national in scope, the employers in covered industries in the capital-surplus states cannot hire workers below the nationally set wage levels. Migration of capital and skilled labor from the capital-surplus to the labor-surplus states will also take place to some extent, but this migration may be impeded by the wage standardization policies.

We now seek specifically to answer the question: Is there any action that the individual states may take which will effectively offset the central government policy of enforced wage standardization? It is evident that only the labor-surplus states will be directly interested in such action.

If offsetting action by the individual labor-surplus state is to be effective, it must result in a lowering of real labor costs for the unskilled and semiskilled

categories in those industries covered by the standardization, where the relative surplus is assumed to exist. We propose a policy of state taxation of employees' wages coupled with the subsidization of all employers in proportion to their wage bills. This policy measure seems clearly to fall within the limits of state constitutional powers as outlined in the policy model. The tax would be generally imposed on all payrolls; it would not be discriminatory. The proceeds would be returned directly to employers of labor in proportion to their total payrolls.

The policy proposal suggested is deceptively simple, so much so that some further analysis of its effects seems warranted. The employer would be authorized to deduct the proportional state tax from the paychecks of the individual wage and salary earners. What would be the incidence of this tax? It is necessary here to distinguish quite sharply between those categories of labor which are in "surplus" and those which are "scarce." In our model, we have assumed that the labor-surplus areas are characterized by a relatively large number of unskilled and semiskilled laborers, but by a relatively small number of skilled workers. For the surplus laborers employed in covered or standardized industries, the tax would fall squarely on the wage earner. Because of the large supply of labor that is able and willing to work at the standardized wage rate, even after payment of the tax, the employer would not find it necessary to increase wages or salaries to offset the tax. The effect of the tax will clearly be to reduce labor costs to the covered industry employer for those categories of labor in surplus. In the case of the labor that is scarce, however, the tax-subsidy scheme will immediately cause employers to bid up the prices of these laborers. The tax will quickly be passed along to the employers who also receive the offsetting subsidy. On these groups, therefore, the tax-subsidy plan is fully neutralizing. There will be no incentive provided for those members of the labor force which are in the greatest demand to migrate to the capital-surplus states because of the fiscal action.¹

In the uncovered industries, wages are assumed to be determined competitively prior to any action. The result of the tax-subsidy scheme will be to increase the demand for labor in the covered occupations, leading to the transfer of labor from the uncovered to the covered industries where labor productivity is greater. If universally applied as this model assumes, the tax-subsidy plan would produce a full equalization of wage levels for comparable quality labor-

¹ If we assume that the supply of the scarce labor (skilled labor in our model) is fixed, the effects of the tax-subsidy scheme can be easily traced. The effect of the subsidy is to shift the demand curve for this category of labor upward so that the price increases by the full amount of the tax. This, of course, is a first approximation. To the extent that skilled laborers are substitutes for unskilled and semiskilled laborers in production in the relevant industries, skilled labor's wages will rise by less than the full amount of the tax. To the extent that the opposite or complementary relationship holds, skilled labor's wage rate will be increased by more than the tax.

This assumption as to the supply curve of the scarce labor may be contrasted with that of the surplus labor. In the latter case, our model assumes that the supply curve is horizontal at the going wage level. Thus, the subsidy-induced shifting in the demand curve has the effect of increasing employment without increasing wage rates. The incidence of the tax must remain with the wage earners.

ers in the covered and the uncovered industries. For laborers remaining in the uncovered industries, the net result of the proposed measure would be an increase in real wages after taxes. For society as a whole, the shifting of economic resources from less productive to more productive employments would clearly increase over-all efficiency, as this is normally measured.

Before the policy action, the primary burden of attaining some ultimate resource equilibrium necessarily rests on migration. After the action, there will be less incentive for those who are underemployed in the surplus-labor states to migrate to the capital-surplus states. Some semiskilled and unskilled workers employed in covered industries before the tax might be induced to migrate to the capital-surplus region to escape tax liability. However, by stimulating the inflow of capital investment into the labor-surplus states the proposed measure would, in the long run, tend to reduce the postulated difference in labor incomes between the two regions.

One of the most interesting features of the simple proposal of wage-bill subsidization from gross payroll taxes is its self-correcting or neutralizing aspects. As implied already in the analysis of the scarce labor sector above, the proposal cannot interfere with the "efficient" working of the market mechanism. It can only correct "inefficiencies," which it does by acting as a substitute for wage flexibility. This feature insures that the proposal could not become a means whereby interstate competition could reduce national real income, as measured by efficiency criteria.

A capital-surplus state, where, by assumption, both unskilled and semi-skilled labor are scarce, can never compete with the labor-surplus states by enacting the same policy measures. For, as we have seen, when the resource is scarce, the effects of the tax and the subsidy are completely neutralizing and nothing could be gained from implementing such measures. There is no federal government policy to be negated here.² By the same reasoning, once the relative labor surplus is eliminated in those states which we have called labor-surplus states, the policy ceases to be effective.

It must, of course, be recognized that the policy proposal presented here does not represent the best of all possible worlds for residents of the labor-surplus states. Broadly speaking, the effects of the policy outlined are similar to those that would result from a change in national policy which produces competition in the labor markets. State off-setting policy must be within the realm of the second-best, but it is important to recognize that it does provide a substitute for national policy changes with respect to minimum wage restrictions and union wage standardization.

Alternative policy measures taken at the state level may also be developed to deal with substantially the same problem. For instance, a policy which might generate more political support in the labor-surplus states would be one which extends the subsidies to only new enterprises. Many variants on policies of this sort are to be found in the real-world attempts of governmental units to

² If in fact national government policy should include the fixing of rates of return to non-labor resource units the analysis presented here is fully applicable. For example, a national minimum land rental policy could be offset by the "land-surplus" states.

attract new industry. Given the national policy as fact, there are many possibilities for independent action by smaller political units. These are worthy of further exploration and research.

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Population and Economic Growth: Comment

1. It is unfortunate that our current propensity to build models requires us to offer simple explanations of complex phenomena. E. E. Hagen's hypothesis that birth rates depend on death rates but that "birth rate and size of family calculus . . . is imbedded in unconscious motives relating to sex and family inculcated in children during their first six years" (p. 319) seems to me an obvious oversimplification.¹ Various objections are possible but in my judgment the most important is that the hypothesis ignores the fact that many children are "unwanted" in the particular sense that they would not be born if birth control or abortion were readily available.²

Recent Japanese experience provides remarkable evidence of the extent to which children may in this sense be "unwanted." Currently, between 40 and 60 per cent of all pregnancies (depending on the number of unreported abortions) are being terminated by abortion, which is both legal and financed by the national health insurance program. As pregnancy rates have not declined significantly, it seems clear that the drop in the Japanese birth rate from 34.3 in 1947 to 17.2 in 1957 can only be attributed to the legalization and widespread utilization of abortion.

Developments in Japan obviously cannot be explained by Hagen's hypothesis. If decisions regarding family size were in fact inculcated in children during their first six years, then the number reaching child-bearing age in a decade could explain a halving of the birth rate only if new mothers had been inculcated to want no children whatsoever—in this case during the height of Japanese militarism!

But it is not the technical inadequacy but the inadequacy of the policy implications of Hagen's hypothesis that I want to stress. If one took his hypothesis literally, birth rates could only be lowered by reducing death rates and then waiting a generation for changing subconscious inculcations to reduce birth rates. Actually, even if we accept his formulation for "wanted" children, so long as "unwanted" children are being born birth rates can be lowered as rapidly as birth control or abortion can be made available. Perhaps the proportion of "unwanted" pregnancies in Japan is higher than it will be found to be in other areas. But no one (so far as I know) foresaw a halving of the Japanese birth rate in a decade. It seems quite likely that, without any change in the inculcations distinguished by Hagen, drastic declines in birth rates in

¹ Population and Economic Growth," *Am. Econ. Rev.*, June 1959, 49, 310-27.

² Obviously this is only one of many possible meanings of "unwanted."

many areas could result if ready availability of birth control or abortion made it possible to eliminate "unwanted" children.

2. I take it that Hagen offers his hypothesis regarding inculcations before six in an effort to explain the observed lag between the fall in death and birth rates. I very much doubt whether a satisfactory explanation of the lag is possible unless the availability of birth control and abortion is brought into the picture—though this will *not* provide a full explanation and in fact is itself in need of ultimate explanation. I shall cite only three bits of evidence. The birth rate first declined among those with higher income, who obviously were among the first with access to birth control information. Secondly, the great reduction in the differential between the birth rates of those with high and low incomes that has recently taken place coincides with evidence from a recent sample survey that 90 per cent of fertile white married couples now use some form of birth control. And, thirdly, the fact that very large families—five children and over—have declined all through the sharp postwar rise in birth rates appears only capable of explanation in terms of increasing availability of birth control.

3. It seems unfair to Malthus to suggest that his model implies that the "birth rate is not influenced by the level of income" (p. 322). Surely Malthus would not have denied the impact of income on birth rates under conditions such as presently prevail in Japan. If he reached a different conclusion, it was only because he was prepared to allow income—or anything else—to influence birth rates only by encouraging continence or postponing marriage. With the possible exception of postponed marriage in Ireland, is there any evidence that, if these are the only acceptable techniques, he was wrong?

4. Hagen suggests that birth rates are likely to fall to a level close to that of death rates, yielding a "fairly low" positive rate of increase varying between .5 and 1.3 per cent per year. He quotes Notestein to the effect that "Western Europe and the English-speaking countries" are tending toward a birth rate "15 per cent above the secular minimum" with "the conspicuous exception of five countries" where the rate is considerably higher (p. 324). I wish Hagen had devoted more time to a discussion of the probable future behavior of birth rates. Keep in mind that what he brands "exceptions" include perhaps four-fifths of all English-speaking people and perhaps 40 per cent of the total population in question.³ Particularly if we continue to have enough illegitimate children to provide infertile couples with two or three children via adoption, an average three-child family involves substantial population growth and an average four-child family very rapid growth. It seems to me that, for a considerable period, the private benefits of having a third or even a fourth child are likely to exceed the private cost. Further, if Hagen's expectations regarding technological progress are realized, it seems likely that the rate of population growth of the "exceptions," where income is higher and expanding more rapidly than in the rest of the area, may well come to be the "rule" for the entire area. In short, there seems no certainty that in the future the rate of increase of developed areas will be "fairly low." Nor do I see any reason for

³ I have taken "Western Europe" to mean non-iron-curtain Europe excluding Greece and Yugoslavia.

expecting an increase in the private cost of having children until long after the social problems raised by continuing population growth have become acute.

5. I gather that while Hagen is inclined to deplore the extent to which our increasing productivity is being devoted to the production of children, he is not worried even for "the very long run"—partly because of his faith in technological progress and partly because he is not bothered by the prospect that 99.9 per cent of our population may in the future be living elsewhere than on earth. I think it is important to be clear on the magnitude and timing of the problem technology will have to solve. Let us conservatively assume that the population of the United States grows in the future at no more than a 1.3 per cent rate (the top of Hagen's range). I doubt if I would get widespread support for a proposal to invite the entire present world population to come live in the United States. At a 1.3 per cent rate of increase the same result will be achieved by 2170. If thereafter we wish to continue to increase at the same rate without adding to numbers on earth, we will have to put 30 million Americans a year into permanent orbit.

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Population and Economic Growth: Reply

For brevity, I shall not reply in detail to each of Professor Villard's points. I agree with many of his statements, but I view his facts in a perspective different from his.

1. I agree that my model would correspond to reality more fully if availability of birth control had been included as a variable. The Japanese case is a highly pertinent one. Still further, the differential desires of men and of women are pertinent, and the psychic costs of birth control methods might well be analyzed separately from their impact on births. If a method of birth control at low psychic and economic cost were readily available to women, I do not doubt that the lag between decline in death rates and that in birth rates would be greatly reduced. However, the model would be greatly complicated by such considerations.

The facts that the rate of population declined in the United Kingdom after 1869-1879, in France after 1855-1865, and in Russia after 1870-1885 are not, I suspect, explained by increasing availability of birth control, and the persistence today of high population growth rates in many countries where death rates were cut sharply ten or fifteen years ago indicates that birth control is not apt to spread rapidly everywhere today. Hence I think that the simpler model is of analytical importance.

Moreover, I do think that psychological attitudes, as distinguished from availability of birth control methods, are very important. But there is no point in arguing this further here. I regret that I did not discuss the possible influence of birth control textually.

2. I suggest that the early declines in rates of population growth cited above do indicate that Malthus' assumption was not highly realistic.

3. Long-run world population prospects run counter to my value judgments as well as to Villard's. In future generations green spaces may become scarce, and human structures may completely cover rivers and lakes; and a person may yet be able to drive from New York to London, past industrial enterprises and juke boxes. The problem is one of private versus public costs, but not merely that. According to the values of the majority of people, living standards may still be rising. I, in turn, am repelled at the prospect; but I see no reason to forecast a return to physiological subsistence levels. The magnitude of the problem technological progress will have to solve is no greater than that of the problem it has solved in the past.

Let me take this opportunity to clarify a statement in my article which has puzzled more than one reader. In my Figures 1 and 2, each v function reflects "a constant function relating saving (S) to income" (p. 317). This does not imply a constant ratio of saving to income at different income levels, but only an unshifting function. Further, the v function is a long-run one; in most circumstances one would not expect movement along it over time; rather, movement to the right or left and movement to a higher or lower function would occur together.

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The Shape of the Income Distribution: Comment

Theoretical doubts have seldom dissuaded students of the income size distribution from seeking that ideal function or even that empirical "law" which will fit income-size statistics of all times and all places. But there is serious cause for dispute when, apart from all theoretical argument, the empirical fit of such a function turns out to be poor.

In an article in the June 1959 issue,¹ Stanley Lebergott suggests that a symmetrical curve, in particular the normal curve of error, is at the heart of the income-size distribution. Lebergott argues that the relevant U.S. income distribution shows less skewness than we are accustomed to think and that such skewness as does exist can be attributed to "credit rationing" to the self-employed. Thus, citing *Current Population Reports* statistics for a population of males, age 25-64, he finds that the income distribution is "remarkably like the normal gaussian, remarkably unlike the usual skewed distribution" (p. 344).

If the notion of a symmetrical, or even normal, income distribution had empirical basis, a traditional view of long standing would be overthrown. But

¹ Stanley Lebergott, "The Shape of the Income Distribution," *Am. Econ. Rev.*, June 1959, 49, 328-47.

analysis of Lebergott's statistics suggests that the data do not entirely support his conclusion.

In Figure 1, we have plotted the income-size distribution on the basis of the statistics which Lebergott provides in his article (p. 341).² The population selected consists of males, age 25-64, not self-employed. This is the basic group for which Lebergott claims to have identified an income curve that is approximately normal. As is evident from our chart, the distribution is not extremely skewed, but on the other hand it is not a symmetrical curve.³

As one is accustomed to anticipate in an income distribution, there is a tendency toward skewness to the right and a distinct tail at the upper end of the curve. If we were arbitrarily to ignore all incomes above \$6,000, the distribution would be approximately symmetrical. But to ignore the tail is to throw the baby out with the bathwater. It is precisely the presence of an upper tail on the typical income distribution which economists have long sought to explain.

In order to provide a basis for comparison we have also shown, in Figure 1, a normal curve drawn on the same scale and with the same standard deviation as the income curve. The discrepancy between the two curves is not violent,

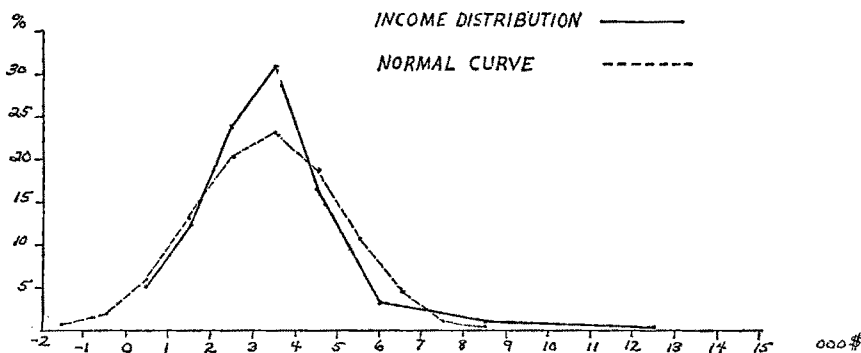


FIGURE 1. INCOME DISTRIBUTION 1951

but it is quite apparent. Considering the large size of the sample on which the *Current Population Reports* are based, a fairly close fit would be expected.

Supplementing casual empiricism with a test for goodness of fit, we calcu-

² I assumed a conservative midpoint of \$12,500 for the "\$10,000 and over" class. The unemployed and institutional population was omitted. It is not clear on what grounds Lebergott includes the unemployed and individuals making a negative income in a population which is supposed to be limited as much as possible to full-time employees.

³ Lebergott does not plot a distribution for these precise data. He may have been misled in his conclusion, by an error made in his chart of income statistics for all males 25-64, his Figure 1 (p. 340). As his graph is plotted, the distribution looks symmetrical. But this is the result only of failure to make allowance for differences in the class interval of the frequency distribution. The distribution would show considerable skewness if it were drawn correctly.

lated a chi-square. The statistic was computed employing the conservative assumption that Lebergott's sample consists of 5,000 observations (the entire *Current Population Reports* sample is approximately 12,000). The calculation yields a very high chi-square of 521.5, which is statistically significant beyond the .001 confidence limit. That is to say, the income distribution plotted would be drawn by chance out of a normally distributed population fewer than one time in 1000. In all fairness, it should be noted that the chi-square test is very sensitive when applied to large samples. But we have ascertained that in this case, the deviation from normal would be statistically significant at the .05 level, even if the income curve were derived from a sample of as few as 100 cases. As an alternative test relating only to the issue of skewness, we calculated α_3 , a measure of skewness based on the third moment of the distribution. An α_3 of 1.187 was obtained. This represents significant deviation from normal at the .01 confidence level. Similar results to those discussed here may be obtained with income data from other sources.

The analysis presented above suggests that even after eliminating the self-employed, contrary to Lebergott's argument, there are statistically significant differences between the normal curve of error and the income-size distribution. The empirical evidence does not support an approach to the problem of income size on the basis of the normal curve.

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The Shape of the Income Distribution: Reply

Mr. Adams makes somewhat heavy weather over two points. One is correct (and made in my article); the other, irrelevant to it. He concludes (1) that "contrary to Lebergott's argument there are statistically significant differences between the normal curve of error and the income-size distribution." My own discussion concluded: "... the shape of the income distribution ... is reasonably well defined as a normal distribution, truncated by credit rationing" (p. 345). Whoever takes that comment to imply that a truncated distribution does not differ significantly from a normal one had better mend his ways—and promptly too. Otherwise he may find both Adams and myself on his trail.

(2) Adams' central assertion is that the population "of males, age 25-64, not self-employed ... is the basic group for which Lebergott claims to have identified an income curve that is approximately normal." His proof that "it is not a symmetrical curve," is sound. But since no such claim is advanced in the article it is irrelevant—and irrelevant at each of the three levels of significance he offers. For I made no assertions concerning the employee group he discusses. This was "a grievous fault, and grievously have I answered for it," for other readers besides Adams must have overlooked the conclusions with respect to those groups that I did consider.

After making a variety of adjustments in reported income distribution data I concluded that the 1951 money distribution for *all* males aged 25-64 (not

merely Adams' "not self-employed") ". . . is clearly much more symmetrical than the typical Pareto curve" (p. 340). I then added: "On the other hand, distinct skewness still appears. Can we explain it?" (p. 340). One lead, I felt, lay in the fact that "the most noteworthy aspect of the usual income distribution is not that it has one long tail—but that it does not have two" (p. 343); and I went on to suggest the vital role of credit rationing in truncating the distribution.

This comment offers an occasion for noting that the original article did not make explicit its limitation of reference to periods of reasonably full employment.

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Errata

There are two typographical errors in the presidential address of Dr. Arthur F. Burns, "Progress Towards Economic Stability," published in the March 1960 number of the *American Economic Review*. The first one is on page 1, line 15, where the word "understanding" should be "understating." The second one is on page 3, line 2, where "proprietorship" should be "proprietorships."

BOOK REVIEWS

General Economics; Methodology

International Economic Papers, No. 8. Edited by ALAN T. PEACOCK, RALPH TURVEY, WOLFGANG STOLPER, and ELIZABETH HENDERSON. New York: Macmillan Company, 1958. Pp. 240. \$4.75.

Like its predecessors, this volume consists of English translations prepared for the International Economic Association. Also, as in the earlier issues, the accuracy and style of the translation itself is superb. Except for the rare and unavoidable instances where clarity requires direct reference to foreign usage, the volumes reads as if originally written in English.

The possible objectives of a collection of this sort are many and not all mutually compatible: The compilation of a representative sample of foreign work is quite different from a collection of outstanding contributions; material intended to further research along the lines of interest to English-speaking readers is not the same as that selected to indicate the direction of foreign interest. In the words of its preface, "Volume 8 of *International Economic Papers* covers a variety of fields and caters to a variety of tastes." This being true, it is unlikely that anyone will be enthusiastic about all the selections. In fact, general disagreement should indicate the successful application of the criterion of selection.

From one standpoint at least, the contents can be divided into four categories: (1) items of historical interest, (2) important scientific contributions, (3) minor theoretical argument, and (4) communist economics.

There are two articles in the "historical" category: The "Preface on the Doctrine of the Physiocrats" by Francesco Ferrara, dated 1850, involves a re-evaluation of the physiocrats. It is beautifully written and admirably translated, and in spirit so modern that one is almost surprised at the absence of reference to input-output tables. The other is Alfred Weber's "Location Theory and Trade Policy," doubly interesting as an example of an able mind at work applying theory to policy, and as a historical insight into the intellectual atmosphere of the Europe before 1914.

Only one of the selections is clearly of the calibre of an important contribution to economic science. That is Erik Lundberg's treatment of "The Stability of Economic Growth." His argument is: (a) that our data—especially those relating to the early period of development—are so poor that we have only the roughest idea of the real phenomena that growth theory is supposed to explain; and (b) that existing growth theories are so oversimplified that they take as fixed parameters the very things whose variation needs study. It is regrettable, however, that this oversimplification is identified with mathematical formulation and—even more dangerous—with quantitative research in general. Thus we find: "Dahmen's . . . [study of Swedish development] . . . provides us with a shocking reminder of how one-sided and narrow must be

the manner of posing the problems in quantitative models" (p. 62). If this is, in fact, a "necessity" of quantitative method, then the "multisided," "broad" approach of theory must be due to divorce from reality.

True enough, some economists—not all young, and by no means all mathematicians—are so eager for quick "results" with simple "tools" that they search for answers to questions nobody asked about an economy that does not exist. The reason mathematicians are often blamed for this is that mathematical exposition is more transparent than a sonorous prose style, and they are more easily found out.

The articles that I would classify under the heading of "minor theoretical argument," are often illustrations of this. Thus Stackelberg's "Price Discrimination in an Arbitrarily Divided Market" deals with the manner in which a pure monopolist would divide a homogeneous market in order to maximize profit via discriminating pricing. It is perfectly clear that there are no such firms, such markets, nor such objectives, and the theoretical model must be an approximation, although to what is not clear. Even in terms of the model, however, the assumption is made that the demand at any one price is independent of what is sold at any other. Such a demand would seem to imply rather strong natural divisions in the market.

Ragnar Bentzel, in his paper "On the Aggregation of Production Functions," follows an impressive list of economists in investigating the conditions under which relationships that hold for individual firms, households, etc. can be extended to a macroeconomic level to hold among social aggregates. Actually the real justification of aggregation lies in ignorance of underlying detail, or in the inadequacy of tools to analyze it with. Thus to ask when aggregates lead to the same relationships as details is to ask "when can we get something for nothing?" The answer "rarely, if ever," agrees with Bentzel's conclusions.

Erich Schneider's treatment of "Income and Income Distribution in Macroeconomic Theory" comes to grief on this very rock. His thesis is that at the microeconomic level "analysis of the formation of income is, at the same time, analysis of income distribution, and this is the reason no special theory of income distribution is needed. . . . This must needs also hold for general analysis in macroeconomic terms since the latter . . . differs only by the fact that suitable aggregation reduces the number of variables to manageable proportions" (p. 111). But it is precisely this that is wrong. Income distribution is *essentially* related to the pricing mechanism which is completely omitted from the aggregate system.

Lief Johansen develops "The Role of the Banking System in a Macroeconomic Model," constructing a system of equations which illustrate how certain "rigorously simplified" behavior parameters of the banking system might be introduced into a simple macroeconomic model of the economy.

Jürg Niehans attempts to develop "An Index of the Size of Industrial Establishments" which will meet a number of criteria of behavior. Actually, all these are satisfied by the arithmetic mean except the requirement that: "If one establishment grows without the simultaneous diminution of any other in the same group, the index should rise in the case of large and medium-sized establishments and fall in the case of small ones." The Niehans index thus.

becomes a combination index of average size and of concentration, constructed by the use of weights based on a Lorenz curve, but it is not clear what it is really expected to accomplish.

The final piece of theoretical argument is the treatment by Maurice Byé of "Self-financed Multiterritorial Units and their Time Horizon." The paper is "concerned only with the proper definition of certain problems" relating to large extractive firms and conflicts of interest with small resource-owning governments. Conflicts are shown with respect to the timing of exploration, development, investment, extraction, etc. A number of ideas are suggested and partially analyzed but never clearly defined; and the paper proves to be disappointing after promising much.

The two concluding articles "Critical Comments on the Use of the Gross Production Index," by Rumen Janakieff, published in East Berlin, and "Money in Socialism" by Stefan Varga, a Hungarian economist publishing in the *Weltwirtschaftliches Archiv*, are the ones in the communist economics category. They are of considerable interest in understanding the problems of theorizing in relation to economic planning in a communist economy.

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Ökonometrie und makroökonomische Theorie. Stochastische Wirtschaftsforschung als notwendige Ergänzung der Theorie. By HEINZ STÖWE, Stuttgart: Gustav Fischer Verlag, 1959. Pp. viii, 190. DM 17.50.

Stöwe's book tries to throw a cloak of respectability on econometrics—for the benefit of onlookers recruited among German economists. It seems to be a timely effort because the teaching of economics in Germany has still quite a few links with the historical school as well as with the psychological and sociological approach. Theory expressed in mathematical form or with the help of mathematical notation is known (e.g., Schneider, von Stackelberg), but gets relatively little attention. A fortiori this applies to econometrics. On the other hand, German economists are eager to pay attention to the methodology of economics, its philosophical presuppositions, and its relation to the sciences. Therefore, to present the achievements of econometrics in works published in English, Stöwe arranges his material to suit the views of his prospective readers.

Econometrics, he says, is not just the use of mathematics or economic statistics; econometrics is also an essential element of the science of economics; or, as his subtitle has it, stochastic economic research is a necessary complement of theory. In economics, as in any other science, we observe, propose hypotheses, and then test them. Testing is not of the laboratory variety: it is based on observation of human behavior. Hence, it must allow for random elements. Econometrics with its use of stochastic relationships fills the bill. It helps the investigator to choose between those theories which are more probably correct and those less probably correct: it helps to avoid the danger of multiplying models not rejectable on the strength of their internal logic. It also supplies the economist with estimates of parameters for those theories • which are accepted as being probably correct. Hence, econometrics supplies

a missing link in the structure of economic science; those paying their respects to methodology should not even blink an eye. Besides, adds Stöwe, it is in line with the views of such recognized forefathers of present-day economics, as von Thünen, Cournot, or Schmoller. This completes the proof of *seize-quartiers* of econometrics.

The greater part of the book is devoted to examples showing the use and the usefulness of econometrics in its application to macroeconomics. Stöwe chooses macroeconomics because it is in the limelight. He does not concern himself with the aggregation problem. There are, however, microeconomic problems such as costs of a firm or the study of demand of individual households which lend themselves to econometric study. These problems may have greater appeal for many theoretically minded economists for whom microeconomics is theory par excellence.

To show how econometric methods can help in estimating parameters and in rejecting unsatisfactory hypotheses, Stöwe reviews studies of Hicks' accelerator (Stöwe's own calculations relating to the German economy are compared with Gene Fisher's findings for the United States), the consumption function, and the determination of investment. Then Stöwe speculates on the possibility of applying econometric methods to the models of input-output, linear programming and the theory of games. In the discussion he shows good knowledge of the literature in the English language.

The book is clearly aimed at German economists. There is nothing essentially new in Stöwe's views that has not appeared in the English language. However, it is a worth-while reminder to his non-German readers that Leontief, Frisch, Schumpeter and others have seen in econometrics more than simply a method of prediction or an interesting field for applying one's knowledge of mathematics. Ragnar Frisch stressed that econometrics supplies us with the unification of the theoretical-quantitative approach and the empirical-quantitative approach. Leontief saw in it the statistical sifting of promising analytical insights. It is good to be reminded that due to the development of econometrics economic science is more complete and methodologically more satisfactory.

STANISLAW WASOWSKI

Georgetown University

Principles of Economics. By the Committee on Principles of Economics. New York: Pitman Publishing Corporation, 1959. Pp. xx, 873.

This long introductory text should have wide use—after all, it has 67 different authors. In the words of the editor, it is “neither exclusively classical nor exclusively Keynesian; not entirely analytical nor entirely institutional” (p. ix). Actually, the book's organization gives it more a traditional than modern flavor. Part I is introductory. Part II deals with the determination of output and prices. Part III discusses the distribution of income (personal and functional) and has chapters on social security and labor institutions. Money and banking are treated in Part IV, and discussions of economic fluctuations and stability follow in Part V. Part VI is devoted to government regulation of the

economy, and Part VII takes up international economic problems. The book ends with a section entitled "The Economics of the Future."

This reviewer has no real quarrel with the book's over-all organization, although an earlier discussion of national income determination, which in the book comes after the chapters on money and business fluctuations, might have prevented some repetition. The book does suffer, however, from the promotional device of 67 authors. Most chapters were jointly authored by two or three individuals. Unfortunately, there was no invisible hand which led each writer's contribution, no matter how excellent or poor, to combine with those of the others into a harmonious whole. The writing is uneven, both within and between chapters. The lack of a basic point of view, I believe, damages the book.

While the integrity of most of the writers led them to rather full treatments of their assignments, the result is a good deal of repetition and disproportionate discussion of many topics. The analysis of oligopoly, for example, receives the same space as the discussion of partnerships—four pages. The whole theory of market structures—pure competition, monopoly, monopolistic competition, and oligopoly—receives only ten pages more than the theory of demand (32 as opposed to 22 pages).

From a technical point of view, the book is generally competent. Occasionally, however, there are errors which represent more than simple slips. For example, pure monopoly cannot be analyzed with simple supply and demand curves (pp. 195-96); the supply of money in liquidity-preference interest theory, being a stock, has no interest-elasticity (p. 387); and Schumpeter would have been greatly perturbed to find himself classified with those who "laid the groundwork for the development and *subsequent acceptance* of Keynes' theory" (p. 23, my emphasis).

At times, the normative and teleological are improperly mixed: "One of the basic tenets of economics is that *one should always* seek to minimize costs (or efforts) and maximize returns (or results)" (p. 15). "The story of economic growth is revealed in the gradual, persistent unfolding of *man's destiny* in the realm of satisfying his material wants" (p. 57, my emphasis in both quotes).

On the whole, students will be able to learn their beginning economics from the book. Indeed, some of the chapters are well organized and well written. Nevertheless, the basic defect of the book—too many authors—seriously detracts from its usefulness as a pedagogical device.

BARRY N. SIEGEL

University of Utah

Price and Allocation Theory; Income and Employment Theory; Related Empirical Studies; History of Economic Thought

The Logic of Investment Planning. By S. CHAKRAVARTY. Amsterdam: North Holland Publishing Co., 1959. Pp. xii, 170. \$4.00.

The general economist in quest of the theory of planned capital accumulation is likely to find this dissertation a rather difficult, if not long, struggle of

escape from habitual modes of thought and expression. There is here little of the usual substance of problems of variable factor proportions, changing production functions and intratemporal and intertemporal choice.

Chakravarty deals with a world in which there is no labor and no land; capital is the only factor of production and it does not depreciate. The relations with which he works are linear. Through most of the analysis there is a complete abstraction from money and prices, and when they are introduced in the penultimate chapter it is hard to see their significance.

The author is concerned with the problem of economic development. After a rather lengthy explanation of his "method" in which he discusses "the mathematical mode of reasoning in economics," "static" and "dynamic models," the role of initial conditions in difference equations, "optimum" versus "specified" problems (relating to methods of choosing the "open ends" or nonhomogeneous elements in the dynamic system), the "structural break," "analytical" versus "policy" problems, and the "planning tools," the author turns to a consideration of various growth models. This is a useful survey tying together within an input-output framework and interrelating the works of Harrod and Domar, Mahalanobis, Leontief, and von Neumann.

Chakravarty finds the Harrod-Domar models suggestive but overaggregative for his purposes. Discussion of Mahalanobis' two-sector model highlights the importance for growth of not merely the total of investment but investment in the capital goods industries. Von Neumann's model of balanced growth is criticized as "a somewhat remote idealization," but the author fails to consider the possibility (suggested by Dorfman, Samuelson and Solow) that such an optimum equilibrium path can be a sort of "turnpike," to which economies may well detour from their initial positions and from which they may depart as they approach their particular destinations but which may be worth the round-aboutness in terms of shortening the time of the entire trip. But in any event, most of Chakravarty's attention is devoted to the dynamic Leontief model. This is taken in Leontief's original version, involving equalities rather than inequalities in the capital-output relations so that, in effect, excess capital or idle capacity in any sector is impossible. And like Leontief, Chakravarty does not meet the problem of long-run optimization.

Whether dealing with open or closed economies and models with or without prices, the author generally presents dual approaches. For "analytical" purposes, he points out, one can take the various amounts of investment (or export balances if these are relevant) as given and ask what outputs will be at some specified period or periods in the future. Or for "policy" purposes, one can set certain targets with respect to future outputs and ask what investment would have to be in each sector to achieve them. The chief novelties in Chakravarty's treatment are his attention to "gestation lags" and the "structural break."

The latter is devised to meet the problem imposed by assumptions of linearity and constant coefficients relating investment of each sector to total saving (in turn linearly related to income). To illustrate (as the author usually does not), if an economy has no output of steel, reflecting the fact that the coefficient for investment in the steel industry is zero, it will never have any output of steel until that coefficient is changed and investment in

the steel industry is begun. Such a change in a coefficient (the only kind considered) is called a "structural break."

It will not, however, result in an immediate pouring forth of output from the previously "empty sector" because of the gestation lag. This is not handled, though, in a particularly meaningful way. For example, Chakravarty writes (p. 135, and this is consistent with his treatment of the lag through most of the book):

$$W_k = \frac{b_k}{L} [V_k^{t+L} - V_k^t]; \quad k = 1, 2, \dots, n,$$

where W_k is the investment in the k th industry, V_k is the gross output of the k th industry, b_k is the capital-output coefficient of the k th industry, L is the "gestation lag," and the superscripts ($t+L$ and t) refer to time periods.

Chakravarty's formulation, which stems from a decision (p. 88) to write investment completed between periods t and $t+L$ as equal to L times the investment initiated in the period t , can only work out consistently when each sector's investment (W_k , better written W_k^t) is constant. But this contradicts both the assumption of the structural break and the author's assumption that total investment is a (generally homogeneous) linear function of (a generally growing) income. Chakravarty's investment relation might be reformulated as:

$$W_k^t = b_k [V_k^{t+L} - V_k^{t+L-1}]; \quad k = 1, 2, \dots, n$$

or, using distributed lags,

$$W_k^t = \sum_{i=1}^L b_{ki} [V_k^{t+i} - V_k^{t+i-1}]; \quad k = 1, 2, \dots, n,$$

where

$$\sum_{i=1}^L b_{ki} = b_k \text{ and the } b_{ki} \text{ for small values of } i$$

(that is, for short lags) can be equal or close to zero.

A reformulation of this nature would of course force Chakravarty to recount the equations in most of his models (and he does devote a lot of space to counting equations and variables); but it would appear to be important, since the investment relation is the only dynamic relation and the only growth-producing relation he includes. Such a reformulation would of course not contradict one of the few *substantive* implications of Charkravarty's analysis (but one not new, for example, to those familiar with Hicks' *Trade Cycle*) that the effect of lags in the relation between investment and output is to slow the rate of growth in a growing economy.

But this is an exercise in the *formal* implications of input-output models for the allocation of investment. While the presentation is hardly sparkling, is frequently tedious and might have gained considerably from fluent English-speaking editors and copy-readers, the work is generally competent and should

prove useful for the growing number of economists throughout the world who are applying techniques of input-output analysis and linear programming to economic problems.

ROBERT EISNER

Northwestern University

The New Inflation. By WILLARD L. THORP and RICHARD E. QUANDT. New York: McGraw-Hill, 1959. Pp. xi, 233. \$5.00.

During July 1958, twenty-three economists spent two weeks at the Merrill Center for Economics discussing the problem of inflation. The group included several distinguished students of the topic, and there can be little doubt that the conference was a rewarding experience for all who attended. One result is the book under review, by Willard L. Thorp, Director of the Merrill Center, and Richard E. Quandt, a participant. In light of this background one might have expected something on the order of *The Impact of the Union*—a volume containing the principal papers and ensuing discussions, together with the editors' comments. Instead, the authors have written a brief and general survey which seems quite unrelated to what went on at the conference.

Judged by the level of sophistication, *The New Inflation* is intended primarily for informed laymen. Its treatment of economic theory is neither rigorous nor detailed, references to professional literature on inflation are few, and empirical documentation is minimal. Chapter 1 is devoted to the definition of inflation, a description of United States price data, and a brief review of recent price-level changes. The next five chapters, which take up the causes of inflation, are the core of the book. Here the authors discuss the quantity theory, simple Keynesian inflation analysis, the role of structural pressures, the relationship of growth to inflation, and international aspects of the problem. The remaining four chapters deal with policy and the prospects for further inflation.

As the title of the book suggests, Thorp and Quandt have a thesis to expound. It may be summarized as follows: (1) Analysis of the periods 1948-54 and 1957-58 indicates that "prices can be pushed about by many forces in addition to, and sometimes other than, expanding demand" (p. 96). (2) Monetary-fiscal policies should be used to counter demand inflations "except when their use will cause serious unemployment or retard economic growth" (p. 228). (3) Inflations from other causes must be attacked through such policies as promotion of resource mobility, reduction of trade barriers, the fostering of investment incentives, improvement of productivity in the lagging sectors of our economy, and creation of "countervailing powers to excessive price or wage increases" (p. 228). (4) Moderate inflations are not the great evil they are commonly thought to be, and there is little likelihood of severe inflation in the American economy.

Of course, similar views have been expressed on many occasions in recent years. To this reviewer, however, Thorp and Quandt's argument is no more convincing than those of other New Inflationists. Like the others, Thorp and Quandt fail to demonstrate the novel character of recent price-level rises. Although they place great emphasis on the fact that inflation continued during

the 1957-58 recession, they largely ignore the fact that this happened as well during several recessions before the second world war. And also like other New Inflationists, Thorp and Quandt do not distinguish carefully between individual price behavior and behavior of the price level. Clearly it is not enough to demonstrate that in some instances excessive wage increases have been responsible for price rises. One must also show either that aggregate demand has increased or that resources have become idle—in both cases *because of the wage-induced price rise*. It may be that Thorp and Quandt's interpretation of the nature of recent inflation is correct, but much more hard analysis will be required before we can be reasonably certain of this.

In a sense the foregoing comments are not quite fair since *The New Inflation* does not pretend to be a definitive monograph on price-level fluctuations. As a popular work on inflation the book accomplishes much. For example, the general reader should find informative Thorp and Quandt's discussions of alternative price indexes and their shortcomings, the concept of excess capacity, and international aspects of inflation. However, the reader will have to wait a while for answers to the basic questions the authors raise in their preface.

RICHARD T. SELDEN

Columbia University

Determinants of Consumer Demand for Housefurnishings and Equipment.

By VERNON G. LIPPITT. Cambridge: Harvard University Press, 1959. Pp. xiv, 172. \$6.00.

This study, originally a Ph.D. thesis, presents an interesting new approach to the joint use of cross-section and time series data in forecasting aggregate consumer expenditures over time. Briefly, the procedure is, as follows:

1. Analysis of variance is used to measure the numerical effect of relevant cross-section variables on the expenditure item under study. Thus, if the spending ratio (R_{ijk}) of families in the i th income group, j th family size, and k th occupation of head is influenced additively by these aforementioned variables, the estimate of this spending ratio (R) can be represented as:

$$(1) \quad R = r_s + r_i + r_j + r_k$$

where r_s is the mean, or "standard," spending ratio in the population. By means of variance analysis, estimates are obtained of the r 's for different income-family-size-occupation cells.

2. Estimates of the aggregate effects of these variables are derived for years for which the necessary cross-section data are available. This is done by using the estimates derived from (1) to compute that portion of the year-to-year change in the aggregate spending ratio due to these variables. The estimate of this aggregate ratio for a given is:

$$(2) \quad R = r_s + \sum_i (Y_i/Y)r_i + \sum_j (Y_j/Y)r_j + \sum_k (Y_k/Y)r_k$$

where Y represents aggregate income, Y_i the aggregate income of those at income level i , Y_j the aggregate income of those in family size j , etc.

An equation similar to (2) is obtained for a base year, which when subtracted from (2) yields an estimate of the change in this ratio. The resulting expression multiplied by aggregate income in the given year provides an estimate of the shift in aggregate expenditures (X) attributable to shifts in the distribution of families by these cross-section variables, i.e.:

$$(3) \quad \Delta X = Y(\Delta R_i + \Delta R_j + \Delta R_k)$$

where, for example,

$$\Delta R_i = \sum_i (Y_i/Y - Y_{i0}/Y_{i0})r_i.$$

3. By means of interpolation, estimates of the distributional effect, ΔX , are obtained for other years in the period of observation. This variable is then incorporated into a time-series function regressing aggregate expenditures on ΔX and on such other time-series variables as may be considered relevant.

Lippitt provides a convincing demonstration of the possibilities of the method by applying it to the prediction of the demand for house furnishings for 1946-53 on the basis of time series for 1929-41 and cross-section data for 1929, 1935-36 and 1941. He demonstrates that the distributional effect, ΔX , has a pronounced effect on the expenditure ratios obtained for the benchmark dates as well as for the postwar years 1950 and 1953. The variable, ΔX , is also shown to make a significant contribution to the time-series relation, both by improving goodness of fit and increasing accuracy of prediction.

Unfortunately, the study is not without limitations, most of them recognized by the author. For one thing, underlying the feasibility of the method is the assumption of additivity of the effects of the cross-section variables, an assumption that is supported by analysis-of-variance tests on 1935-36 expenditure data. However, these tests were carried out on group averages, which are notorious for suppressing interaction effects in consumer behavior studies. To judge by some past work of the reviewer ("Service Expenditures At Mid-Century," in Conference on Consumption and Saving, University of Pennsylvania, March 30-31, 1959, to be published), had the more valid approach been followed of carrying out the analysis for individual families—an approach which, with unequal class numbers, is not exactly a statistician's delight—significant interaction effects would very likely have been obtained. If so, the procedure outlined does not produce unbiased results.

Question may also be raised regarding the extent to which effects determined from cross-section data are applicable over time and therefore can be substituted for corresponding variables in a time-series relation. The relationship obtained from the cross-section data is a static one: it does not necessarily portray what may happen with the passage of time. Thus, the fact that families earning less than \$3,000 spend one percentage point less this year on housefurnishings than do those earning between \$3,000 and \$5,000 does not necessarily mean that families whose incomes fall below \$3,000 next year will adhere to the same spending pattern as those previously at that level. It is not unlikely, however, that yearly shifts from one cell to another are generally relatively infrequent, so that the resulting biases on spending patterns are

small on an aggregate basis. This is an area where panel data would be extremely valuable.

Another question relates to Lippitt's admittedly bold assumption that the effect of the cross-section variables remains constant over time, and hence effects measured in 1935-36 are applicable to any year from 1929 to 1953. In the absence of supporting evidence, it is hard to believe that such an assumption is indeed valid. Considering the basic importance of this assumption to the validity of the method and of the results obtained, it is surprising that this assumption does not appear to have been tested, particularly considering the availability of the 1950 consumer expenditure data (though possibly they did not become available to the author until after he had received his union card).

These comments notwithstanding, there is no doubt that Vernon Lippitt has made a real contribution to means of explaining and predicting economic behavior. The general procedure may be applicable to areas other than consumer behavior. In addition the book is clearly written, well illustrated with simple examples, and could serve as a model for other technical studies of this type.

ROBERT FERBER

University of Illinois

Ausgaben und Verbrauch in Abhängigkeit von Einkommen und Haushaltsstruktur. By H. GOLLNICK. Hannover: Alfred Strothe Verlag, 1959. Pp. 262. DM 20.75.

In 1927-28 and again in 1950-51 the official German statistical office induced small samples of wage earners to keep diaries of their expenditures over a twelve-month period. While the summary results were published in 1932 and 1957, respectively, it remained for Dr. Gollnick to present the relationships of diverse consumer expenditures to explanatory variables and to compare the prewar and postwar data. He argues that it is useful to consider homogeneous groups when one wishes to analyze the relation of consumer expenditures to income. The original studies were restricted to families in which the head worked for wages and salaries during the entire year; Gollnick further restricted the samples to three- and four-person families (husband and wife, with either one child or two children under 21) who did not have any income in kind. The postwar study is then based on data from 697 families who did not differ much by income. The range of the annual family income is between 2000 and 8000 DMark, but the income of about 75 per cent of the sample falls between 3300 and 4700 DMark.

The author examines the relationship of various expenditures to the age of children and to differences between three- and four-person families, as well as between blue-collar and white-collar workers; but the major analyses relate expenditures to income. Thus, it is shown which expenditures remain fairly stable in different income groups, which increase proportionately to income, and which increase more than income. The expenditures for potatoes, bread, milk and sugar are inelastic; in the middle ranges are meat, eggs, and rent; • and the highly elastic expenditures are for coffee, beer, clothing, furniture,

education and recreation. Most income-expenditure curves after the war are similar with those before the war. Price differentials have, however, influenced consumption. In addition, some structural changes in consumption are shown. For instance, 20 per cent less milk was consumed in 1950-51 than in 1927-28 in all income groups, although relative to other prices milk prices fell by approximately 10 per cent. "The cause for this surprising development probably is to be found in the successful weaning of the consumers from milk during the war years and the first post-war years" (p. 158). To the reviewer this explanation is more surprising than the finding itself; commonly deprivation makes a commodity more desirable so that consumption rises after the shortages have disappeared.

Although the author presents numerous regression equations and their statistical significance, the limitations of the data are not discussed adequately. A few words are said about the fact that the prewar study was conducted during a prosperous year while the postwar study took place at the very beginning of the upswing, two years after stabilization, when there was much deferred demand. No reference is made to the fact that the prewar study embraced all Germany and the postwar study only Western Germany. How the samples were selected is not discussed and possible limitations of the diary method, particularly regarding the income data, are not mentioned.

The volume with its very extensive material—there are 54 tables and 80 charts—is disappointing to the reviewer in a deeper sense, too. Because of the limitations of the material, there is no discussion of the great economic problems on which the reviewer hoped some light would be shed. In the course of a few years, West Germany rose from great poverty to widespread prosperity; how has this transformation affected consumption patterns? We cannot receive even a hint of the answer to this question because the material relates, as said before, to 1950-51 and no similar studies and no comprehensive income, savings, or expenditure surveys were conducted since that time in Germany. Similarly, we do not receive any information on the effects of income increases, upward mobility, or consumer expectations on consumer expenditures. The differences among social classes and between the expenditure patterns of the rich—who have been excluded from the studies here discussed—and of the poor are not studied.

The usual explanation an American traveler receives in West Germany for the absence of a variety of much needed economic studies and especially of cross-section studies (in contrast to analyses of aggregate data) is lack of funds. The author of the present study also refers to this lack when he explains why he was not in a position to use electronic equipment needed, for instance, for multivariate analysis. It would be a sad commentary indeed, if it were true, that Germany, once in the forefront of scientific inquiry and today one of the most prosperous countries in the world, had not enough funds available for an economic and sociological analysis of its prosperity.

GEORGE KATONA

University of Michigan

Dynamic Changes of Income and its Distribution in Japan. By CHOTARO TAKAHASHI, in collaboration with RYOTARO IOCHI and KIOCHI EMI. Economic Research Series No. 3, The Institute of Economic Research, Hitotsubashi University. Tokyo: Kinokuniya Bookstore Co., Ltd., 1959. Pp. 182.

This study, concerned chiefly with shifts in income distribution over the course of the business cycle, falls into three parts. The first consists of a brief analysis of the various measures of dispersion employed in the study, while the remaining two portions, dealing with the substantive problem itself, are concerned respectively with the period up through 1944 and the postwar period in Japan. In its treatment of these two periods the study utilizes a variety of data. The analysis of the former period considers tax statistics (on personal income and profit) and wage data classified by industry and sector of the economy, while the analysis of the postwar period employs income tax statistics, special studies of family income and expenditure, and welfare survey.

In so far as the main problem of the study is concerned (the cyclical relation between income level and income dispersion), the results are inconclusive. Much of the data, particularly those dealing with the period up through 1944 (where the data sometimes cover several decades) indicate that income dispersion grows during the period of expansion and falls during the period of contraction. (A notable exception to this pattern is the period of the second world war itself; and it may be noted further that, for the first time over any comparable period covered by the statistics, the data clearly show a movement toward greater equality since the war's end.) However, the statistics revealing the parallel cyclical movement of income level and income dispersion are largely those drawn from tax returns up through 1944. Owing to the high exemption level, as the study notes, the personal income tax returns cover only the highest 10 per cent of the income recipients and account for only a fraction of the national income. The data thus reflect changing cyclical dispersion mainly among property-income recipients.

Though the wage data are not comprehensive, the analysis here does in fact suggest (along with a portion of the welfare survey findings) that, cyclically, the level and dispersion of income among the lower-income groups move in opposite directions. This movement, as is observed, may well overbalance the effects of any wage lag relative to profits during both phases of the cycle and determine the pattern for the distribution of income as a whole. If so, this would conform to the findings for the United States which, at least for the period 1939-1949, indicate that, owing to a variety of movements affecting lower and middle income groups (movements to jobs with higher pay or lower income dispersion, a reduction of income differentials, a rise in the number of full-time workers, and an increase in the number of supplementary family workers), income inequality declined for the distribution as a whole. (Cf. Herman P. Miller, *Income of the American People*, New York: 1955, esp. Ch. 8 and 9.) It may be noted that a recent study, *Employment Structure and Business Fluctuations*, by the Economic Planning Agency in Japan, indicates

that, owing to the attractiveness of higher wage rates, here too the number of supplementary family workers increases during periods of prosperity, and that the common supposition that, over-all, such workers swell the labor force during periods of contraction (in an attempt to maintain family income) is incorrect.

It appears that Takahashi and his collaborators have made extensive use of available statistics in Japan in their painstaking analysis. Indeed, in view of the (acknowledged) inadequacies of the data bearing upon the central issue, the statistical analysis may well appear too painstaking. As the statistics are presented segmentally (with each type of data considered separately and in a rather unintegrated fashion), the study sometimes leaves the reader in doubt concerning the rationale of the procedure and the authors' own views on the general significance of their findings. The authors, however, offer their work mainly as a preliminary exploratory venture, not as an argument in confirmation of a specific hypothesis. As such the work will provide material (particularly on various subsidiary movements revealed by the different types of data) for those studying the same question in other countries and will serve as a pathbreaker in a field which is certain to grow in importance in Japan.

EUGENE ROTWEIN

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Wirtschaftsgestaltung. By HANS BAYER. Berlin: Duncker & Humblot, 1958. Pp. xvi, 758. DM 58.60.

As an economist Hans Bayer was brought up in the tradition of the Austrian School. He began his career as an assistant to Professor Mayer at the University of Vienna, taught as professor of economics at the University of Innsbruck, and is now director of the Sozialakademie Dortmund. Of his writings, *Wirtschaftsgestaltung* is up to now the most significant. It represents a synthesis of economics, achieved through an applied theory of economic tensions (*Spannungen*). In his aim for synthesis Bayer is progressing along the road indicated by Johan Åkerman (*Das Problem der sozialökonomischen Synthese*, Lund 1937); in his systematic attempt to view the economy as a complex of economic tension relationships he is making an original contribution to dynamic economic theory.

The first part of the book is an exposition of the modern problems of national and world economy as well as of those that have arisen in the course of economic history. Through this exposition the basic relationships in the economy are recognized, and illustrations are provided for the second part of the book which constitutes a theory of economic tensions. Existing theories are found to consider economic tensions only to a degree. In his own analysis of tensions Bayer distinguishes (1) tensions between relative and absolute values, (2) tensions between consumption and production, (3) tensions between the economic spheres of money and goods, and (4) tensions between the individual and society.

Testing his theory on four models (simple economy, free competition, monopoly, and monopolistic competition), Bayer puts to excellent use the suc-

cessive approximation techniques of the Austrian School. The model of simple economy (Friedrich von Wieser's *einfache Wirtschaft*) corresponds to the principle of *Wirtschaftsgestaltung*, where "the decisions are from the outset directed toward the achievement of all-over goals" (p. 454). The concept of *Wirtschaftsgestaltung* does not, however, exclude free competition.

The comparison of the models is tied to the question of the possible reduction (*Überbrückungsmöglichkeiten*) of tensions which exist in various models, because it is through the reduction of tensions that the goal of the economy can be reached. In contrast to the view championed by Max Weber, which limits the economist to the analysis of what *is*, Bayer's system is oriented toward absolute values, and these can be arrived at ontologically. From such perspective the goal of the economy is "to secure constantly the widest possible material basis for the development of personality" (p. 417). Of the four models, that of the simple economy is the best suited to lead toward the achievement of this goal. The model of monopolistic competition is found to be the least satisfactory because "from a national-economic standpoint monopolistic competition essentially combines the disadvantages of free competition and those of the monopoly" (p. 476).

The realization of the model of simple economy is dealt with in the third part of the book. The investigation shows possibilities of wide decentralization, which permits an unrestraining (*freiheitlich*) realization of the model. Through co-ordination of autonomous social forces (*Ordnung von unten her*) only a few residual tasks are left to the central authority (*Ordnung von oben her*). Thus Bayer closes the circle which leads from empirical evidence by the way of theory back to the solution of actual problems indicated at the outset. In the successful application of theory to current economic problems lies the significant difference between Bayer and the Austrian School which, finding complacency in the theory, failed to provide solutions to the pressing questions of its time.

Bayer's style is lucid and pervaded with a sort of *Gemütlichkeit* which comes to expression in colorful analogies and examples. Much of the material which Bayer uses as empirical evidence has been collected during his travels through Europe, America, and Asia. His impression of a certain relaxation in Chinese planning would have to be re-evaluated in the light of the introduction of the communes in 1958. While there was some decentralization of decision-making because of the new small-scale industries operated by the communes, there resulted also a weakening of the position of the family, which Bayer considers an important factor in the *Ordnung von unten her*. A thorough application of tensions apparatus—*mutatis mutandis*—to the communist system would be of great interest, but this would be too much to demand from the single-handed author who, as it is, has shown an intimacy with the modern theoretical and practical literature which would ordinarily be expected only from a team of specialists.

TOUSSAINT HOČEVAR

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Economic History; Economic Development; National Economies

The Comparative Study of Economic Growth and Structure. New York: National Bureau of Economic Research, 1959. Pp. viii, 200. \$3.00.

This is a report on two National Bureau conferences which considered research objectives in the area of comparative national economic growth and structure. Part I, by R. W. Goldsmith, presents the results of the exploratory survey at these conferences, while Part II reproduces eleven supplementary memoranda by individual economists (Aubrey, Cairncross, Colm and Geiger, Goldsmith, Hagen, Hirschman, Hoselitz, Kuznets, Reynolds, Spengler, and Tinbergen).

Obviously the basic questions involve the selection of what problems are to be examined, in what countries, during what periods, and with what methods. Goldsmith offers such answers as the following: the selection of topics should be guided primarily by the contribution they can make to an understanding of the character of the growth process and the relative importance of the various factors which retard or accelerate it; these findings should be expressed wherever possible in the form of measurable relations; the comparative study should be world-wide, free from dependence on Western industrialized capitalistic societies and the experience of the last century; particular attention should be given to a systematic and statistically founded study of the early stages in the development of countries that are now advanced; the statistical techniques should be related mainly to an extensive use of the national accounting approach, with only secondary reliance on formal econometric models and input-output techniques. In view of our fragmentary knowledge of growth, it is understandable that these answers must necessarily be simply suggestive. Indeed, the report lists more than a hundred substantive problems regarded as directly relevant and important.

The selection of an order of priorities within such an overwhelming catalog is but one basic problem confronting those making comparative studies of growth. Another is that, although comparisons can best be made in terms of a typology of countries and phases of development, there cannot be a "correct" typology: one typology can only be considered as more convenient than another, depending on the choice of problems. It is therefore a somewhat meaningless exercise for Goldsmith to offer a "master list" of 20 countries, although he would justify it by his desire to have comparative studies cover as much as possible of the world's population or income, together with an "adequate" representation of countries situated in different parts of the world, and of countries having different levels of real income per head, different dominant branches of the economy, and different economic systems.

We may also wonder just how instructive a comparative study can be if its major emphasis is limited to the economic aspects and the quantitative dimensions of growth. For the sake of efficiency in research, Goldsmith suggests that comparative studies start with the economic aspects and leave the integration with noneconomic factors to the future. This, however, presupposes that the economic and noneconomic aspects can be examined in simply a serial or additive manner—a dubious proposition. An interdisciplinary ap-

proach is also necessary to modify the economic analysis itself. Although Western economics is not irrelevant for poor countries, its application to particular countries does require amendment according to the institutional environment and specific social and cultural factors. Moreover, some of these ancillary disciplines have used the comparative approach much more than has economics; recourse to the knowledge they have accumulated would strengthen the methods and results of comparative economic studies.

The emphasis on the quantitative approach may also be unduly narrow. As Cairncross suggests, its adoption "necessarily introduces a bias to the analysis of economic growth, since it concentrates attention on what can be and has been measured, while other changes that may be of more fundamental importance are slurred over, and the causal interconnections that such research aims to establish may not emerge at all" (p. 106). In their memoranda, Hagen, Hirschman, and Hoselitz also support this view.

The diverse viewpoints expressed in the several memoranda defy any general summary. These are, however, strong individual statements, and a reader may accordingly find them more provocative than the broader survey in Part I.

Finally, the proposed organization of a center for comparative quantitative study of economic growth will be welcomed by the student of growth (unless he is already immune to the establishment of yet another center). Such a center could perform the valuable functions of systematically collecting comprehensive data, testing hypotheses, integrating the comparative studies of individual researchers, and training specialists in comparative quantitative studies on an international scale.

It is to be hoped that this report will generate considerable response. For despite the continuous flow of country studies, there is still a dearth of systematic and thorough comparative studies. Serious attention to the pertinent suggestions offered here could do much to overcome this deficiency.

GERALD M. MEIER

Wesleyan University

An Econometric Model of Postwar State Industrial Development. By WILBUR R. THOMPSON and JOHN M. MATTILA. Detroit: Wayne State University Press, 1959. Pp. viii, 116. \$3.00.

This short volume reports the results of a 48-state multiple correlation and regression analysis of employment growth for the period 1947-1954 in 20 manufacturing industry groups taken from the two-digit classification of the Census of Manufactures.

Thompson and Mattila begin by computing simple correlation coefficients between annual average change in total employment and 13 "independent" variables representing 5 classes of factors which might be expected to influence interstate differentials in employment growth: prior growth of the local market, prior industrialization, state and local taxes, labor market characteristics, and educational levels and facilities.

They find that those variables chosen to represent the prior growth of the local market—state population and income changes (1940-1947)—show a

fairly high degree of association with subsequent employment growth. Any implications of causality are, however, unwarranted. This is particularly true since the degree of association is highest for such industries as transportation equipment, instruments, fabricated metal products, and paper and allied products, which, with the possible exception of some branches of the fabricated metal products group, can hardly be expected to depend on the local market for their growth.

The second group of variables—average annual change in employment in all manufacturing 1939-1947, expenditures for new plant and equipment, and average annual number of patents issued to new residents, 1946-1948—are chosen to represent the prior degree of state industrialization. Of these variables, the correlation coefficients are generally statistically significant and of the expected sign (but exceed .8 in only two cases) for the durable-goods industries, but for only paper and allied products among the nondurable-goods industries.

The variables chosen to represent state and local taxes do not show significant association with employment growth in any industry.

Labor-market characteristics are represented by average annual earnings in manufacturing, 1949, and by estimated trade union membership as a per cent of nonagricultural employment, 1947. The only significant associations of the expected sign occur in the apparel industry, in which employment growth correlates negatively with both labor-market variables used. The authors recognize that they might better have included a productivity index among their labor-market variables.

The final variables relate to educational levels and facilities. The average level of education, represented by median school years completed by persons 25-years old and older, 1950, shows significant association with employment growth in only the lumber and wood products industry—a most surprising result if given a causal interpretation. The two college variables—number of college graduates, 1950, and total staffs of institutions of higher learning, 1947-1948—show significant association with employment growth in the durable-goods industries and in paper and allied products.

The bivariate analysis is followed by a derivation of multivariate employment-estimating equations for the 20 industry groups. The two variables which show the closest bivariate association with employment growth in each group are combined into multiple regression equations. Because of the high degree of intercorrelation between the variables which show a fairly high degree of association with employment growth in the bivariate analysis, the coefficients of multiple correlation for the estimating equations are appreciably higher than the simple correlation coefficient for the best single explanatory variable only in scattered instances.

The authors supplement their study of average annual change in total employment with a parallel analysis of the rate of growth of employment. The results of this second foray are only marginally different from those of the first.

How well does this volume fulfill the authors' stated purpose: "to learn

more . . . about the basic causes of the growth and decline of state economies" (p. v.)? The major positive result of the study is that employment growth in the durable-goods industries is associated with prior industrialization (i.e., that agglomerative factors are at work in these industries), and that it has taken place in the richer states which had a relatively large number of college graduates at the beginning of the period. Apart from this result, the study offers few plausible generalizations.

To the reviewer, the meagerness of the findings suggests that the model is overly aggregative. The findings probably would have been far richer had separate models been formulated for that portion of manufacturing which serves the national market, and for that portion which is more purely residentiary. Further, the explanatory variables used do not include productivity-adjusted cost data. Such data are difficult to obtain and their use would probably require separate models for each of the nonresidentiary industries. However, it is likely that satisfactory results would have been achieved through their use.

In their preface, Thompson and Mattila tell the reader that what they offer is "primarily a bit of *our* education as grist for *your* educated guesses." The regional specialist who keeps its tentative nature in mind will benefit from a close examination of this work.

WILLIAM WOLMAN

Washington State University

The Social Evolution of Industrial Britain: A Study in the Growth of our Industrial Society. By L. G. JOHNSON. Liverpool: Liverpool University Press, 1959. Pp. viii, 178. 25 s.

Dr. Johnson, senior lecturer in economics in the University of Leeds, has written a particularly timely study. In a period characterized by the wave-like spread of the industrial revolution to new areas, attention has recently been focused on the technological achievements of the past two centuries, because of the interest in accelerating the "take-off" of the industrial development of the new industrial nations. But this preoccupation should not be permitted to lead us to overlook the pool of social and political experience which has accompanied industrial development.

This thoroughly documented essay on the economic and social influences and the politico-economic views which played their part in the integration of the British working class into British society is a basic companion piece to the recently well-publicized researches of W. W. Rostow into the stages of industrial growth. Rostow's analysis of the patterns of industrial development makes due allowance for the role of nationalism, in all its cultural ramifications, in determining the crucial choices which are made as industrialization reaches "the dangerous stage of maturity." The choices of mass consumption or welfare arrangements are viable, but the third alternative, that of the pursuit of external power and influence, requires redirection in our modern world. The position occupied by the working class in an industrial economy is a basic determinant of these choices.

Johnson's study serves to explain one of the Marxian misjudgments, noted by Rostow, that the working class accepted the framework of private capitalism when it was combined with political democracy. His study treats historically of the manner in which working class integration was actually achieved, the profound causative economic changes, and the prevalent views among statesmen, economists, idealists, spiritual leaders, statisticians and working class leaders in regard to the proper form of integration. What emerges is the significance of the working class not merely to the economic development of Britain, but also to its political and cultural development.

Working class integration into British society was the product of a complex of factors. These included the British genius for accommodating to heterodoxy, the wealth of the free enterprise economy and the rise in real wages, and the democratic experience gained by workers in their private self-help organizations, including the cooperatives and the unions. In the first stages of demographic and technological revolution at the start of the last century, the peopling of the new industrial centers was accomplished by the workers themselves. Although uprooted from native habitats, they remained in their own country. They carried with them the British traditions of Parliament as an "institution of community," of an interest in education despite its limits, and of the community spirit to which the religious evangelical movements had contributed. They cooperated and they made contributions to technological change, accommodating themselves to the new environment rather than seeking a recreation of society as contemplated by Owenite communism. The community spirit was reflected in the growth of savings banks and mutual benefit societies, in which experience was gained for the later organization of cooperative organizations and trade unions.

The expanding influence of the working class through trade union organization was formally recognized with the enfranchisement of the workers and the trade union legislation of the 'sixties and 'seventies. Conservatives and liberals vied for the political support of the workers, thus hoping to achieve social harmony. Others decried the divisive influences of trade unions, functioning as restrictive combinations, violative of the common law. The trade unions conformed to neither view, as Johnson points out. They served to make explicit the distinction between capital and labor, rather than serving as agents of social unity. On the other hand, rather than plunging society headlong into anarchy, they contributed to the increasing vigor of a society which was moving "to a new position of equilibrium."

The independent political role taken by the labor movement at the turn of the century was the product of many influences. There was an abiding concern with the state of the working class among all classes of British society. The "problem of poverty" and "the state of the people" were matters of widespread concern. Fabian socialism, with its perspectives rooted in working class efforts through trade unions and cooperatives, held Parliament to be an institution which represented the interests of the entire community. The strains of Utopian socialism also continued to have their influence for, in the author's view, they were rooted in the community sense of working people, with their zest for the cultural and spiritual interests in life, as well as the material. It is

political and religious freedom with its attendant pluralism and community spirit to which the author attributes the direction which the British social fusion has taken.

Johnson's book is not only a tract for the times for the newly emerging industrial economies. It is also an appeal for understanding within Britain itself. As he points out, the wartime dislocation of the British economy placed the country virtually in the position of an underdeveloped country with regard to future economic orientation. Trade union leaders had to work together with management and politicians to raise levels of production in key industries. In assessing the economic prospects and problems which confront Britain, the author cautions against the tendency to the shortening of memory which comes with the apparently incessant changes accompanying industrialization. His is an appeal to the realization of the role of political democracy and community identification in meeting the new technology, the requirements of productive efficiency in the present intensified competitive world situation and in the possibility of reorientation in collective bargaining. The British experience cannot be recapitulated in detail elsewhere; but the connection between democracy and the integration of the working class into society does have universal applicability. It is a major assurance that the viable choices will be made by the mature economy.

JOSEPH P. GOLDBERG

Washington, D.C.

De Caboul à Pékin: rythmes et perspectives d'expansion économique. By GILBERT ÉTIENNE. Geneva: E. Droz; Paris: Minard, 1959. Pp. 268. 20.—sw. fr.

The author's aims in this book are to describe the differences characterizing nine Asian economies (Afghanistan, Thailand, Cambodia, Malaya, Ceylon, Pakistan, Indonesia, India and China), and to compare and assess their efforts to maintain or raise levels of living in the past decade. "Level of living" is viewed mainly in terms of per capita food consumption. Consequently, both the data and discussion emphasize population and food production ratios, time series, and growth rates. Though reference is occasionally made to nonfood crops, as well as to industry, transport and services, the author often seems to look at the population-food relationship in terms of closed economies, and hence to overlook the relevance of specialization and international trade. If the rate of population growth exceeds that of domestic food production, it doesn't necessarily follow that investment and technical assistance should be concentrated in agriculture. Though this inference may in fact be warranted in several of the countries Étienne visited and studied, the relevant considerations entail comparative costs and prospective terms of trade, not simply demographic trends.

The descriptive material used by Étienne is accurately and conveniently arranged to facilitate a quick overview of each of the countries covered. There is, however, very little that is new in his account, and one can find more complete data in the U.N. Demographic and Statistical Yearbooks, and the annual Economic Survey of the Economic Commission for Asia and the Far

East. Still, there are some surprises; for example, the natural rate of population increase in Malaya is 3.4 per cent and in Ceylon 2.8 per cent; and the Chinese have contrived various "anti-Malthusian" arguments for regulating population based mainly on improving the health and productivity of mothers, and increasing their annual working days! Readers interested in a readily intelligible survey of some of the main facts and problems concerning economic development in the Asian area will find the book worth a rapid reading.

The first third of the book deals with Afghanistan, Thailand, Cambodia, Malaya, Ceylon, Pakistan and Indonesia. Étienne groups the first four together because of their relatively favorable progress and prospects for reaching or maintaining a "tolerable" standard of living, which he defines as a daily per capita consumption of 2,000 calories and an annual per capita income of \$100 dollars [*sic*]. Ceylon is discussed separately as "a more difficult case," because of its high rate of population growth and its resource limitations. Pakistan and Indonesia, "where problems become more complex," are also viewed rather pessimistically; Pakistan, because of soil depletion, resource limitations, and its geographic split; Indonesia, largely because of administrative and political shortcomings.

The remaining two-thirds of the book is devoted to a description and comparison of the development plans and progress of India and Communist China. Up through the end of 1957, Étienne finds India's growth slower than China's, but not uniformly so. In machine-tool and textile production, for example, India's percentage growth rates exceeded China's, and in basic food production India's percentage growth in per capita output slightly exceeded China's. But with the "great leap" of 1958 and 1959, China apparently moved ahead by a substantial margin in all fields. Although Étienne doesn't provide better data than have been available before, his interpretation of the official data and their significance seems well balanced. Without presuming the data to be entirely accurate, he stresses that the relevant issue isn't whether they are off by 10, 20, or 30 per cent, but rather how, regardless of the reasonable corrections that might be made, the growth that remains has been so large relative to India's. In seeking answers he stresses China's success in mobilizing its masses for capital-creating uses, its concentration on raising agricultural yields rather than attempting to increase cultivated area, its decentralization of industry and its encouragement of sharply differing technologies in a given industry to take advantage of dispersed resources and varying labor costs and skills. Even though the disenchantment with backyard furnaces occurred after his book went to press, Étienne is probably right in suggesting that some of China's methods of accelerating growth may be relevant in noncommunist countries as well.

Étienne concludes on a hortatory note concerning the need for the non-communist countries in the Asian area to step up their own development efforts, to profit from China's example in so far as it is relevant, and to obtain more external assistance from the West, though he doesn't find ground for optimism on any of these counts.

CHARLES WOLF, JR.

The RAND Corporation

The Welfare State in New Zealand. By J. B. CONDLIFFE. London: Allen & Unwin, 1959; New York: Macmillan, distributor. Pp. 396. 35 s.

J. B. Condliffe is one of New Zealand's most visible exports. He left New Zealand about 30 years ago, after a period as professor of economics at Canterbury University College (now the University of Canterbury) and has since served with distinction in the United States and in international economic organizations.

His book, *New Zealand in the Making* published in 1930, has long been regarded as the standard economic history of New Zealand, and it was with great pleasure that New Zealanders heard that he intended to revise this work and to extend it to cover the important economic and social events which have occurred during the past three decades.

In the event, he has produced two new books—firstly, a revised edition of *New Zealand in the Making*, which concentrates primarily on the economic history of New Zealand up to 1935, and secondly, the book under review, which is mainly concerned with the story of the new period of social and economic experiment which began with the advent of a Labour Government in the mid-thirties. It is incumbent upon the reviewer to point out, however, that important aspects of the more recent period, such as the adaptation of the Maori people to economic and social change, and the growth of certain industries, are covered in *New Zealand in the Making*, while important features of the earlier period, such as the earlier history of borrowing for development or of the regulation of wages by the state, are dealt with in the book under review. Thus, the reader who wants a reasonably complete picture of developments in either period must necessarily consult both works.

The Welfare State in New Zealand can be recommended as providing a very fair and comprehensive assessment of New Zealand's economic and social history during the past twenty-five years. After the first two chapters which deal with the impact of depression and the "insulation" policies of the Labour Government which was in office between 1935 and 1949, the book takes the form, not of a connected historical narrative, but of a series of essays on the structure of the economy, borrowing for development, the economic functions of government, the regulation of wages, social welfare policies and New Zealand's role in the world. This method of approach may make it difficult for the overseas reader to obtain a clear picture of the background against which changes in policy have taken place, especially in the period after 1949. Nevertheless, he will find a useful collection of information, not available in such convenient form elsewhere, on the subjects mentioned above, together with an objective assessment of the policies followed by successive governments during the period.

It is, of course, difficult for the writer of a history book to satisfy all of his readers with the relative emphasis which he gives to various topics. In future editions, I would myself welcome a fuller analysis of the effects of the measures taken to try to stabilize the incomes of exporters, measures which are rather sketchily analyzed in Chapters 2 and 3. I find the treatment of state regulation of wages inadequate because it fails to discuss the important system of

varying minimum award rates of wages by General Orders of the Court of Arbitration. And, in the last chapter, on New Zealand in the World, it is surprising that so much emphasis is placed on New Zealand's relations with its island dependencies and with Asia, and so little space given to trade relations with Britain, Continental Europe and the United States, to the preferences accorded to and received from the United Kingdom, and to New Zealand's role in international economic organizations, particularly GATT.

However, despite these limitations, it can confidently be said that there is no better book available on New Zealand's recent economic and social history.

F. W. HOLMES

Victoria University of Wellington, New Zealand

Portrait of a Modern Mixed Economy: New Zealand. By C. WESTSTRATE. Wellington: New Zealand University Press, 1959. Pp. x, 310. \$4.75.

The author calls his book a Portrait of the New Zealand economy, but its style and organization might better be described by the word Anatomy. Professor Weststrate, who came to the University of New Zealand from a chair at Leyden, defines his approach as "morphological, or rather characterological," deriving from the work of Sombart and Eucken. What he does is to examine each of the main features of the economy and to ask an economist's questions as to the effectiveness with which it functions. After analyzing the mixture of collective and individualistic elements to which its title refers, the book discusses in turn the purposes of public policy, the continuing dependence on external trade, and the rate of economic growth.

"Far from being a simple agrarian economy," New Zealand "is a modern mixed economy with all its complexities. It shows communal control of economic life in many forms. It has an important public-enterprise sector," most of which antedates the Labour Party and owes its origin more to opportunism than to dogma. "The corporate elements" in the economy "are numerous and some of them are influential." By this term, which may startle his New Zealand readers, Professor Weststrate refers to the marketing boards for primary products which exercise governmental authority but are largely operated by representatives of the organized producers. "Communal care of individual welfare . . . is well developed"; the "wings" of the welfare state have a wide span and give remarkably full cover. Finally, the system of compulsory arbitration represents "an unusual element of central direction."

Morphology without genetics makes hard reading. Many students will prefer to learn about the welfare programs in an account that traces their origins to two periods of national emergency and about the marketing boards in an account which places the battles over their formation in their social and political setting. These they may find in J. B. Condliffe's book on *The Welfare State in New Zealand*, and many will also feel the need of the background of development provided by the same author's *New Zealand in the Making*.¹

¹ J. B. Condliffe, *The Welfare State in New Zealand* and *New Zealand in the Making*, London, 1959. The second of these volumes is a substantial revision of a book first published in 1936.

Professor Weststrate's method gives no opportunity to examine the degree to which the special characteristics of New Zealand derive from its origins as a "new" country and from the circumstances under which the small man's industry of dairying came to overshadow the large man's industry of wool-growing.

On the other hand, students of comparative economic organization will find particular value in a book specifically planned to permit international comparison. The author looks at the New Zealand scene with the eyes of a scholar whose principal training and experience have been obtained elsewhere. A native of the country would probably not have called attention to the almost complete absence of anti-trust legislation or related the marketing agencies to Portuguese and Dutch corporatism. Some of the specific comparisons are of considerable interest. According to the estimates presented, New Zealand is second to the United Kingdom both in the proportion of gross national income devoted to welfare measures, as compared with the Netherlands, Sweden and Australia, and in equality of income distribution, as compared with Sweden, Denmark, the United States and the Netherlands. Economists will respect the persistence with which the author presses the question of the economic consequences of the measures examined, even though the answers are often admittedly inconclusive. They will appreciate also his demonstration that the purposes embodied in public action—such as economic development, full employment, stability of agrarian income, greater equality, and "some insulation from disturbances from overseas"—are not in all cases fully compatible. Some at least of these incompatibilities are, as he suggests, characteristic not only of New Zealand but "more generally of the modern mixed economy."

CARTER GOODRICH

Columbia University

New Forces in American Business: An Analysis of the Economic Outlook for the '60s. By DEXTER M. KEEZER and Associates. New York: McGraw-Hill Book Co., 1959. Pp. 278.

Will the American economy sustain through the next ten years the "remarkable record of both growth and stability" it has experienced since 1946? Mr. Keezer and his associates of the McGraw-Hill Department of Economics give a confident, affirmative answer. They foresee "relatively steady growth and prosperity" over the next decade as a result of institutional changes which make the economy basically different from what it was before the second world war. The argument behind this unequivocally stated forecast is clearly and forcefully outlined in this well-written book addressed to both businessmen and professional economists. The latter, however, will find the book more useful as something to recommend to laymen and undergraduates than as a source of new or deeper insights into the workings of an awesomely complex economy.

The only major qualification to the prognosis is that although the general condition will be one of growth and prosperity over the decade, the actual

time-path will be somewhat irregular, and, say the authors with equanimity, short periods ("months") of unemployment amounting to 6 or 7 per cent of the labor force are likely. Technological bottlenecks will be as important here as dips in general business conditions. For recessions of this magnitude the authors would rely on automatic stabilizers and the inherent recuperative powers of the economy—and on generous unemployment compensation. More in the nature of assumptions than qualifications are the views expressed on the prospects of a major war (none expected), shortages of basic resources (none before 1970), the intrinsic social value of economic prosperity (beyond the scope of their book), and the "prospective capacity of corporate management to do its job" effectively (not a problem). Readers will vary in their judgments as to how these premises affect the authors' conclusions.

Unlike most optimistic forecasters, the authors take pains to state and answer the arguments of those who are fearful about the continued good health of the U.S. economy. Specifically, they reassure those who fear the following: (1) a depression brought about by a relaxing of international tension (the rebuttal: rising, not falling, defense spending is much more likely); (2) runaway inflation caused by excessive wage demands of organized labor or greedy exercise of monopoly power by large firms (ample productive capacity, increased competition, rising output per man-hour, public resistance and disapproval); (3) inflation, or maybe recession, induced by chronic agricultural surpluses (the probability that direct subsidies will replace supported prices in the one case, and the magnitude of the problem relative to the magnitude of a growing, prosperous economy in the other); (4) disaster due to irresponsible federal fiscal policies, either deficit-generated inflation (monetary and credit controls) or tax strangulation (greater efficiency in defense spending, public and official awareness of the need to reduce taxes, the growing tax base); or (5) that the state of economic knowledge is inadequate for the job of keeping our complex, \$500 billion economy on an even keel (no problem).

But the authors reserve their big guns for the problem of maintaining over a long period a rate of investment sufficient to support full employment. They agree with those who maintain that adequate investment is the essential condition for achieving both short-run stability and sustained growth. But whereas many economists might then argue that a high level of investment leading to expanded productive capacity (and higher levels of production, income, and consumption) will eventually slacken and fall because the stock of capital will have outdistanced population and consumption and thus further investment will become unprofitable, Keezer and his associates are confident that structural changes in the U.S. economy in the past decade or two have made it not only possible but likely that the levels and rates of change in private investment (and hence in productive capacity), aggregate demand, and the supply of labor will be such that reasonably continuous prosperity will prevail for at least the next ten years. The analysis behind this conclusion considers these structural changes according to the familiar categories: private investment, consumer expenditures, government demand, and export demand.

In addition, the authors place great emphasis on the role of industrial research, "the key to growth and stability."

Three major forces are identified as promising to sustain the rate of growth of private investment: fewer hours of labor relative to population as the latter expands; a "formidable backlog" of overage and antiquated productive facilities; and the greater importance of industrial research and the subsequent innovations. This ever-growing industrial capacity will be supported, the authors think, by a growing population, consumer commitments to high levels of spending relative to income, and the development to a fine degree of the "arts of marketing" which can be used "to increase the inclination to spend."

Although this reviewer agrees generally with the optimistic view of Keezer and his associates, he cannot help but feel that they have seriously underestimated the problem of purely technical maladjustments in, particularly, the investment sector and overestimated the willingness of consumers to spend, year in and year out, at the necessary rates. Even granting the high state of the marketing arts to which they refer, one suspects that the heavy artillery of fiscal policy may well be needed in order to achieve the kind of stability and steady growth the authors optimistically predict. It is also possible that the increasingly glaring gap which they anticipate between the level and quality of private and social goods may act as an obstacle to the rate of growth they envisage so confidently.

JAMES E. HOWELL

Stanford University

American Economic History. By DONALD L. KEMMERER and C. CLYDE JONES.
New York: McGraw-Hill Book Co., 1959. Pp. xv, 680. \$7.75.

This new text has been designed primarily for use in lower-level courses. Its 31 chapters are organized around a conventional framework of four eras ending in 1789, 1860, 1914, and 1959, respectively. In each of these periods, except the colonial, a chapter is devoted to each important phase of the economy such as transportation, agriculture, money and banking, labor, and so on. Besides maps, charts, pictures, and tables, the volume contains a valuable annotated bibliography (pp. 612-45) and an index. Jones takes credit for the eight chapters on agriculture, transportation, and labor.

Kemmerer and Jones seek to apply general economic principles to the treatment of history, and point with pride to their general theme: that since 1860 this nation has been confronted with overproduction. Before 1930 the manufacturers and railroads sought to meet the problem by controlling production or organizing the market in one way or another; and with the New Deal the federal government adopted the very same philosophy of production control. Other economic propositions which show up quite consistently are to the effect that tariffs are bad in proportion to their height; that hard money, particularly gold, is inherently superior to paper money because of the danger of paper inflation; and that it is better for a nation to finance itself by taxation, even in time of war. Although the authors carefully state both sides on controversial issues, the reader is left with a general impression that we need a

- return to the economics of the pre-Keynesian era.

In general Kemmerer and Jones appear to have their facts in good order; and they have done an excellent job in marshaling a countless number of items of information. Those with a statistical bent will like the liberal use of index numbers. In this connection I would advise a recasting of the account covering commodity prices between 1800 and 1860 (p. 196). Kemmerer notes that although the currency situation during that period was quite unstable, "the price level did not vary markedly," and he ascribes such price stability to the employment of bimetallism. The difficulty lies partly in the choice of terminal years and partly in the choice of index numbers. The figures he cites are identified with the Snyder-Tucker general index, of which wholesale prices are a minor component, and which has an amplitude of variation only about 50 per cent as great as wholesale commodity price indices. New York wholesale prices rose about 100 per cent between 1790 and 1814, declined over 40 per cent between 1814 and 1821, recovered substantially until 1837, then dropped to a low point in 1843 some 60 per cent below the peak of the War of 1812. And the extent of inflation in the 1830's was considerably greater in the West and South than it was on the Eastern seaboard. This is, of course, a minor degree of instability compared with the hyperinflation of the American Revolution, in the South during the Civil War, or in Germany and Russia after the first world war. Nevertheless, the American price experience in 1790-1860 was comparable with that in the North in 1860-1914, or in the nation as a whole between 1914 and 1959.

Kemmerer gives a masterful account of events during the 1930's and shows a better sense of proportion on "The Great Depression" than any other authors whose works have come to my attention. In his account of events leading up to the bank holiday, however, he gives little if any emphasis to the bank difficulties in Detroit in January 1933, which have always impressed me as a prime factor in generating the tornado which swept the nation later on. Will we ever know exactly what happened in Detroit at that time? For a long time the story was that this was no ordinary run on banks on the part of the general public but rather a question of huge withdrawals on the part of a very few depositors.

In his treatment of the second world war Kemmerer devotes considerable space to military and diplomatic events (pp. 464-67) but leaves a curious void as far as mobilization of the economy for war is concerned. Production simply increased "miraculously," and prices simply failed to rise as rapidly during the war as they did in the years immediately following. I wonder whether we appreciate how much the various wartime controls saved us in lives and resources, and whether we can afford to forget the experience.

Space does not permit listing all the parts of this volume which are extremely well written (such as the chapters on agriculture) or all the items which require revision (such as the apparent confusion between Santo Domingo and Haiti on page 282). For the most part it is a solid book indeed and leaves the reader with a feeling that he has learned more than he expects from a college text.

THOMAS SENIOR BERRY

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Statistical Methods; Econometrics; Social Accounting

State Income Differentials, 1919-1954. By FRANK A. HANNA. Durham, N.C.: Duke University Press, 1959. Pp. xix, 268. \$7.50.

This is the final report of a study of differences in per capita state incomes carried on at Duke and financed in part by grants from the Rockefeller and Ford foundations. The chapters fall into three groups. One deals with relative changes in state incomes over time; the second with differences in the economic activity and income patterns of the various states. A final chapter discusses demographic aspects of state differentials. Two significant appendices deal with problems of statistical analysis encountered in the report—regression and standardization procedures. A third appendix reports new statistical data on per capita farm income and per capita components by states, 1929 to 1954. All this is preceded by an introduction and summary, especially valuable for the reader who is more interested in the findings of the analysis than in the technical aspects of the methods by which the findings are derived.

Chapters 2 and 3 deal with secular and cyclical changes in state income differentials, 1919-1921 and 1929-54. Most striking is the narrowing of interstate income differences over the years, as measured by the coefficient of variation. At the beginning of the period the state coefficients were in the range 37-41 per cent. Since 1954, they have narrowed to 21-24 per cent.

The chapter on cyclical behavior is one of the most interesting and suggestive parts of this report. The author discusses fully and clearly the difficulties of isolating cyclical and secular elements of income behavior during a period marked by the distortions of major depression, inflation, and war. He employs sensitivity and time-factor indices for his analysis. He concludes from his comparison of these two indices that approximately two-thirds of the decrease in interstate dispersion, 1932-34 to 1952-54, is associated with factors related to the sensitivity indexes and about one-third to elements associated with time. Since the chief obstacle to the isolation of cyclical patterns and secular trends in the data is the erratic behavior of farm income, sensitivity and time-factor indexes have been calculated separately for per capita farm and nonfarm incomes. Because of this difficulty and others of a more technical nature it is impracticable to summarize the findings of this chapter with the brevity required of a review. However, the interested reader will find a close study of Hanna's analysis rewarding.

Chapters 5 and 6 deal with differences in occupational and industrial earnings by states. However, this reviewer finds Hanna's statistical analysis questionable and his conclusions puzzling. The statistical problem and the author's approach to it may be described as follows: Average state per capita earnings by occupation are the end-result of a totality of area and interareal economic forces. Among these, three factors are surely important: the occupation pattern, the interoccupational distribution of skills, and the hourly or weekly rate of pay. Given only the crude data, is it possible to isolate the effects of each of these three variables from the total average per capita earnings by statistical methods? The affirmative answer of this report is expressed in this quotation from the summary (p. 17):

A state which has relatively more of its experienced labor force engaged in the higher-paid occupations would be expected to have a higher average earning. To obtain a measure of the state average earning that would be expected if occupation composition were the only difference among states, there has been computed for each state an earning figure which is based on the national rates for each occupation and the State's occupational distribution of its experienced labor force. Since identical occupational earning rates have been used for every state—only the occupational composition of the states has been allowed to vary—the computed earning figure is called the state's rate-constant earning. The difference between two states' rate-constant earnings provides a measure of the difference in their occupational compositions.

The author then raises the question of "how much of the observed interstate earning differential is explained by interstate differences in occupational composition" (p. 139). In attempting to answer the question he makes use of the coefficient of determination as a method of measurement. State rate-constant earnings are taken as the independent variable and state reported earnings as the dependent variable. After computing various coefficients for 1949 state occupational earnings he states that: "The coefficient for all wage and salary workers indicates that the interstate variation in occupation composition accounts for about 87 per cent of the interstate variation in reported earnings" (pp. 141-42).

This statement is enough to startle any state planning officer or regional economist who might wish to use Hanna's findings. The reader not deeply versed in methods of statistical analysis surely will feel that this figure is unreasonably high; and he will be even more puzzled by the author's statement on page 142 that the measure is "wholly statistical" and "contains no implication concerning the existence or nonexistence of any causal relationship between the two series"; as well as by the footnote which states that 10 per cent of the total variation is "explained by independent effects of composition," while 77 per cent is explained by other effects correlated with composition.

The technical objections to Hanna's use of and interpretation of the results of the coefficient of determination already have been voiced by all three discussants of Hanna's original paper on this subject presented before the NBER Conference on Research on Income and Wealth held in June, 1955. (*Regional Income, Studies in Income and Wealth*, Vol. 21, Princeton 1957.) George H. Borts of Brown University objected particularly to the "overestimate of the independent influence of occupation mix on state income differentials." In the reviewer's opinion Hanna's resort to the association of correlated effects with the independent effects of occupational variation is invalid.

Hanna concludes the report with a chapter on demographic factors. He sees such factors as sometimes reflecting occupational and industrial income differentials, and sometimes acting independently to help explain the existence of these income differentials. Five are selected for special study. Three are age groupings; the other two are labor-force participation, and size of the employed labor force. Analysis of the data shows that lower per capita incomes tend to be associated with larger percentages of state population in the younger age groups, and with smaller percentages of population in the labor

force. However, the abstraction of such age and labor force participation factors still leaves a substantial proportion of interstate differentials to be explained by other factors, some of which may be demographic. Hanna suggests several such demographic factors, including time spent in schools, gainful employment of women outside the home, etc.

Finally in deference to the many students of metropolitan regions, Hanna examines the relation of city size to state income differentials. He concludes that other demographic factors are likely to be much more important than city size in explaining income differentials among states, and that city size is not a very useful framework for such analysis.

This book adds to our knowledge of the behavior of state income differentials among the states since 1929, but its contribution is limited because of its emphasis upon statistical explanations of these differences, and because of the author's failure to inquire into the fundamental economic and institutional factors which underlie the data.

MORRIS E. GARNSEY

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Obshchestvennyi produkt i ego struktura pri sotsializme. (Social Product and its Structure under Socialism.) By YA. A. KRONROD. Moscow: Gospolitizdat, 1958. Pp. 554. Rbl. 11.

This book, by a leading member of the Institute of Economics of the USSR Academy of Sciences, is a comprehensive treatise, from the Marxist standpoint, on the structure and dynamics of the national product. Primarily theoretical rather than empirical, it is concerned chiefly with an elaboration of Marxist theory on this subject and its application to a socialist economic system such as that of the USSR. The structure and growth of Soviet national product, however, receive substantial, if secondary, attention. Although much statistical information is presented on the Soviet economy, little of it is new. The book is scholarly both in format and in tone. In contrast to most Soviet economic monographs, it has both a subject index and an index of authors cited. There are many footnote references to other works and to sources of statistics used, although sources are not given for all figures cited. The author's manipulations and adjustments of published data are in most cases fully explained. Although Kronrod frequently asserts the superiority of Soviet socialism over capitalism and of Marxist over "bourgeois" political economy, his approach is expository rather than polemic, and "bourgeois" economists are cited without being denounced.

The book's "investigation of the laws of socialist expanded reproduction of social product and of the development of its economic proportions" consists of seven long chapters. In the first chapter, Kronrod analyzes the Marxist concept of "social product," stressing the meaning of material production and the difference between social product, or total gross material production, and national income, or total net material production. In the next chapter, he examines various aspects of national wealth and various ways of classifying economic activity. Chapter 3 is concerned with an explanation and justification of the "law" of preferential development (i.e., more rapid growth) of the means of production compared with the means of consumption. Chapter 4

deals with the characteristics of industrial and agricultural production and their role in the social product. In Chapter 5 the concepts of constant capital, variable capital, and surplus value used by Marx in his analysis of capitalist production are adapted to the conditions of a socialist society and designated, respectively, the "fund for replacement of fixed and working capital," the "fund of production for oneself," and the "fund of surplus product." The sum of the latter two constitutes net production or national income, which is devoted in part to consumption (personal and collective) and in part to accumulation. Chapter 6 analyzes the two components of the replacement fund, while Chapter 7 deals with the accumulation and consumption "funds."

Most Western students of socialism and of the Soviet economy are likely to find Kronrod's very detailed elaboration of Marxist doctrine—which builds on and extends his earlier work¹ as well as that of other Soviet writers, such as Notkin and Kolganov—of less interest than his remarks on various aspects of Soviet economic development and economic policy.

In his explanation of the need for higher rates of growth of production of producers' than of consumers' goods, Kronrod distinguishes six major reasons for this policy in the USSR: mechanization and automation in industry; introduction of new technology; concentration of production in larger, more specialized plants; investment in production, transportation, and housing and other ancillary facilities in connection with the development of Siberia and Central Asia; development of agriculture through mechanization, electrification, and provision of chemical fertilizers; and production of equipment for consumers' goods industries.

Kronrod is critical of Soviet price policy, pointing out that the shares of the different sectors in aggregate social product are inaccurately represented because of the pricing of producers' goods "below their value." (However, he says, because of "a compensatory deviation of prices above their value" in the case of consumers' goods, the total value of the social product is nevertheless correctly stated.) Among other undesirable consequences of the underpricing of producers' goods are the understatement of materials costs relative to labor costs, "artificial losses" for some producers, and the inability to compare the "effectiveness" of investment in different branches of industry and sectors of the economy.

Kronrod also criticizes Soviet policy on amortization allowances as being unsuited to conditions of rapid technological progress because these allowances are based on original rather than reproduction cost and on expected physical life, without recognition of the likelihood of obsolescence.

In his discussion of capital-output ratios, Kronrod cites with respect the work of Goldsmith, Kuznets, Domar, and others. He concludes that the capital-output ratio in the USSR is declining, primarily because recent technological progress has reduced capital requirements per unit of output and secondarily because of increasing efficiency in the use of the capital stock.

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¹ For example, *Sotsialisticheskoe vosпроизводство* (Socialist Reproduction), Moscow 1955.

Economic Systems; Planning and Reform; Cooperation

Instrumente und Probleme westlicher und sowjetischer Wirtschaftslenkung.

By GÜNTER HEDTKAMP. Giessen: Wilhelm Schmitz, 1958. Pp. 187. DM 14.80.

In this study the author compares the problems and objectives of economic policy-making and implementation in the West with the formulation and execution of the Soviet national plan.

The work, which is well organized, consists of two main parts. Part I presents a brief, yet critical, survey of Western social accounting and of the history, development, and present form of the Soviet national plan. The second part deals with the actual problems of carrying out economic policy in the countries of the West and economic planning in Russia.

It is, of course, the second part which is by far the more interesting. In it, Hedtkamp draws attention to the failure, thus far, of Russian economists to develop a comprehensive theory of central planning. He attempts, therefore, to reconstruct, so to speak, from the existing practical instruments and given preconditions the planning methods actually in use in Russia today. Thus, Hedtkamp finds that "contrary to the Soviet thesis, . . . the production program is more and more determined by private consumption" as average income rises. Consequently he discerns a tendency for more subtle, "indirect" policy measures to replace the fiat of the economic planners. This would amount to some narrowing of the differences between Soviet economic planning and traditional Western policy methods. It would also lend new importance to Russian fiscal and monetary policies.

Hedtkamp's work is based on sufficiently recent material to be able to take account of Khrushchev's decentralization policy, first announced before the Supreme Soviet in 1957. His documentation is impressive: it has obviously been assembled with great thoroughness and includes material published in Germany, Russia, France, and England, and by the United Nations in New York. What one looks for in vain, however, is references to the U.S. literature in this field. Most probably, this omission is not a deliberate one; it seems more likely that it was dictated by the relative accessibility of sources for research. The U.S. scholar who will, in any case, be familiar with the work that has been done in this country will for that reason find the bibliography no less useful.

A word of caution to the prospective U.S. reader of Hedtkamp's book. The language is difficult and interspersed with neologisms which the reader may not be able to find in his dictionary; and the style does not make for particularly easy reading. But then, when it comes to economists who are not masters of literary expression, Hedtkamp is in good company.

The summaries in French and English at the end of the book would be more useful if only the translations were a little better. As it is, it seems unlikely that anyone who has difficulties reading the summary in his own language will take the trouble to attempt reading the entire book in a foreign language.

But these are minor criticisms. The book is competently written, interesting, and well worth reading.

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Systèmes et structures économiques. By ANDRÉ MARCHAL. Paris: Presses Universitaires de France, 1959. Pp. viii, 716. 22.00 NF.

The volume under review is published in *Collection "Thémis"* which specializes in providing textbooks for students working for degrees or certificates under the revised and extended program in the social sciences initiated by the Decree of March 27, 1954. André Marchal, professor in the Faculté de Droit et des Sciences Économiques, University of Paris, has been in charge of the course on Economic Systems and Structures since its inauguration in 1954-1955. It was at first an elective one-semester course for third-year students, but became eventually a yearly course obligatory for all fourth-year students in economics. *Systèmes et structure économiques* is the by-product of Marchal's teaching and is addressed primarily to his students.

The study is divided into four main parts: A general introduction (pp. 1-109) which deals with the notions of structure and system and questions of methodology; Part I (pp. 111-378), Systems and Combinations of Structures; Part II (pp. 379-597), Dynamics of Systems and Plasticity of Structures; and Conclusions (pp. 598-691), The Acceptance (*prise en considération*) of Structures and Systems by Contemporary Economic Theory and Policy. Each of the four main divisions is subdivided into titles (*titres*), chapters, sections, paragraphs and smaller entities designated by letters and Roman numerals—an elaborate and forbidding scheme of organization.

Marchal deplores the estrangement between economic history and economic theory and believes that their integration is attainable by using the notions of structures, systems, and "types of organization," especially as developed in the writings of Johan Åkerman and Walter Eucken. The author has a great deal to say about structures and systems which he examines in their static and dynamic aspects. A structure, in the latter sense, is defined as "elements of an economic entity [*ensemble*] which during a specified period appear as relatively stable while compared with others"; and "when the various structures which make the entity under consideration are mutually 'coherent'—coherence emanating [*naissant*] from the internal forces which unite the structures and also from the common internal and external obstacles to which they oppose a concerted or spontaneously synchronized reaction—we are in presence of a 'system,' while the absence of coherence revealed by observation shows that one is faced not with a 'system' . . . but with a 'regime.'" These and similar concepts and definitions may well appear somewhat labored and arbitrary and are not easy to grasp or use effectively.

Marchal's theoretical generalizations are the weaker part of his study. He quotes numerous European writers on economics and sociology whose names mean little to Anglo-Saxon readers. His reasons for selecting American authorities are not always obvious. He devotes, for instance, several pages to a summary of the classification of countries according to the state of their bal-

ance of payments offered by Boggs (neither his first and middle names, Theodore Harding, nor his initials are given) in a volume, *International Trade Balance*, published in 1922 (in Marchal's bibliography the date of publication is listed as 1929), that is, at a time when the study of the balance of payments as a major public issue was still in its infancy. In the section on business cycles no reference is made to the works of Wesley C. Mitchell and the publications of the National Bureau of Economic Research.

The descriptive part of the book presents much interesting and valuable material on capitalism, corporativism, and collectivism, and their evolution. The section on the contemporary French economy is particularly enlightening and useful. Inevitably Marchal is concerned chiefly with change. His conservative-minded readers, however, will find comfort in Montesquieu's dictum which the author quotes: "Such is the nature of things that wrong-doing [*abus*] is quite often preferable to redress [*correction*], or at least that the good [*le bien*] which exists is always preferable to the better [*au mieux*] which does not."

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Business Fluctuations

American Business Cycles 1865-1897. By RENDIGS FELS. Chapel Hill: University of North Carolina Press, 1959. Pp. vii, 244. \$6.00.

Professor Fels has presented us with a theoretically informed and—within the limits set by the published quantitative data—a carefully documented analytic history of United States business cycles during the last third of the nineteenth century. The successive business cycles during 1865-97 are examined as historical individuals in Chapters 6-11. In Chapter 5 the author deals with the entire period as an historical unit and weighs the evidence concerning the competing "real" and "monetary" explanations offered in the literature for the coexistence of secularly falling prices and a series of unusually severe business depressions. Similarly, in Chapter 12 he treats the submerged business cycle of 1894-97 as part of "The Depression of the Nineties" (1893-97) and enquires into the causes of the latter. The first four chapters of the work are devoted to methodological issues and the elaboration of a theoretical framework for the historical investigations; and a final chapter summarizes the conclusions of the entire study.

The author has approached his topic as a specialist in business cycles, concerned with the light which nineteenth-century experience may cast on the nature of business cycles. Now, a principal feature of the historical approach is that it treats each cycle as a unique individual, and among other things this means giving explicit attention to exogenous factors. On this score, Fels concludes that external events were important but mattered less than internal forces (p. 221). This judgment is heavily qualified for the "depression of the nineties," however, when the silver problem was "the real villain of the piece

- . . . without which the contraction of 1896 would have been mild and the con-

traction of 1893-94 little worse than 1882-85" (p. 218). If this interpretation of the 1890's is correct, it reduces from three to two (1870's and 1930's) the number of observed depressions since the Civil War which *could* have been both deep and prolonged because of the operation of internal forces alone. Moreover, though the contraction of 1873-79 was prolonged and was marked by severe price deflation, it was "singularly mild" in terms of output, and the extent of unemployment was grossly exaggerated in some contemporary reports (pp. 107-08). These are significant findings for anyone who is interested in the frequency and causes of catastrophic depressions and the likelihood of their recurrence.

Historical studies may also be used to test cycle theories. This endeavor occupies much of Fels' attention. He uses Schumpeter (innovations and the process of adaption to them), Gordon (creation and exploitation of investment opportunities), and Hicks (multiplier-accelerator models) as representative examples of important theoretical schools. The respective theories are regarded as essentially complementary, but there is nonetheless an attempt to judge which is the most consistently rewarding as an explanatory hypothesis for the business cycles of the period. The first, second and third prizes are awarded in the order listed above. The issues are complex, and I have far too little space to do justice to Fels' views on these matters. I can do little more than suggest some reservations to be kept in mind by the reader when he is assessing Fels' conclusions.

The low score for Hicks' theory is partly because it is a poor construct for empirical interpretation or testing and partly because it is really several theories. As Fels observes, the distinctive feature of Hicks' theory "... is stress on the acceleration principle. It is exceedingly difficult to get convincing evidence on the acceleration principle, so that the negative results in this instance are not decisive" (p. 224). The theory was found wanting, not because it was necessarily inconsistent with experience but because its basic mechanism did not provide a useful tool for empirical research of the historical variety. On the question of consistency with experience, moreover, Hicks' theory is eclectic and has enough variants to fit almost any case. For example, monetary ceilings were encountered in 1873, 1890, and 1893 even if real ceilings were not, and Hicks has allowed for this possibility. Furthermore, Hicks believes that catastrophic depression is the result of profound monetary instability and would not occur from the real accelerator mechanism alone. In short, whether Hicks' theory is even broadly consistent with experience depends considerably on which of its variants one has in mind.

Fels believes that a modified Schumpeterian theory provides the best single explanation of the cycles of the period 1865-97. Gordon's theory includes innovation as one source of investment opportunities, but Schumpeter places it in the center of the stage and also analyzes the "competing-down" process by which the new displaces the old. Even more attractive to Fels, one suspects, is the unity given to the separate Juglars of the period by the hypothesis that they belong to a single Kondratieff recession—that of the railroad era. But as Fels himself shows in his chapter on the "long-wave depression," that same unity can be imparted as readily by an hypothesis of inadequate growth rates

of autonomous investment opportunities, the money supply, or both. And Fels has introduced no evidence that the distinctive feature of Schumpeterian recession—the competing-down process as the fruits of past innovations and the investment opportunities they induced elsewhere are harvested—really applies to a thirty-year period even in response to a major innovation like railroad transportation. Isard's schema of a twenty-year transport-building cycle makes use of the same central idea and has more statistical support, but twenty-year cycles are neglected in Fels' work.

The author has little to say about the extent to which his "basically Schumpeterian" explanation of the post-Civil War nineteenth century business cycles applies to the present-day economy, and rightly so. It was Schumpeter's own opinion "that the darkest hues of cyclical depressions and most of the facts that make of business cycles a bogey for all classes are not essential to business cycles per se but are due to adventitious circumstances." Even if it were granted that the disturbances introduced by innovations are still the first cause of business cycles, one would have to ask to what extent monetary and financial reforms, modern conceptions of contracyclical policy, and other structural changes may have mitigated those "secondary consequences" which were responsible for the darkest hues of depression. More generally, it is certain that the structure of the economy has changed since the nineteenth century and even since the 1920's—both in a qualitative sense and in terms of the values of some of its basic response parameters—and the vital question is how much these changes may have affected cyclical behavior. Fels has not tried to answer this question, but he has provided a scholarly treatment of his period which will be useful to those who would undertake the task.

BERT G. HICKMAN

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Money, Credit and Banking; Monetary Policy; Consumer Finance; Mortgage Credit

Voprosy valiutnogo kursa rublia. (Problems of the Ruble Exchange Rate.)

By I. AIZENBERG. Moscow: Gosfinizdat, 1958. Pp. 158.

This is the first Soviet book in at least three decades to deal exclusively with the ruble exchange rate. It is also the only book on Soviet foreign trade since before the war to treat the subject in other than purely descriptive, administrative, or "verbalization of tables" terms and which makes some attempt to grapple with economic problems. Not that this book is without faults—it is repetitious and the analysis leaves much to be desired from a Western point of view.

The book begins with a useful history of the development of the present system of exchange control. While foreign trade came largely under state control immediately after the revolution, until 1926 the ruble (*chervonetz*) was a partially convertible currency supported by the State Bank in Western foreign exchange markets. Beginning in 1926, partly as a result of a large trade deficit the previous year, convertibility was ended, the Bank was given

the exclusive right to hold precious metals and foreign currencies and, with a few exceptions, the export and import of rubles were forbidden. At this point, domestic prices became essentially insulated from foreign trade influences and the exchange rate from balance-of-payments influences. Minor regulations promulgated from 1926 through 1931 completed this process.

An important reason for the controls instituted in 1926 was the fact that the ruble was overvalued. The new controls then, of course, facilitated (in a domestically inflationary situation) the development of further overvaluation. The existence of overvaluation is admitted and discussed at length by the author who says that a 32 per cent devaluation (in 1928) would have made Soviet exports profitable again (p. 74). But devaluation was not yet in the cards, and the overvaluation increased between 1928 and 1936 as world prices fell and Soviet internal prices rose (p. 136). These trends led to a change in the exchange rate of the ruble in 1936 to eliminate the (p. 135) "... gap between the level of internal prices of Soviet imports and exports and the prices of these goods on foreign markets. . . ." This is the most candid discussion by a Soviet economist of ruble overvaluation which I have seen. Aizenberg does refuse, however, to admit that the change in the value of the ruble in 1936 can be likened to a "bourgeois devaluation." Bourgeois devaluations, he argues, (1) involve a fall in the standard of living of the workers; (2) lead to a rise in internal prices; and (3) have an impact on the structure of foreign trade (but see below) and balance of payments. Leaving aside the pejoratives, Aizenberg is simply (and correctly) saying, in effect, that under Soviet conditions the foreign exchange ruble is purely an accounting unit and, as such, changes in its rate have no impact on major economic variables. The reason given for the devaluation is the same as that usually offered for the elimination of subsidies: preservation of managerial incentives. This is the one economic variable which is affected by an overvalued currency. With an overvalued currency, exports are sold by foreign trade organizations at a large loss relative to domestic cost, thereby necessitating regular subsidies; and imports are bought at a large profit. Under these circumstances, it is difficult to keep managers of foreign trade organizations striving for efficiency and profits. Hence the need for devaluation.

Aizenberg does not deal quite so candidly with the 32 per cent revaluation of 1950. He argues that revaluation was required by the fall in Soviet internal prices from 1947 to 1950 (p. 145). In fact, only consumers' goods prices fell in this period and these goods were not a large part of foreign trade turnover. Despite an 18 per cent drop in 1950, for the whole period 1947-50 producers' goods prices roughly doubled. It can be demonstrated that at the time of the revaluation, Soviet internal prices of both consumers' and producers' goods were higher than world prices and that the revaluation increased the degree of overvaluation.¹ If anything, devaluation was in order in 1950.

In his final summing up, Aizenberg argues that two ruble exchange rates are presently necessary: one for tourists and embassy representatives which equates consumers' goods prices with those of other nations; the other for

¹ My own estimate is that internal producers' goods prices were 40 per cent higher than export unit values in 1950, before revaluation and after the internal 1950 price cut.

producers' goods and raw materials. (This is, in fact, the case.) He admits, contrary to an earlier opinion cited above, that an appropriate exchange rate has considerable significance in planning the volume and structure of foreign trade turnover in the USSR and in defining the "economic effectiveness" of exports and imports of different commodities. My feeling is that this admission is unnecessary, or at least overstated. If Soviet internal prices were "rational" and only the exchange rate were out of line, it would be a relatively simple matter for the planners to appropriately discount the overvaluation of the exchange rate. It is the general irrationality of relative prices which primarily complicates the rational planning of Soviet foreign trade.

Most of the book is concerned in one way or another with the above issues. There is also discussion of other matters, however, such as the balance of payments and the interrelationship between Soviet prices and the value of gold. Finally, Aizenberg presents some percentage figures which enable us to calculate firm figures of 3.9 and 4.5 million fine ounces, respectively, for Soviet gold production in 1934 and 1935.

FRANKLYN D. HOLZMAN

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International Economics

Essays in World Economics. By J. R. HICKS. Oxford, Eng.: Clarendon Press, 1959. Pp. 274. \$4.20.

It comes as something of a surprise to one who has been observing the American economy carefully for the past decade or so to learn that the United States and Canada were characterized during this period by "the combination of rising wages with stable prices." To John R. Hicks, whose opinion we have come to respect in so many areas, this situation was "in itself a thing that is extremely pleasing."

Our confidence in his judgment does get something of a blow from his statement that this stability was not the result of keeping the average level of prices stable "while particular prices would be changing, some rising, some falling, in such a way as to keep demands and supplies in a moving equilibrium." What is more, it is not probably true that our idyllic situation was due to the "choice . . . and the exercise of its powers by the monetary authority, with such perfect skill that it got its objective exactly right."

What actually was happening in the North American economies was a steady rise in productivity and a wage drift. The rise in wages did not communicate itself to higher prices. In fact, says Hicks in his essay entitled "A World Inflation?", a rise in wages under these conditions "does not in itself push costs up, but it does (in large measure) prevent them from falling." In due time, of course, when the wages in some, at least, of the industries which have not shared in the productivity gains are brought into line "prices must rise." But this is yet to come.

This extraordinary estimate of North American experience deserves at least a respectful hearing. Part of our own confusion on the subject of inflation—

and if you can think of a better word than confusion, please supply it here—may be traced to the obvious fact that there was political fat in the fire. Although it shouldn't have mattered to the analysis, the position the observer took with respect to the nature of the dangers of inflation determined how he viewed the facts. It is possible that there never really was an inflation during this period at all. That this estimate knocks all the arguments over demand-pull and cost-push and monetary policy into a cocked hat is something of a shame, to be sure, but here is a chance to see ourselves as others see us.

The United States and Canada belong, according to Hicks, in category A, "countries with a moderate price rise." In category B, where the United Kingdom is to be found, are those nations which have known a mild postwar inflation. To explain their situation, Hicks unlimbers the best of his artillery of analysis.

Demand-pull, he feels, does not give a complete picture of the British situation, though both for Britain and Germany demand has been excessive in the 1950's "whether that excess demand has arisen from the profitability of exports or from some other cause." Rather it is to the labor market that we must look to see the complete picture. In the modern economy, this market is crucial to an understanding of the adjustment process.

The cost-push of labor, however, can not be interpreted properly without a clearer view of the cost-of-living reaction, which is in turn traceable—in significant degree—to the cost of imported consumer goods. Working in this way, the cost-of-living mechanism "tends to move internal prices into line with external prices, but it does not, in itself, push them above."

"The general characteristic" of the B countries' experience "is that there has been a rise in their prices, relatively to those in the A countries; and yet they have been able to maintain their exchange rates." This, to be sure, is inflation, but by no means "scandalous"; for this inflation is "really a special problem of post-war recovery spun out into the years of apparent normality."

All this reasoning is not only cast in terms of the European context—more particularly British—but also in the context of what Hicks calls the labor standard, which, in his view, has since the 1930's replaced the gold standard. In his essay "Economic Foundations of Wage Policy" he tries to make it quite clear that he regards "the Labour Standard as an unquestionable benefit" but there are "a number of defects" among which is the fact that "while the Gold Standard was an international standard, the Labour Standard is national." Thus, it follows that the value of money in terms of wages undergoes "no deliberate determination. It is a mere by-product of the wage fixing process."

But in this wage-fixing process, unlike the view taken of it by many authorities, it is not the cost of living—at least operating internally—which need cause any rise in wages. These rises are traceable directly to rises in productivity. Nor can we, except temporarily, seek a determining cause in the structure of wage differentials though these may play some part in scarcity of labor here and there. Nor finally can we find the recent wage rises to be traceable to profits. In short, "the continual rise in money wages since 1945

is not a necessary consequence of Full Employment (or of the Labour Standard)."

The external influences, proceeding as they do from changing rates of productivity particularly in North America, however, are vital to an understanding of the world inflationary process. The rate of economic growth has been so much faster in the United States and the dollar area in general than in the nondollar area that we have tended to overlook the fact that a growing bias is being built into the imports and exports of the dollar area.

The key is the slowdown in the rate of growth of agricultural productivity—or what is the same thing from an international point of view the failure to pass on the results of such improvements because of the operation of farm price-support policies—which has made the dollar area less able to export its traditional wheat, cotton, and so on. This has tended to force wage-goods costs up in the nondollar area and transmit inflation to the B countries.

It is to be observed however that those nations most closely tied to the dollar countries—e.g., Latin America—have suffered the most inflation in the postwar period. Part of this "must no doubt be attributed to the unfortunate system of multiple exchange rates" but to a greater extent the blame must be found in the peculiarities of the dollar market. Put directly, terms of trade within this market have been turning sharply against agriculture and primary products in general both within and outside the United States—see the falling parity index as an evidence of this—but overseas producers have not had available the largess of a Congress willing to stockpile agricultural surpluses to fall back on. On the other hand, they had been led by a series of very good years right after the war to make spending plans in anticipations of more normal times. "Such anticipations were asking to be cheated . . . and though the crisis was not *caused* by a fall in export earnings, its consequences were only too likely to be the same as those which had traditionally occurred in that event."

Thus in calm and extremely finely spun analysis, Hicks presents a case which emerges as one of the most powerful condemnations of our national economic policies that has yet appeared. He would be the first to deny that this was the purpose of this volume on world economics, but I think he would insist, as he says in another connection in this volume, "the analysis which emerges does not sound to me so unrealistic. It sounds to me like ringing true."

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International Financial Transactions and Business Cycles. By OSKAR MORGENTERN. National Bureau of Economic Research Study. Princeton: Princeton University Press, 1959. Pp. xxvi, 591. \$12.00.

There are some books that cannot be reviewed adequately after one reading, however careful. Professor Morgenstern's *International Financial Transactions and Business Cycles* is obviously in this category. The wealth of material, the new insights into the international financial system, particularly as it

- operated in the forty years before the first world war, and the many questions

raised are all worthy of repeated study. It will surely be years before students and practitioners in international finance catch up with the full implications of this book; though it would be most unfortunate if the views which have been and still are current on the behavior of interest rates, long-term as well as short-term, the interrelations between interest rates in major money and capital markets, the impact on international financial operations of exchange rate variations and their relation to interest rate developments were not to be reconsidered in the light of Morgenstern's penetrating analysis. Interest rates and exchange rates and their interdependence are the relatively narrow focal point of his investigation, reflecting the importance he attaches to monetary phenomena in the international aspects of business cycles but also reflecting the importance he attaches to careful investigation. A broader focal point would necessitate a less rigorous analysis.

The progress recently made by the Western European countries in establishing currency convertibility means that the questions posed by Morgenstern have great current importance for policy formation. An international financial system has again been established in which major money markets, particularly London and New York, are also major foreign exchange markets where all the forces affecting the world demand and supply of various currencies are felt. Demand and supply in these markets is still importantly affected by governmental restrictions on trade and financial transactions; but to a rather startling degree and in a manner unexpected even to some who have followed these matters very closely during recent years, the international financial system now encompasses and reflects the actions of individuals and firms rather than of governments. Freedom to move funds, together with the general conviction that this freedom will continue and that the exchange rates (or par values agreed upon with the International Monetary Fund) will remain stable within narrow limits, means that we are again in a world where interest rate differentials, variations in income-earning possibilities in various countries, small relative changes in exchange rates, subtle changes in expectations in these fields, are of prime importance.

Morgenstern's analysis sets forth how these factors operated before 1914 and between the two world wars and attempts to give some explanation for the differences between the observed behavior and what might have been theoretically expected. He does not deal with the conditions following the second world war. When the book was published, it might have been regarded as essentially of historical interest with important theoretical implications. The change in international financial relations in recent years makes it germane to any analysis of present and future developments and policies affecting the existing international monetary system.

The international monetary system into which we are now moving, with its emphasis on freedom from current restrictions and its internationally agreed par values, made feasible by international collaboration, is centered in the International Monetary Fund. This system draws much of its inspiration from the experience with the international gold standard. It is an attempt to get the benefits of that system in a way compatible with the purposes set forth in Article I of the Fund Agreement, including the expansion and •

balanced growth of world trade, the maintenance of high levels of employment and income, and the development of the productive resources of all members. This is something new, and yet it contains much that is old and tested. A correct reading of the history of past international financial systems will greatly enhance the likelihood of proper policies being pursued in the future. Morgenstern has made a major contribution to this, particularly because he has stressed the presentation of facts in an orderly and responsible manner so that it is always clear what are the pitfalls in various possible interpretations of the data. His method of analysis and presentation is such that others can look for parallels or differences from current and future developments in international financial relations and thus help shed light on these matters. Once the relevancy of his material for present conditions is appreciated, it will obviously be useful to compile similar data on the present period.

A summary of the contents of this book, such as Morgenstern has himself attempted in the final chapter, cannot be an adequate substitute for a careful reading of the book, even for those whose main interests lie outside the details of Morgenstern's analysis. Much of the value of the book, and I suspect eventually its greatest influence, comes from the massive data which have been collected, the careful, even meticulous, scrutiny of these data, the method of analysis, the suggestions, implicit and explicit, for new studies, both current and historical. None of these things can be adequately summarized, but a few of the many interesting points and conclusions may be listed.

Noteworthy is the erratic behavior of financial operations in the period between the two world wars. Cyclical developments in the United States, the United Kingdom, France and Germany during that period affected financial developments and policies in a manner defying generalization sufficiently precise for policy formulation. A study of the interwar period is interesting in order to know what not to do today (e.g., the maintenance of appreciably over-valued exchange rates, the belief prevalent in the 1930's that currency manipulation can accelerate lasting growth or offset wrong domestic policies or inadequate savings and resources, the repeated attempts in the major countries to conduct interest rate policies as though other countries did not exist, the frequent but rather misleading assumption for policy formations that the "closed economy" is anything but a convenient simplification). On the other hand, the author's detailed account of the pre-1914 period sheds light on what we might have to do to maintain the aborning international financial system, e.g., the importance of not underestimating the important significance of relatively small differences when judging the need for interest rate changes or considering appropriate timing and the need to watch carefully the interacting autonomous and international influences affecting money and capital markets, while recognizing how difficult it is to evaluate them accurately.

Another interesting conclusion of Morgenstern's book is that financial markets interact more intensively in times of stress and great activity than in relatively quiet periods. The degree of "dependence" of one market on another changes from time to time (and by no means always in the same direction). A most interesting measurement device, the "effort" series, has

been devised to measure this sensitivity on the basis of empirical data, and Chapter 6 which explains this series merits specially close study. The data clearly reflect how difficult it is not only to predict what market reactions will be to financial changes but also how difficult it is even to know and understand what is actually happening. The psychological factors involved are complex and of constantly varying strength.

Morgenstern points out in painstaking detail how the actual operations of the gold standard differed from the usual theoretical expositions of it. For example, arbitrage among free foreign exchange markets is shown to have been quite imperfect—much more than would be reasonable to expect—and exchange rates repeatedly violated the gold parities. His analysis helps to put us on guard against the possible error of misjudging the impact of exchange rate developments today. For example, it seems that the flow of funds in 1959 among the United Kingdom, the United States and Germany did not correspond completely to the existing patterns of exchange rates, spot exchange rates and forward exchange rates. This is no surprise to many who engage in these transactions in the commercial banks or central banks, but the reasons back of this situation are still only vaguely understood outside of these circles.

Morgenstern emphasizes that a theory of the gold standard based on the assumptions that its operation was fully automatic, and that certain financial interreactions among the principal money markets were predictable and complete is inaccurate because the interaction between money markets via exchange rates and interest rates was not nearly as precise and rigid as postulated, even though the gold standard was a monetary system with the greatest amount of freedom for the automatic forces of the various markets. In countries where the gold standard operated, attention was always paid to domestic as well as to the international repercussions of financial policy. They did not always respond quickly or fully to changes abroad; there was a wider margin within which purely national objectives could be pursued, as well as a greater concern for these objectives. The popular strictures against the gold standard are thus in fact frequently against what people said the gold standard was, rather than against what it actually was—an error also to be found in some of the comments made on the international financial system that is now being developed.

Moreover, the proponents of the gold standard frequently concentrated too much on the theoretical essence of the system and neglected its practical operations. The misunderstanding of the gold standard, as the author sees it, is a product of poor analysis of available data, particularly, the failure to submit the acceptable theory to factual investigation, and inadequate data on gold movements, foreign trade, interest rates, and other matters (he rather terrifies the policy-maker who has to operate with such data as are available). He thinks that the theory of games of strategy may help to give a better understanding of financial operations and cyclical behavior. The need for better and more data, a more careful scrutiny of available data, and willingness to look for a new theoretical framework are the burden of Morgenstern's book.

The book merits close and repeated study. Both scholars and policy-makers may well go astray if they neglect it. Students will find in it both the inspiration and building blocks for further useful research. Such research may improve the statistical and analytical bases for a more sophisticated understanding and therefore enhance the likelihood of a successful management of our new international monetary system.

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The European Common Market & Its Meaning to the United States. A Statement by the Committee for Economic Development, Research and Policy Committee. New York: McGraw-Hill, 1959. Pp. 152. \$4.75.

For some years the Committee for Economic Development has been enlivening the literature of economic policy with well-argued and well-written statements on a wide range of issues. Some of these papers contain detailed programs susceptible of fairly direct translation into legislation or administrative action. Others emphasize the broad features of a problem and stress the general direction of U.S. policy. Because it is one of the latter type, the statement on the European Common Market remains interesting and relevant even though conditions have changed considerably since it was published in May 1959. Then the negotiations for a free trade area had broken down but the Outer Seven had not yet organized their "little free trade area." When the report was written, United States policy toward the linking of the Common Market with the rest of the Western European economy was notable chiefly for its reticence. Since then the American initiatives of December 1959 have led to the creation of a committee in which the United States and Canada sit with representatives of the two European groups to discuss problems of trade. But at the moment this review is written (February 1960) it is still unclear what the next steps in Europe will be, how far the United States will go and even what direction American policy will take. On these matters the CED report has something to say.

Those familiar with committee-born documents will detect some of the marks of the draftsman reconciling divergent views, such as the seesaw of pros and cons, the caudal proviso to conclusions, and the occasional slight haze that conceals precise lines but not large shapes. On the central policy issues, however, the report is explicit enough. It expresses confidence that the European Economic Community will "increase the efficiency and raise the income of its area." Whether repercussions in the rest of the world will lead to more liberalization or toward further fragmentation "depends upon decisions still to be made. . . ." The report stresses the advantages of widening the area of freer trade within Europe. It suggests a number of ways of doing this and, without coming down flatly for one, expresses a preference for the retention of "many of the key trade liberalizing features of the Free Trade Area idea." The report rejects the view that the Six will be politically weakened by such a step. The United States, it argues, should accept the wider area of discrimination be-

cause such an arrangement would strengthen forces making for a lower level of preference. These arguments are still as pertinent to policy as they were when the report was published.

The same is true of one of the strongest and most striking features of the CED report, its emphasis on U.S. policy. In part, this is a matter of tariff bargaining to hold down the level of the Community's outside tariff and of insuring that regional economic agreements conform to GATT. But the main emphasis of the CED conclusion is more general. On the one hand, the future character of the Common Market and a possible Free Trade Area will depend to an important degree on the kind of world economy into which they fit. On the other hand, what Europe and the United States do is of tremendous importance to the economy of the rest of the world. To offer the underdeveloped countries opportunities in a liberal and thriving free world economy is at once to assure them of protection against possible damage from the consolidation of a major part of the European economy and to reduce the pressures on them making for protectionism, whether national or regional.

The CED volume also includes a report by the Comité Européen pour le Progrès Economique et Social (CEPES), a group of French, German, and Italian businessmen. After discussing the relation between economic cooperation and political solidarity, this report devotes a chapter to disparities in income and productivity within the European Economic Community and the problems of internal development it faces. The CEPES report suggests that a reorganized OEEC might provide the means of negotiating a settlement between the Six and the other countries of Europe. It raises the possibility of using tariff quotas as a means of cushioning adjustments during the process of trade liberalization.

A summary of the Common Market Treaty and of the relevant passages in GATT, a substantial statistical appendix, and a number of interesting charts and maps make this volume a substantial introduction to Common Market problems as well as an interesting contribution to a continuing debate.

WILLIAM DIEBOLD, JR.

Council on Foreign Relations

Economic Integration: Theoretical Assumptions and Consequences of European Integration. By ROLF F. SANNWÄLD and JACQUES STOHLER. Translated by Herman F. Karreman. Princeton: Princeton University Press, 1959. Pp. xvi, 260. \$5.00.

The major emphasis of this book, as its title indicates, is upon a rigorous theoretical exploration of the assumptions, consequences and implications of preferential regional trading areas. A secondary emphasis, in terms of the amount of space devoted to it, involves an argument justifying the establishment of such regional trading arrangements. The authors make little distinction between the Common Market approach and the Free Trade Area, implying that their economic consequences are indistinguishable over time.

While the authors admit global free trade to be preferable to regional free trade, they aver that the former is not feasible in the current world and, there-

fore, irrelevant for analytical purposes. Concurrent with the process of international disintegration since 1914, national integration has occurred in the developed economies. In fact, national integration has been considered a desideratum for economic development. Institutional integration through supranational agencies is a logical extension of this national integration beyond national boundaries and necessarily involves intergovernmental coordination rather than reliance upon a market and price economy. The authors are in disagreement, therefore, with the functional approach to global integration advocated by Röpke and others and embodied in the most-favored-nation principles of the Organization for European Economic Cooperation. The regional integration should be gradual and irreversible.

The chief merit of the book lies in the theoretical analysis which has been distilled from the vast body of existing literature in this field. Those who are not already thoroughly conversant with this material will find this book one that can be read with profit. The discussion is concise, straightforward, and free of pedantry.

The authors rely heavily upon the extensive work of J. E. Meade and accept his concept of maximization of welfare through the optimization of trade and the maximization of production as an underlying premise. In turn they analyze the following theoretical issues concerning the economics of a customs union: trade-creating and trade-diverting effects; internal and external equilibrium problems; common currency; harmonization of social costs; fiscal policy and unique tax problems; and factor mobility. In brief, some of their conclusions are: a customs union need not be trade-diverting but rather, under certain circumstances, may be trade-creating; in the absence of a common currency which is desirable in the long run, variable exchange rates by both surplus countries and deficit countries within the customs union are necessary to bring about external equilibrium through price changes; there is no valid reason for permanent harmonization of social costs by the participating countries. The authors buttress their theoretical analysis with occasional references to the empirical evidence available from the experiences of the European Coal and Steel Community and Benelux. And, lastly, they demonstrate that they are not oblivious to the serious political obstacles implicit in the surrender of sovereignty by national governments.

The book underscores clearly that this is a complex question indeed, and that not enough empirical evidence exists to provide determinate solutions to the questions posed. The economist is forced to state alternative assumptions and, therefore, alternative consequences. He is forced also to consider the political context within which a customs union must evolve. This is why, perhaps wisely, the 1957 Treaty of Rome has left many of the provisions for operational procedure vaguely defined.

There has been a two-year delay in bringing out this book's English translation, and in the interim rapidly moving developments have superseded some of its material. But this time lag in no way diminishes the worth of the excellent theoretical analysis.

THOMAS J. LEARY

Il mercato comune europeo. By ORLANDO D'ALAURO. Pavia: Prem. Tip. Successori Frat. Fusi, 1959. Pp. 167.

Much of the literary output dealing with the European Common Market has thus far amounted to little more than either a repetition of the economic principles involved (with views of Jacob Viner and J. E. Meade often easily discernible) or to a restatement of what should be done to achieve the purposes of the Market (with minimum indication, however, of *how* to achieve them). Either approach has invited sweeping generalizations.

Professor D'Alauro's book does not quite break with the aforementioned tradition. While it gives evidence of greater realism than found elsewhere, it does not go far enough in its analysis of specific questions and problems. The interested reader can hardly avoid asking: where are the facts with regard to which the expounded free trade principles acquire their full interpretative meaning? Can some empirical evidence be produced to demonstrate the anticipated boon of economic integration?

Although the present volume is devoted to three additional topics (the problem of currency convertibility, national accounting models, and input-output analysis) this review is confined to the principal section in which the Common Market is discussed.

D'Alauro subscribes more or less to the liberalism—and idealism—of the Treaty, seldom questioning, other than implicitly, the economic theory, policy objectives and instrumentalities of the European Economic Community. The author deviates, however, from the conventional thought of the integrationists in one noteworthy respect when he stresses that, in international cooperation, monopoly and cartels sometimes have a positive role to play. Consequently, business combinations should not be rejected simply because they are theoretically objectionable—a pragmatic view which has been held by many Continental economists and businessmen. D'Alauro states further that during the (12-15 year) transition period, producers' (collusive) agreements may be instrumental in promoting Western European integration because they may facilitate rational geographic redistribution of productive activities. Cartel-type organizations may help to avoid the more serious difficulties which are bound to accompany the adjustment processes of both entire industries and individual enterprises. In line with what has in the postwar years been called "reasonable liberalism," the author contends that in most countries the complex problems of the transition period will require substantial political-economic intervention by the State.

D'Alauro also appears to be convinced that these broadly defined forms of State participation should be coordinated internationally (among the Common Market nations). To some extent, he is carried away by the idea that the more one member country resembles another—in terms of economic and financial regulations, and of institutional arrangements in general—the greater the promise of a successful Common Market. His discussion invites the question whether trade takes place between countries which are equally constituted economically (if this were possible, and in Western Europe, conceivable) or between countries which differ as to factor endowment, prices, protective measures, interest rates, fiscal burdens, etc. While harmonization of many •

policies will doubtless become indispensable for any success of economic integration, the latter's primary objective is the freeing of goods and factor movements with only incidental concern, in the main, for the development of uniformity of economic and financial policies.

Of the policies whose coordination in the Common Market area is desirable, D'Alauro considers monetary policies of strategic importance for both economic stability in each country and international balance-of-payments adjustment. Significantly, he doubts that monetary cohesion of the area can be attained without some political unification.

The author foresees many problems for each member nation during the transition period. They are connected, for instance, with (1) geographic redistribution of production, in which case there is the danger that some regions (e.g., the Rhine basin) may benefit at the expense of other nations (particularly in Southern Europe); (2) adjustments to be made by individual enterprises; (3) changes in the competitive position of some nations; (4) industrial concentration resulting from standardization and a new pattern of specialization; (5) harmonization of production conditions (in view of prevailing factor price discrepancies); and (6) agricultural integration.

D'Alauro maintains that if the six nations' productive systems were more complementary and less competitive, the objectives of economic integration would be more certain of achievement. (Perhaps this conclusion was drawn from the abortive Franco-Italian Customs Union which was under consideration a decade ago.) While many will agree with him as to the urgent need of "the Six" for investment capital from other nations, few will share his view that the notorious failure of agricultural integration schemes can be solved by eliminating protection of agricultural products and by discarding the already well-established—and in many instances beneficial—government intervention. D'Alauro is quite right in recognizing that the Common Market cannot be exclusive and that cooperation with (at least) all other OEEC nations is as indispensable for all the member nations now as it was prior to 1958. But he does not say how the Market would induce some overhauling of the existing Western European trade pattern—with or without the creation of the Free Trade Area.

Discussions of the Common Market are usually focused on reduction of tariff barriers and on questions pertaining to them. Inadequate attention has been paid to the problem of long-term investment, which our author does little more than identify. If the Market is to bring about accelerated economic growth, most of the six nations will unquestionably need more capital. Where is it to come from? Only a small fraction of the requirement (still undetermined with any meaningful accuracy) is likely to be raised from domestic sources. For various reasons, some obvious, but some dubious, the idea of Western European economic integration has been fostered with the United States serving as an example of a market of continental dimensions. There is little evidence, however, that the integrationists have analyzed the region's potentialities and set targets in complete awareness of the much more plentiful resources which have been at America's disposal. If comparisons were made

- in other respects than the size of population, number of consumers, steel pro-

duction, etc., some sobering results, and what really counts, the crux of the Western European integration problem would be arrived at.

An aspect of the problem which bears repeating is the fact—often disregarded on this side of the Atlantic, but undisguised on the Continent—that Europe still is “a tangle of interests with some Franco-German understanding at the summit level.” Europe is still a continent divided against itself, in which the Common Market is to be developed not only as a customs union but also as a political merger.

D’Alauro’s contribution to the study of the European Economic Community is likely to have more weight as an Italian interpretation of the basic provisions of the Treaty of Rome than as an attempt to satisfy the growing need for a realistic appraisal of the Common Market’s prospective impact on its member nations and world commerce.

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The Schuman Plan—a Study in Economic Cooperation 1950-1959. By WILLIAM DIEBOLD, JR. New York: Praeger, for the Council on Foreign Relations, 1959. Pp. xvii, 750. \$6.50.

Despite its short history, the European Coal and Steel Community has quite possibly been the subject of more scholarly articles and dissertations than any other international treaty or institution. However, Dr. Diebold’s book is, to my knowledge, the first to offer a complete, systematic treatment. Anyone who has tried to plow his way through the mass of official and unofficial literature on this subject will appreciate the service the author has performed for future research workers in this field. Diebold has followed the evolution of the Community from its inception, sorting out the facts and analyzing them with sympathetic, yet critical, interest. The result is an authoritative and well-balanced survey which will undoubtedly become a standard reference work.

Part I deals with the historical background of the Schuman Plan and the circumstances and negotiations which led to the treaty establishing the Community. Part II examines the Community’s actions to remove tariffs and quantitative restrictions, subsidies, and discrimination in freight rates; the way it faced its problems in the field of cartels, concentrations, and price policies; its work in guiding investments and in providing assistance for the modernization and reconversion of plants and the retraining and resettlement of workers. There is a particularly lucid discussion of the complicated issue which arose at an early stage regarding the treatment of turnover taxes in intracommunity trade—an issue which, for once, was settled neatly and expeditiously by reference to expert opinion (the Tinbergen Committee) rather than by a test of strength or a compromise. Part III describes the Community’s relations with the outside world, with particular reference to the United States, and its commitments vis-à-vis GATT.

As Diebold points out, his book is written primarily from the public record; it is not an “inside story.” As a result, it only touches on problems which thus far have largely remained the subject of internal discussion within the High Authority, such as compensation schemes designed to protect marginal coal

mines; price collusion in steel; the High Authority's efforts to induce greater price flexibility and to remove special advantages or handicaps resulting from differences in the structure (as distinct from the average level) of taxes, social security systems, transport charges, etc. These problems are only hinted at in the High Authority's annual reports and the full story remains to be written.

The most interesting part of the book is the final section which attempts a broad evaluation of the Community's achievements and prospects. Here Diebold examines the Community's impact on the coal and steel markets, and the "distortions" and tensions created by partial integration. It is here that one might have wished for more rigorous economic reasoning and a more penetrating statistical analysis. The wealth of new data on cost factors, productivity, prices and interregional trade flows compiled by the Statistical Division of the High Authority should now make it possible, for example, to obtain greater insight into the changing relative competitive positions of the different producing areas. To what extent have the effects of integration fallen short of what would be expected in a truly unified market in which trade flows freely in response to economic incentives? What practical consequences have resulted from the fact that partial integration will give a different answer to the question of relative economic advantage than total integration? Do the facts tend to corroborate Scitovsky's expectation that the main effect of integration is to reduce, through increased competition, the range of costs among different plants, within the same country as well as between countries? The author readily admits the need for this kind of close investigation to advance our understanding of what the Community has done and is likely to do. In his analysis, based on cruder indicators, he is careful and circumspect, and his judgment is invariably sound.

Diebold is not taken in by the mystique of the integration movement. He is fully aware of the fact that as of today, the European coal and steel markets are neither unified nor very competitive. He explains the reasons for the continuing price discrepancies, which are far in excess of what would be expected as a result of transportation costs and sales taxes, and some of the factors which impede the adjustment of prices to fluctuations in demand. He rightly focuses on the continuing interference by the national governments as the main obstacle to the achievement of a unified market, and also points to the persistence of "traditional commercial relations" and restrictive business practices among enterprises. He does not expect these impediments to disappear quickly, but he concludes that the Community is solidly entrenched and, most likely, here to stay.

Writing at the beginning of 1959, he saw that the recession, by intensifying competition, was bringing about a greater interpenetration of markets and a gradual convergence of prices; but that it threatened, at the same time, to set in motion all the usual protectionist reflexes, particularly in Europe's ailing coal industry. The book went to press before the resulting strains reached their climax in May 1959 when the Council of Ministers refused to concur with the High Authority's proposal to invoke Article 58 to deal with the "manifest crisis" in the coal industry by imposing production quotas on individual enterprises. As the French Minister of Industry Jeanneney, then president of

the Council of Ministers, explained at a press conference, this was not a matter which governments could leave to the High Authority, as it was not the High Authority which could call out troops to quell disturbances if mines were closed. The problem, which tested the solidarity of the member countries, was ultimately dealt with by a combination of expedients including certain informal restrictions on intracommunity trade which would seem to be inconsistent with the spirit, if not the letter, of the treaty. At the time, the crisis led many observers to predict the impending disappearance of the Community as a supranational institution; but in the historical perspective, it is now evident that this incident (which, with foresight, could have been avoided) has changed little in the character of the Community—where control has always been shared among the High Authority, the governments, and the industries concerned—or in the basically favorable outlook which is the theme of the last chapter of this book.

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Paris, France

Canada in the World Economy. By JOHN A. STOVEL. Cambridge: Harvard University Press, 1959. Pp. xiii, 364. \$7.50.

Mr. Stovel has provided a detailed statistical and analytical survey of Canada's balance of payments since confederation. Almost inevitably, however, the focus of his attention is on the tumultuous decade and a half which preceded the first world war. It will be recalled that it was this period which Jacob Viner selected for study in his Harvard doctoral thesis, published in 1924 under the title *Canada's Balance of International Indebtedness, 1900-1913*; and Stovel's book, also based on a Harvard doctoral thesis (of 1949), is a large-scale critical review of Viner's analysis and of the literature inspired by it.

Viner's main purpose was to test the Thornton-Mill-Taussig view of the mechanism by which disturbances to the pattern of international transactions of a country are accommodated. In particular, he sought to "verify" the hypothesis that, under the gold standard, the attempt to transfer capital from one country to another gives rise to the following sequence of events: a rise in the price of bills drawn on the borrowing country; a flow of gold from the lending to the borrowing country, the rate of exchange being at the lending country's gold export point; an increase in prices in the borrowing country, a decrease in the lending country, with the changes being most marked in domestic prices and least marked in import prices; changes in exports and imports, the borrowing country acquiring a passive balance on current account and the lending country acquiring an active balance; after the passive balance of the borrowing country had become exactly equal to the rate of borrowing, the return of the exchange rate to parity, the cessation of gold movements, and the stabilization of relative prices at their new levels. The Canadian experience 1900-1913 was thought by Viner to provide a satisfactory test case since the inflow of capital was not only on a large scale (in some years it was nearly one-quarter of national income), but it was held to be the most significant

disturbing factor impinging on the Canadian economy. Canadian institutional arrangements demanded one minor modification of the classical theory: the New York balances held by the Canadian trading banks took the place of gold reserves.

Viner's analysis of the Canadian episode has long been recognized as a brave and important pioneering work. But reservations concerning the success of his "verification" of the classical theory were entertained right from the start. The objections have been most cogently expressed perhaps in two recent articles by Meier and Ingram and in an unpublished doctoral dissertation of Jacques Parizeau. (G. M. Meier, "Economic Development and the Transfer Mechanism: Canada, 1895-1913," *Canadian Jour. Econ. and Pol. Sci.*, Feb. 1953, 19, 1-19; J. C. Ingram, "Growth in Capacity and Canada's Balance of Payments," *Am. Econ. Rev.*, Mar. 1957, 47, 93-104; and Jacques Parizeau, *The Terms of Trade of Canada (1869 to 1952)*, thesis submitted to the University of London, June 1955, esp. pp. 68-87, 227-232.) Parizeau's criticisms relate mainly to the unreliability, incompleteness and inappropriateness of the annual time series deployed by Viner—especially of his import and domestic price series. Meier's and Ingram's criticisms are of wider scope. They relate partly to the incompleteness of the theory to be tested. Bastable, Nicholson and Wicksell had already drawn attention to the shifts in purchasing power associated with a capital movement. Since the shift *conceivably* could bring about the transfer without any gold or price movements, it is difficult to understand how the classical theory could ever be verified. At best, a test could be provided of the proposition that the shifts of purchasing power were, in the period studied, of negligible importance. Finally, objections were raised by Meier and Ingram to Viner's view of the fundamental dynamic of the Canadian economy during the period studied.

Space does not permit a detailed recounting of these objections. It is enough to note that several features of the period 1900-1913 could not have been predicted on the basis of the classical theory and of Viner's view that the import of capital was the dominant disturbing factor impinging on the Canadian economy. Foremost among the unpredictable elements of the situation was the fairly steady increase in exports throughout the period. Next was the steady drift upwards of interest rates. Finally, the order in time of developments was, in some years, in flat contradiction of what might have been expected in the light of the classical theory: borrowing did not always precede the real transfer, and prices had been rising in Canada since 1896, long before the inflow of capital became substantial.

It is always possible, of course, to account for exceptions in terms of "special" circumstances analogous to the *deus ex machina* of the ancient theatre. Thus, in Viner's view, "the increase in the value of the total exports was due almost wholly to the entrance into the exports trade of the products of newly-exploited natural resources, where special factors kept down the costs of production" (Viner, *op. cit.*, p. 274). But evidently one must prefer a theory which can accommodate not only those features of the period which the classical theory is able to account for, but also most of the "exceptional" circumstances.

Such an alternative exists. In its essentials, indeed, it existed at the time of Viner's writing in the lengthy exhibit prepared for the Board of Inquiry into the Cost of Living by R. H. Coats, who later became Dominion Statistician [Board of Inquiry into the Cost of Living, *Report II* (Ottawa 1915)]. The importation of capital took place against the background of a boom generated locally by the expansion of the wheat economy, by the rapid growth of population, by industrialization behind the "National Policy" tariff wall, and by steady urbanization. The boom began in the mid-'nineties, it predated by several years the first substantial inflow of capital, and it provided the profit-incentive for the inflow itself. It also gave rise to a steady increase in prices and interest rates. As a result of the high rate of investment, productive capacity grew rapidly throughout the period; the enlarged capacity combined with falling costs of transportation to produce a steady increase of exports. But exports failed to grow fast enough to completely finance the import requirements of the boom; hence the need for a net inflow of capital. The capital import was effected by the relative expansion of purchasing power in Canada, and by the greater rate of price increase in Canada than in England. I doubt whether, in view of the poverty of the (annual) time series, it will ever be possible to determine with any precision the quantitative role of the several factors involved in this theory.

Stovel's own analysis of the period conforms quite closely to that summarized above. It is a pity that Stovel's book has been so long in appearing, for in the interim many of his best points have been made by Meier, Ingram and Parizeau (to none of whom, incidentally, is reference made). Nevertheless the book is a useful addition to the published literature on the adjustment mechanism. In the first place, it provides a convenient and accurate summary of the debate inspired by Viner's thesis. Second, it supplements Viner, Meier and Ingram in providing a careful analysis of the relation between the Canadian terms of trade and rate of capital import during the period 1895-1913. (Stovel—and Parizeau—reach the not-unexpected conclusion that the relation is not nearly as close as the classical theory would lead one to expect.) Finally, it contains a useful appendix on the sources of Canadian balance-of-payments statistics. (On price series, however, Parizeau's first chapter is more satisfactory. The appendix to that chapter contains a balanced and detailed appraisal of each of the numerous import and export price series which have been constructed for the period since confederation.)

Stovel's use of the available time series is at all times quite informal. Indeed he explicitly eschews formal econometric methods. Further, he himself emphasizes the paucity of the price series. It is surprising, then, to find him confidently asserting (pp. 137, 293), in opposition to some even more vigorous assertions of Viner, that variations of import prices played an insignificant role in the process of adjustment. My own feeling is that the role of prices in accommodating the capital inflow during this period is likely to remain an unresolved mystery.

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Business Finance; Investment and Security Markets; Insurance

Struktur und Funktionsweise des Kapitalmarktes in der Bundesrepublik Deutschland. By WERNER DANNEMANN. Tübingen: J. C. B. Mohr (Paul Siebeck), 1959. Pp. 209. DM 16.00.

The book is a companion to E. Brehmer's previously published study on the West German money market, both having been done at the University of Kiel under the supervision of Erich Schneider. In traditional German manner, the author (who is now with the Bundesbank in Frankfurt) begins with a discussion of all previous definitions of what constitutes the capital market in contrast to the money market and he comes close to concluding that there really is very little difference, as in both markets highly liquid debts are exchanged for less liquid ones, and vice versa.

The second part deals with the interesting experiments of the Bundesbank and the Treasury in guiding the German capital market during the decade of 1949-58. From 1949-52 new security issues had to be approved and interest rates were set by a capital market committee with the purpose of channeling the yet meager flow of savings into uses that might yield relatively low returns but that were considered socially of greatest urgency, especially housing. Industry meanwhile was left to rely on depreciation charges and retained earnings. From 1952-54 the government exercised control of the flow of savings into investments through a more indirect method, by extending tax exemptions to all bonds issued for housing and related municipal works. This measure divided the capital market into two parts, the tax-exempt one with a top yield of 5 per cent, while industrial bonds had to be offered to yield 7-8 per cent and even somewhat higher in order to compete for their share of the supply of savings. With the end of 1954 began the period of a free capital market. Because it was now held that internal financing of business investment, which had been subsidized through very liberal depreciation allowances, had achieved its purpose (of keeping the business demand for savings from competing with social demands) and might even have begun to result in faulty investment, the corporate income tax was split; the rate was set at 45 per cent for retained profits and only 30 per cent for profits paid out as dividends.

The results of these measures appear to have been a fall in the rate of interest between 1957 and 1959 from 8 to 5-5½ per cent for industrial issues, and a sharp advance in the prices of shares during this period, with dividends disbursed greatly exceeding the issuance of new shares.

Following the methods developed by the Bundesbank in its annual calculations of capital formation in West Germany, the author then analyzes the supply of and demand for funds for the years 1950-56. The analysis reveals for these years, as well as for 1957-59, a steady rehabilitation of the private capital market in West Germany. (Capital formation has shifted from being primarily dependent on retained earnings to the channeling of savings by private households into industrial securities. In the government sector investments have held to a steady percentage of total investments but in the foreign sector, there has been a radical change from capital imports to exports.)

The following comments of the author are not borne out by United States experience and may therefore also turn out to be misleading in regard to West German conditions as they approach closer to ours:

It is categorically stated that the growth of the government's social security arrangements has restricted the desire for and the ability to pay for private insurance and pension schemes (p. 102).

It is held that the reluctance of Germans to invest in stocks is based on the feeling that equities are too speculative. It appears to this writer that this reluctance could be overcome if the par value of stocks would be reduced—the market price of most German shares is in excess of \$1000—if dividends were to be paid more than once a year and if the banks were not to charge a fee for retaining customers' shares for safekeeping and collecting the dividends. Most of all, the financial reports of German companies understate earnings, and therefore surplus accounts, to such an extent that only insiders can form a true opinion. Management's right to keep stockholders in the dark had been greatly strengthened under the Nazis since the "leader" principle made managements even more autocratic than they always had been. Some reforms, such as the requirement that sales figures be published, went into effect on January 1, 1960, but a more radical adjustment to United States standards of financial reporting is still under discussion (p. 102).

The author expressed the opinion, widely held in Germany, that the prices of stocks are determined by the annual supply of cash dividends paid vs. the supply of new shares offered, a compartmentalization of the capital market which does not appear justified if a new class of stock buyers should become attracted to equities. Furthermore, he holds that stock prices are determined by dividend yields rather than by growth prospects. Both statements may have had some historical justification in Germany as long as equity holdings were restricted to relatively few "industrial" families, but it will no longer apply when equity-ownership becomes as popular as it is now in the United States.

ROBERT M. WEIDENHAMMER

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Industrial Organization; Government and Business; Industry Studies

Merger Movements in American Industry, 1895-1956. By RALPH L. NELSON.

Princeton: Princeton University Press, for the National Bureau of Economic Research, 1959. Pp. xxi, 176. \$5.00.

As a welcome addition to the writings of Bain, Markham, and Stigler on merger movements, Professor Ralph Nelson presents a new series of data on mergers in American industry for the period 1895-1920. Had he done no more than present this excellent new series, students of industrial organization would be deeply indebted to him. He, however, also suggestively examines some leading hypotheses, and analyzes the relationship of merger activity to business cycles.

His quarterly series, covering 1895-1920, are broken down into two-digit SIC categories. They list merger activity in a variety of ways: number of consolidations, firm disappearances by consolidation and by acquisition, au-

thorized or estimated capitalizations of mergers. Thus they describe merger activity in both relative and absolute terms. For his analysis of merger activity and business cycles through 1956, Nelson relies on the Thorp and Federal Trade Commission data (1920-1956).

In Burns' data on production trends in the United States Nelson finds no empirical support for Watkins' "retardation thesis." On the contrary, he suggests that merger activity is prevalent in periods of accelerating growth rather than in periods of retardation.

It is with his test (and rejection) of Bain's transportation thesis that I would quarrel, however. He argues that the following conditions must have held if the thesis is valid: (a) the railroad network must have expanded and transportation costs must have decreased in the period preceding the merger movement; (b) a substantial proportion of the mergers must be accounted for by industries with high per-mile transportation costs relative to price of the product; (c) these industries must have had widely dispersed producing centers if the reductions in transportation costs could result in the expansion of market areas; for "if most producers were concentrated in small geographical areas there would be no exclusive local markets for reduced transportation costs to destroy. . . . Thus, on the purely technical grounds of cost minimization, we should expect to find higher geographical concentration in industries with high per-mile transportation costs than in those with lower costs" (pp. 84-85). But do not Nelson's own measurements permit one to cast doubt on this relationship? His simple, unweighted indices of geographic concentration are .510 and .477 for high and low transportation-cost industries—not an impressive difference. Again, on the basis of his reasoning, would not one expect the manufacture of bricks, asphalt, ice cream, and similar local-market products to be highly concentrated?

Furthermore, there is little reference to the qualitative changes in railroad transportation in the latter part of the nineteenth century, e.g., integration of the rail network. And some of the quantitative measurements are unsatisfactory: in measuring real costs of rail freight, Nelson deflates revenue-ton-miles by the BLS Wholesale Price Index, and derives annual rates of change from data for the initial and terminal years of the 1882-1900 and 1900-1916 periods. (Nelson uses the latter period as a basis for comparison because adequate data for the years before 1882 are not available.) But the choice of 1916 as a terminal year (reflecting the beginning of war-induced inflation) is unfortunate because it substantially distorts the measurement of changes in costs. Had the more representative year 1914 been used as a terminal year, the estimate of the average annual change in price levels would have been almost 50 per cent less, and the estimated real costs of freight transportation would have decreased about 2.6 times faster in the pre-1900 period than in the 1900-1914 period. Thus, not only is Nelson's argument open to question, but his choice and interpretation of data also do not warrant rejection of the transportation thesis.

In a few obiter dicta Nelson casts some doubt on such other explanations of merger movements as: economies of scale, the canalizing effect of judicial interpretation of the Sherman Act, and the role of promoters in forming com-

binations. One hopes that further investigation of these explanations will be taken up by other students of merger movements.

A careful and thorough analysis of the cyclical behavior of merger activity emphasizes the close relationship between mergers and capital-market conditions in peak periods; but there Nelson's investigation ends, leaving the issue of financial versus technological and economic factors unresolved, though most suggestively illuminated.

Economists owe Nelson a debt of gratitude for bringing a breath of fresh air into an area smogged in with conflicting and obfuscating generalizations. His book demonstrates once again the value of an empiricism that does more than keep score through time.

JOHN S. DYDO

Vassar College

Imperfect Collusion in the Cement Industry. By SAMUEL M. LOESCHER. Cambridge: Harvard University Press, 1959. Pp. ix, 331. \$7.00.

This is an important addition to basing-point literature. Professor Loescher of Indiana University regards geographical formula pricing in the cement industry not as the product of autonomous evolution but as a device developed and improved to create a defensive monopoly, not so much intended to raise prices as to stabilize them. The motivation was the industry's susceptibility to price-cutting due to high overhead costs, economies of scale, ease of entry, excess capacity, and unstable and inelastic demand.

Price stability was to be achieved by minimizing oligopolistic uncertainty "so that all sellers would *know* (author's italics) the competition to be met and could exercise the kind of self-restraint in pricing policy posited by simple oligopoly theory" (p. 84). This was to be accomplished by the use of identical delivered prices on a multiple basing-point system. Freight allowances were standardized by a common freight-rate book. All sales were made on the basis of rail charges and no buyers could get delivery at the mill and provide their own transportation. No special treatment was to be given to large purchasers.

Adherence to this rigid price structure was enforced by the possibility of using a punitive base whereby those wishing to enforce the formula might lower their prices depending upon the offender's base, thus forcing him to lower his price on the bulk of his sales. There also developed a number of supplementary trade practices such as the protection of dealer territories and the dissemination of individual company statistics.

Loescher sees this systematic use of the delivered-price formula as something which developed step by step, finally achieving a self-defining, self-enforcing operation which required no conferences or consultations to fulfill the limited objective of price stabilization. The objections which he sees to such a price structure are its inflexibility which tends to encourage excess capacity, excessive cross-hauling, inordinate selling costs, and the various distortions in emphasis which are created by limitation to nonprice competition. Loescher considers that substitute structural materials have provided an

upper limit to cement prices. Prices have not necessarily been too high in boom times but they have not fallen as they should have in weaker periods.

Loescher strongly condemns the basing-point system formula as it has operated in the cement industry. He suggests that the Federal Trade Commission's order (1948) was decidedly inadequate, becoming effective only when there is an industry plan or agreement and not affecting acts independently taken. Loescher would go considerably further to create more uncertainty, suspicion, and even distrust among the oligopolists. This requires not only the destruction of any rigidly controlling and therefore predictable pricing system, but positive action to create competition. Buyers should be assured that they may truck away from the mill. Large contractors should be able to buy directly. Identical bids should constitute inherent collusion, an unfair method of competition, and an improper price discrimination. If these did not succeed in promoting unsystematic pricing and a fairly flexible price level, he would have the Commission "voice the positive injunction 'compete!'" (p. 299), but this comes at the end of the book and the ways, means, and standards are not specified in any detail.

Since cement prices are at the heart of his analysis, one should look at the record. After the beginning of the first world war, they skyrocketed until 1920 and then fell gradually, with only one slight upturn, to 1932. After 1934 they were extraordinarily stable until the second world war. All this can be fitted into Loescher's picture—the long period of falling prices encouraging the "defensive monopoly" through which price stability all through the depression was achieved, although the prices of most nonagricultural commodities were rising after the 1933 low.

However, since 1942 cement prices have risen every year to reach new high levels. Loescher himself comments that since he regards formula pricing of the basing-point variety as primarily a reserve tool to be used to support prices in periods of weakness, he cannot judge the effectiveness of the FTC's Cement Order because of the prolonged postwar cement boom. However, almost immediately after he completed his writing (January 1957), weakness appeared but prices rose nearly 5 per cent in 1956, another 4 per cent in 1957, 2 per cent in 1958, and nearly 1 per cent in 1959. It of course should be noted that the failure of prices to reflect the 1957 recession was a general characteristic of nonagricultural prices. Loescher suggests that the industry "appears to have been administering a moderate price policy (possibly much too moderate) throughout most of the postwar boom, preferring to ration short supplies rather than allow price, even approximately, to clear the market" (p. 289). This suggests numerous questions as to how this was done; but these are current questions, and judgment on the FTC order in Loescher's terms will have to be delayed.

One wishes that the record had required Loescher to give more consideration to the relationship of formula pricing to upward price movements. Perhaps in the end one might come out with the propositions that the dominating factor in the situation is the oligopoly rather than the formula, that oligopolies will tend to act in certain ways, that certainty may make the end result of any

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action more predictable but there still are choices open, and the final result may not be very different whether the choice is made in a more certain atmosphere or under a considerable degree of gamesmanship.

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Tin Cans and Tin Plate: A Study of Competition in Two Related Markets.

By JAMES W. MCKIE. Harvard University Series on Competition in American Industry, 5. Cambridge: Harvard University Press, 1959. Pp. xiii, 321. \$7.50.

The tin can industry, in which two firms of nearly equal size sell 80 per cent of the product, buys tin plate from the oligopolistic steel industry and sells its output principally to a monopolistically competitive canning industry in which there are some firms of considerable size and influence. Thus the theme of the study is bilateral market power in a vertical chain of markets. The author's ultimate purpose, which is more explicitly stated and followed than in most industry studies, is to determine whether the structure of the can industry uniquely determines a level of economic performance that is adequate by general standards of public policy—essentially the purely competitive norm plus progressiveness. If so, the industry is to be adjudged workably competitive. The main sources of data are the records of the antitrust cases of the 1940's and interviews with company officials, who were no doubt the more willing to talk because of loss of the suits. No new research methods are developed, and it is difficult to single out any specific advance in theory, but the author has done a very careful and competent job of interpreting for economists this segment of the industrial framework.

By general economic reasoning rather than statistical investigation the author establishes with reasonable plausibility that: demand is inelastic with respect to both price and income; short-run marginal costs are relatively constant nearly to peak capacity; and there exists no sharply defined optimum size of plant in the can industry. It was not these general characteristics, which are found in many industries, but two special conditions that shaped the policies of can manufacturers. The demand for cans in the packing of fruits and vegetables, over half of the total, is strongly seasonal and is, furthermore, highly unpredictable with respect to both quantity and precise timing. High costs of storage and of transportation rule out production in the off-season and offsetting on a nationwide basis, so the solution chosen is a regional network of plants that can be pushed to satisfy peak demands. A second peculiarity is that the final operation of can-making, sealing on the top, is performed in the canning establishments, on closing machines developed by the can manufacturers.

To avoid uncertainty canners preferred to contract for a season's unspecified requirements at a price fixed for the season, and were willing in exchange to accept long-term contracts. They also wished to lease closing machinery and receive the maximum amount of service. The can manufacturers took

advantage of this situation and, by excessively long can contracts and machinery leases at well below cost, were adjudged to have created a tying arrangement that artificially blocked the entry and growth of competitors. The decree of 1950 imposed, with a time limit in some cases, rather drastic remedies that were unfavorably regarded by small canners and, in the spirit of the Robinson-Patman Act, denied some genuine economies to large canners. There is some evidence that the can market has loosened up, but entry has not occurred and profits have not fallen. It is possible that the long-run effect may be simply to eliminate the competitive waste of duopoly and raise profits.

The author's main conclusion is that, with the added safeguards of the decree, the tin can industry, an 80 per cent duopoly, is workably competitive. This may cause some to remark that economists have come a long way since Marshall and, indeed, have little farther to go. The author has, I believe, satisfactorily established his conclusion, but certain special circumstances need to be emphasized. Collusively high prices are forestalled by: (1) American Can Company's early experience that high prices encourage entry; (2) the threat of large canners to integrate backward; (3) the virtual certainty that the gains would have to be shared with the steel industry in the bargaining over the price of tin plate. Buyers are informed, so there is little advertising. The requirements of the product are well defined and product competition has taken the form of technologically better ways of meeting those requirements. Yet the technology is such that the intensive research of the large companies into product and process has gained them only temporary advantages. There is some cross-hauling and service rivalry may be excessive, but the record with respect to nonprice competition is excellent. The special conditions of the industry have thus checked collusive actions and channeled rivalry into economically acceptable activities.

My main criticism is that the author focuses too myopically on his particular segment of industry and appears to lack perspective. An early chapter summarizing the relevant theory is largely ignored thereafter, and few comparisons are made with other industries. There are now a sufficient number of excellent industry studies so that cross-comparisons can be an effective start toward fruitful theoretical generalizations.

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The Economics of Competition in the Transportation Industries. By JOHN R. MEYER, MERTON J. PECK, JOHN STENASON, and CHARLES ZWICK. Cambridge: Harvard University Press, 1959. Pp. xvi, 359. \$7.50.

This book makes an important contribution to a timely subject: the economic plight of the railroad industry and the advisability of relaxing its economic regulation. With this aim the study undertakes: (1) to measure the cost of alternative means of transportation, (2) to determine the structure of their respective markets, (3) to assess the effects of current regulation,

and (4) to propose policy measures designed to advance efficiency in the transportation industries.

To many students the most interesting part will be Chapters 2 to 5 where the authors place definite values on the fixed cost (sometimes called "threshold cost") and the marginal cost (sometimes approximated by average variable cost) in terms of cents/ton-mile or cents/passenger-seat-mile of transportation. For instance, the latter turns out to be: 1.3 c. by railroad day coach, 2.7-3 c. by parlor car, 5-6 c. by Pullman, 1.25 c. by intercity bus, 2.7-3.5 c. by piston-engined airplane, tourist class for distances over 500 miles, and 1.8-3.2 c. by first-class jet for distances over 1000 miles. It also emerges that trucks, and in particular diesel trucks, pay somewhat less than their share of road taxes; and passenger cars, in particular on urban roads, make up the difference. These chapters deal with the economic difficulties encountered in pinning down these cost figures. Detailed questions and statistical procedures are treated in appendices.

On the basis of their cost findings the authors derive some broad principles of an efficient division of labor in the transport industries (Ch. 6) with results reminiscent of a von Thünen model: high terminal cost discourages short-haul traffic by railroad and plane; higher variable cost, long hauls by truck (other than piggy back) and passenger train. From this description the objectives of a rational transportation policy begin to emerge: to discourage short-haul or low-density traffic by train and convert long distance trucking to piggy back operation on freight cars. The authors see no future in railroad passenger transportation. As Commissioner Howard Hosmer has put it: "... in a decade or so this time-honored vehicle [the railroad passenger coach] may take its place in the transportation museum along with the stage coach, the sidewheeler and the steam locomotive."

The question of what is standing in the way of achieving this optimal division of transportation activities is not treated in a separate chapter but is answered *passim*: the regulation of rates and of entry by public agencies and by cartels in the form of rate-making bureaus. The doctrine of rate-making by "value of service" is examined as an instrument of discriminatory pricing designed to recoup fixed cost in a (constant or) decreasing marginal-cost industry.

This leads to the next question of whether free competition can be relied on to remove the great inefficiencies that have resulted from excessive regulation—and in particular from the discriminatory freight tariff—and whether it can be relied on to produce the ideal division of labor and meet all demand that would pay its cost. In Chapter 8 the authors find that trucking has indeed all the prerequisites of a competitive industry—small units and numerous firms—and that, in all but the overland haul of bulk commodities where monopoly appears to be a real possibility, the elasticities of substitution among firms and forms of transportation are such as to render competition effective. The policy conclusions stated *passim* and in the last chapter are in essence that there should be a considerable relaxation of regulation, particularly of entry into and exit from transportation industries, but no outright removal

of rate regulation, especially not of maximum and even minimum rates on bulk transportation.

This is an interesting and useful work. While it contains no great surprises, it serves to convert more or less vague guesses into more precise estimates of cost relationships. It also constitutes an important step toward a theory of transportation organized along the lines of the general theory of price. This remains true even though the authors, addressing themselves to a public of nonprofessionals, have chosen to advance their arguments in a style which is often repetitive and laboriously elementary to the detriment of theoretical clarity and precision.

While it is all too easy to criticize the techniques that apparently seemed necessary to get results in a fairly short time, many readers will feel uncertain about the true meaning of the statistical cost estimates in view of the obvious question not discussed by the authors: How serious is the bias introduced into the least-squares estimates by leaving other relationships out of the model, in particular those that have determined the size of track and the various other capital items—the “size variables” in the authors’ measurements? For obviously these size variables are not independent variables. A different point may be made about highway cost: the authors do not consider congestion and resulting times losses, and this deprives their discussion of rail commutation versus passenger car transport of much of its relevance.

Apart from these technical questions I have only one point of general criticism: In their policy recommendations the authors appear to be overly cautious. If one agrees with their statement that “reform is most urgently needed . . . in the thinking of management and union groups. . . . Many of the preceding suggestions could be carried out by properly vigorous, imaginative, and knowledgeable managements within the framework of present regulatory practices” (pp. 271-72), one is led to add that precisely this type of management or management behavior would be attracted into railroading only when all or all but traces of the present system of regulation with its stifling effects on initiative had been removed. The case for free competition seems to emerge much more strongly from the analysis of this book than the authors are willing to admit. In any case transportation could be made subject to the antitrust laws to prevent an unlikely repetition of the early railroad wars. A great industry as desperately sick as the railroads can be cured only by drastic means. The partial measures suggested by the authors might actually tend to reduce railroad earnings on high-value commodity shipments while preventing them from raising revenue on long-distance bulk hauls. No matter where one stands on these policy questions one can, I think, agree with the concluding statement:

Transport regulation began in a day and age when the North American continent was first beginning to see the development of a modern industrial economy characterized by large oligopolistic corporations . . . whose operations were restrained by only a minimum of competitive checks. . . . Since those early beginnings industrial concentration has grown markedly, farmers and other small producers have become powerfully organized,

government has developed new techniques and agencies to deal with the problems created by increased concentration of economic power. At the same time, technological advance has created new and very effective competitors to rail transportation. While all this has been going on, regulatory policies in transportation have changed but little. The resulting economic absurdities cry out that some action should at long last be taken.

MARTIN BECKMANN

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Industrial Organization. By JOE S. BAIN. New York: John Wiley & Sons, 1959. Pp. xviii, 643. \$9.25; text ed., \$7.75.

This is the most comprehensive book yet to appear in "the area of economics usually designated as Industrial Organization," which has been carried forward by many scholars since it was pioneered in the 1930's by E. S. Mason but by none more productively than Joe S. Bain.

Its heart is an eight-chapter empirical and analytical study of the American economy, mainly although not exclusively, the manufacturing sector. Attention is focused on the individual industry, but Bain does not present a series of industry studies. "Although the industry-study approach has a demonstrated entertainment value, it is seriously deficient in that it encourages a casuistic process of 'generalizing from a single instance'; correspondingly is prone to engender confusion between accidental associations and fundamental tendencies toward association; nearly always deals with too many parameters and variables for effective analytical handling; and provides no straight or passable road toward scientific generalizations" (Preface, pp. viii-ix).

Using a cross-sectional approach, therefore, he successively examines market (1) structure, (2) conduct—a category which Bain develops intensively, and (3) performance, and seeks interrelationships among them. Performance is what we ultimately want, but for well-known reasons direct government regulation of performance should be minimal. However, public policy might operate through structure and conduct if they in fact determine performance. This policy problem—as well as scientific curiosity—underlies the central inquiry of the industrial organization field: Is performance determined by structure (or by structure and conduct, if conduct can be independently observed, which Bain generally doubts)?

No review can adequately indicate the amount of evidence Bain assembles (drawing heavily, but not exclusively, on a sample of some 20 manufacturing industries), the care and objectivity with which he weighs it (e.g., in the subtle analysis of "excess capacity," pp. 358-62, and of profits, pp. 363-87), and the caution with which he draws conclusions. In brief summary, however, he finds the structural conditions for workable competition to be the following: (1) "high" or "very high" seller concentration "generally seems to be conducive to poor performance in the crucial matter of price-cost relations or profits, without evidently bestowing offsetting advantages in other dimensions of market performance" (p. 423). He discovers a distinct "break" at

the point where the largest eight sellers supply more than two-thirds to three-fourths of the output of an industry; moderately concentrated oligopolies appear to perform about as well as relatively atomistic industries. (2) High barriers to entry are also adverse, compared with moderate and low. (When accompanied by high barriers to entry, high concentration is even worse than when it occurs alone.) (3) Extreme product differentiation increases concentration and raises entry barriers much more than do economies of scale. Also, it results in wastefully high selling costs. Therefore it does *not* make competition more "workable" than it otherwise would be. Moreover, it seems to be hard to correct by public policy, because it is usually associated with products consumers cannot readily evaluate because they are infrequently purchased, complex in design and function, etc. (But soap, cigarettes, and possibly whiskey are puzzling.)

Bain's policy recommendations, tough-minded but judicious, follow naturally from his empirical and analytical investigation. They range over a wide field, but space confines us to one of them. He distrusts direct government regulation of performance, and except for some predatory and exclusionary practices, it is not possible to find an operational set of conduct prerequisites for workable competition. Consequently, he proposes to amend the Sherman Act "to state that structural situations (involving high concentration and impeded entry) which might be expected to have and demonstrably do have monopolistic performance tendencies are generally illegal, without particular reference to the lines of market conduct through which the undesirable structure has been created, maintained, and exploited" (p. 608). He would change the usual remedy to dissolution or dismemberment if there would be no serious side effects and lesser remedies would not clearly suffice. Where undesirable structures originate in product differentiation associated, as noted above, with relatively immutable consumer characteristics, this will not be easy.

How would the book serve as a textbook? In his preface, Bain comments, "I realize that the book is not written to fit numerous existing course outlines in the general field, but would of course be pleased if some such outlines were rewritten to fit the book." This is good advice, especially because it would assure that discussion of public policy would occur only after a thorough economic analysis. Although the reasons for avoiding a "collection of industry studies" approach are impressive, some acquaintance with these would seem necessary to convey the historical, personal and accidental factors that affect every market and to offset the necessarily static bias of cross-sectional treatment. Unfortunately, the length of this book would prevent the use of much supplementary material. In general, for undergraduates the length and the often tedious style (especially in early chapters) are severe handicaps. For graduate students, footnote citations to other works should have been provided, or a larger number of "supplementary readings." As a general treatise, however, this is undoubtedly the most thorough in its field. It has real substance. Its spirit is thoroughly scientific.

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Land Economics; Agricultural Economics; Economic Geography; Housing

The Great Farm Problem. By WILLIAM H. PETERSON. Chicago: Henry Regnery Co., 1959. Pp. xix, 235. \$5.00.

This readable little book presents an illuminating discussion of the "politonomics" of American agriculture in historical perspective. The costly impasse into which the "farm program" has led is damaging the national economy and our international relations. The book is therefore timely. Though written in journalistic style for ordinary citizens, it should not be lightly ignored by economists. Its content is predominantly true and significant, and most of its shortcomings are readily discernible. Some of the language is too strong, but its blunt speaking is mostly warranted. If its brief proposals seem at once too bold and too weak, it is pertinent to observe that many minds through many years have failed to produce a solution of "the farm problem" that is at once economically workable and politically acceptable.

The prologue ("The Vanishing Farmer of Grant Wood") rightly stresses the transformation of American farming in recent decades, under "the inexorable march of technology and economics in a still capitalistic society" (later elaborated on pp. 147-53). The author's broad "Argument" is briefly presented in Chapter 1; his "Analysis" in a long Chapter 7; his "Summary and Conclusions" in a 4-page Chapter 8. Finally, there is a brief "Epilogue" ("Nature's Laws and Man's Laws").

Chapters 2-4 present the highlights of agropolitics and agrarian movements from 1600 to 1917. These provide a historical background for later farm legislation but deliberately give little attention to the evolution of American agriculture in this long period. The selected details bring out the "agrarian proclivity for inflation" (pp. 36-37), public land disposals at bargain prices or free (pp. 39, 50), cheap transport usually involving losses "to the public treasuries" (p. 49), cheap credit (p. 88), and allegedly "the farmer's penchant for intervention from colonial days to the present" (p. 13). One may question this and some other generalizations, e.g., that the Civil War was "created by intervention—the North with tariff intervention, the South with slave intervention" (p. 52).

Chapters 6-7 sketch the evolution of activist farm programs from the first world war to 1959, under powerful and persistent political pressures. Here is a broadly reliable summary of a sorry history, which deserves a wide reading. Insufficient emphasis, however, is laid on the important fact that the central objectives have been to protect and raise farmers' income and status, by political measures, above what a free economy would yield them. Some partial or temporary successes are largely overlooked, and even of the more controversial elements of the farm program it is an exaggeration to say: "A generation of 'farm policy' adds up to hopeless tinkering, fantastic losses, and planned chaos . . ." (p. 217). Few will endorse the author's implication that the creation and development of the U. S. Department of Agriculture, state agricultural colleges and experiment stations, the agricultural extension and farm credit systems, the Rural Electrification Administration (p. 145), and

various forms of federal regulation (Ch. 4) were all unwise policy moves—whatever the criticisms to which they are properly subject.

Peterson's viewpoint is that of an uncompromising "liberal" of the Von Mises school, for whom "statism" is the cardinal economic sin. He finds "the great farm problem" simply political intervention (pp. 8-13). He is basically hostile to any and all kinds of government intervention, and he evinces remarkable faith in the adequacy of a wholly free market. Few economists share this indiscriminating hostility and faith, yet the disgraceful record of failure in much agricultural intervention understandably tempts some to say: "Away with it all!"

The failures, broadly construed, include the Federal Farm Board experiments, the Agricultural Adjustment Administration's more comprehensive ones, various attempts to restrict farm production through acreage allotments, marketing quotas, and "Soil Banks," and the Commodity Credit Corporation's surplus-concentration and disposal programs. Technological advances and government stimuli keep generating surpluses faster than they can be disposed of by all sorts of means. Yet political resistance to effective program revisions continues so powerful that the CCC "investment" in surplus farm products threatens to rise from \$9 billion in mid-1960 to \$12 billion in 1963. It is outrageous that our hard-pressed federal budget should be burdened with several billion dollars a year—equal to some 40 per cent of farm income from farming—to swell cash incomes and enhance farm land values to benefit mainly the larger farmers while restricting their freedom of action.

In his preface the author promised to spell out a simple "remedy for our farm ills" (p. xii). There is none, of course. His one-page "positive program . . . to be gradually effectuated" (p. 213) by no means fulfills his promise. Its three main points are: (1) the progressive abandonment of all price supports and guarantees, all acreage and marketing controls; (2) the gradual sale of government holdings of farm products back to the farmers; and (3) "the progressive abandonment of statism . . . in every phase of American industry." This bold "remedy" is obviously weak politically; but it is hard to conceive that, if adopted with or without tempering supplements, it could cure all the "farm ills" that have given rise to the measures condemned.

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Related Disciplines

Higher Education for Business. By ROBERT AARON GORDON and JAMES EDWIN HOWELL. New York: Columbia University Press, 1959. Pp. xi, 491. \$3.50.

The Education of American Businessmen. By FRANK C. PIERSON and others. New York: McGraw-Hill Book Co., 1959. Pp. xvii, 740. \$7.50.

That two leading foundations (Ford and Carnegie) should simultaneously commission major studies on collegiate education for business may have been

purely coincidental. Yet the coincidence suggests that influential educational leaders have suspected the need for reform of business education. Both studies have confirmed this suspicion.

The staffs of the two foundations were doubtless aware that an appraisal of business education and the prescriptions that would flow from this appraisal would depend in part upon the kind of person selected to direct such a study. Both foundations chose distinguished economists of broad interests who have *not* been closely identified with business schools. (The Carnegie study was directed by Professor Pierson of Swarthmore and the Ford study by Professor Gordon of California.) It is hardly surprising therefore that the approach and the conclusions in the two volumes are similar. The total impact might have been stronger if the similarity of conclusions had resulted from two studies made by persons from very different perspectives. It is intriguing to consider how the conclusions might have varied had the studies been directed by accountants, psychologists, industrial engineers, marketing specialists, or educationists—all of whom have some claim to competence in the field of business education.

In both studies, the question of whether there is a legitimate place in the university for the undergraduate business school was considered. The answer was scarcely a ringing testimonial in its behalf. Yet in a sense the most significant conclusion of both volumes is that the undergraduate business school does (or could) have a useful function, and that it should survive. This finding, however, by no means implies approval of things as they are.

The criticisms of current business education are sweeping and devastating, and they apply to both graduate and undergraduate business education. The Gordon-Howell report refers (pp. 5-9) to "low level and narrow vocational character," standards that are "embarrassingly low," "not providing the kind of education tomorrow's businessmen will need," undefined objectives, "the rut" in which business schools have "plodded." The Pierson report speaks (pp. ix-xvi) of vague definition of subject matter, low academic standards, slack admission requirements, inflation of subject matter, excessive specialization, and "sorely deficient" general quality. Both reports concede that there are notable examples of schools to which these criticisms do not apply, but that the exceptions are rare. Both point out, however, that there are stirrings in business education and that with leadership improvement is feasible.

Business education is a subject of great moment because of the enormous and growing enrollments involved. Business accounts for a major fraction of all higher education. The Pierson report ably describes the development of the field (Ch. 3) and the Gordon-Howell report presents an excellent statistical analysis of its growth (Ch. 2).

A basic problem is that business education, according to overwhelming evidence, attracts students of relatively low average quality. Both reports recommend an upgrading of student quality. This might be done in several ways: by abandoning some kinds of work that might be transferred to junior colleges and noncollegiate institutions; by raising the intellectual rigor of business school courses and thus discouraging weak students; and by imposing selective admissions requirements. It is pointed out, however, that

there is as yet no proven method of selecting students who have the capacity for future success in business careers.

The issues concerning the low quality of students in business schools are among the most difficult. At present these schools are apparently a major haven for many students who are admitted to the university but do not have the ability to compete in most other areas. If the authors' recommendations for greater rigor were followed, many of these students would have no place left for them in the university (except possibly the school of education) and so would be denied the benefit of college education and the honor of college degrees. These reports, therefore, are basically dealing with the question of what persons have a right to be in the university or what minimal level of intellectual rigor should be demanded there. They suggest that the business school should be at least as demanding as the liberal arts college, and preferably more so. I personally take issue with the occasional suggestion in the Gordon-Howell report that those students unqualified for a rigorous business program might better be shifted to the arts college (e.g., pp. 330, 338). The authors also state, however (p. 141): "It is partly because of the less gifted student that we have argued for reform rather than the abolition of the undergraduate program."

The objectives of the business school are inescapably unclear because business itself is changing, because much of the skill and knowledge needed for business success cannot be taught in college but must be learned through experience, because the demands on a businessman vary as he proceeds through his career, because the activities we subsume under "business" are many and varied, and because the businessman is concerned both with his organization and with the environment in which the organization functions. The authors of both reports, therefore, are inclined to discount preparation for the first job, to emphasize skills that are applicable to many situations, general education, solid analytical content, social responsibility, sound work habits, and basic tools such as writing, speaking, mathematics. They believe that there are many possible kinds of education relevant for future businessmen and that the business school provides only one (or a few) of these. Along this line, the Gordon-Howell report strongly recommends (p. 125) a sequence of a limited number of business courses for students majoring in arts, engineering, or other fields and who may seek business careers. The words "analysis" and "analytical ability" appear prominently in both reports. Both recommend fewer courses in business, particularly fewer descriptive courses. It would not be far from accurate to describe the authors' attitude toward many present business courses, with respect to both content and method, as contemptuous.

A major problem of the undergraduate business school is that it is trying to provide both general education and professional education in the span of four years with the result that neither is accomplished satisfactorily. The reports recommend that the weight be shifted toward general education. The Pierson report suggests 50 to 55 per cent of the student's work be in areas outside business and economics (p. 195). The Gordon-Howell report suggests

- that this percentage should be "not less than half" (p. 133). Both reports

emphasize that all is not well in the arts college and that changes are needed there too if the nonbusiness portion of the student's education is to serve its purpose. The Pierson report has a very illuminating section (Ch. 8) on the kinds of nonbusiness courses that would meet the need. Pierson writes (p. xii): "... any steps that are taken to shift the work of business students from the business specialties to greater emphasis on general background subjects need to be coupled with a revitalization of the liberal arts studies as well. Merely to require these students to take a variety of courses in non-business areas taught in a perfunctory manner by instructors whose main interests lie elsewhere will not meet the situation and in fact may cause much harm." This observation led me to the thought that if Pierson, Gordon and Howell had prepared reports on liberal arts schools, they might have offered criticisms nearly as damaging as those they have leveled against business schools.

Both reports include sections on business research (which they consider to be in a parlous state) and on the preparation of faculty for teaching the kind of program they envision. Research and teacher preparation are considered to be key elements in achieving needed reforms. Both reports contain excellent chapters on adult education of businessmen including the sizeable programs sponsored by business firms. Both discuss education in small colleges having no separate business schools. The Pierson report has several excellent chapters on the various special fields of business contributed by specialists in these areas, and also a splendid contribution by G. L. Bach on "Managerial Decision Making as an Organizing Concept."

I can offer nothing but praise for these two reports. They are judicious, restrained, and statesmanlike. They offer realistic and practicable suggestions. They recognize that there are varying local situations and that not all institutions can or should try to do the same things. They point out that the field is in flux and that there are abundant opportunities for experimentation. Yet they never flinch from the basic proposition that radical reform is clearly indicated.

These reports have significance far beyond business education. They are relevant to all higher education, not only in the professions but also in the liberal arts. They show that there are many routes to good education, and that merely because a given program is designed in part for practical career objectives, it need not be narrow, stultifying, or illiberal. Indeed, they might have emphasized that it is no sin (as sometimes suggested by purists) to capitalize on the motivation provided by a career interest to stimulate students. The tragedy of business education is that the built-in career motivation has not been exploited in the interests of excellence.

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Related Disciplines

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NOTES

A nominating committee consisting of George W. Stocking, chairman, Vincent W. Bladen, Everett E. Hagen, Charles J. Hitch, Frank C. Pierson and Edwin Young has submitted the following slate of nominees for 1961 officers of the American Economic Association:

President:

Paul A. Samuelson

First Vice President:

Edward S. Mason

Vice President:

Solomon Fabricant

Milton Friedman

Walter W. Heller

Richard A. Lester

Executive Committee

Kenneth J. Arrow

Abram Bergson

Ewan Clague

Charles P. Kindleberger

Representative, Social Science Research Council

George H. Hildebrand

THE NORMAN S. BUCHANAN PRIZE

Friends of Norman S. Buchanan have established a fund at the University of California, Berkeley, to provide a biennial award of \$250 for an outstanding doctoral dissertation.

NEW PUBLICATION

The Food Research Institute, Stanford University, has begun publication of a journal that contains articles reflecting the current research interests of its staff members. The first issue of *Food Research Institute Studies* was published in February 1960. There will be three issues a year, in February, May and September, with an annual subscription price of \$7.00. All communications should be addressed to Food Research Institute, Stanford University, Stanford, California.

Announcements

Section K—Social and Economic Sciences—of the American Association for the Advancement of Science will hold sessions for contributed papers at the annual meeting of the A.A.A.S. in New York, December 26-31, 1960. Association members interested in presenting a paper based on recent research at these sessions should forward titles and abstracts not later than September 1 to Donald P. Ray, Secretary of A.A.A.S. Section K, National Institute of Social and Behavioral Science, George Washington University, Washington 6, D.C.

The Maxwell Graduate School of Syracuse University, in cooperation with the Danforth Foundation, will present the second faculty seminar dealing with problems of metropolitan areas during the period August 28-September 7, 1960. This ten-day seminar will focus on identification of substantive problems of urban regions and on the research methods available for the analysis of those problems. The 1959 seminar was considered very

successful as an interdisciplinary approach to the complexity of the metropolis, and this year's plan is built upon that experience. Jesse V. Burkhead of the Maxwell School, Norton E. Long of Northwestern's Transportation Center, and Otis D. Duncan of the Population Center, University of Chicago, among others, will serve as discussion leaders.

The Inter-University Committee again has limited funds available for grants to colleges and universities in support of postdoctoral faculty specialists on the Soviet Union or East Central Europe, for short-term, study-related visits in those areas. Countries within the scope of the program are: the Soviet Union, the Communist-controlled countries immediately contiguous upon it, and Albania, Bulgaria and Yugoslavia. Although awards have already been made to candidates selected from applications received up to April 20, additional awards can be made throughout the year if funds remain available. Applications should be sent to the Inter-University Committee on Travel Grants, 409 West 117 St., New York 27, N.Y.

The *1960 Directory of College Graduates Available for Business Personnel*, issued annually by Alpha Kappa Psi, national business fraternity, will be mailed to business firms interested in employing college graduates with majors in business fields, economics and statistics. The selected list presents photographs and brief factual summaries of qualified graduates of leading universities and colleges who will be available for employment in 1960. Copies of the directory may be obtained from Alpha Kappa Psi Fraternity, 111 East 38th Street, Indianapolis 5, Indiana.

Deaths

- Oskar N. Anderson, University of Munich, February 12, 1960.
 Julius B. Bearnsen, emeritus University of Utah, October 17, 1959.
 John D. Black, emeritus Harvard University, April 12, 1960.
 Margaret L. Brew, Cornell University, November 21, 1959.
 Alzada Comstock, emeritus Mount Holyoke College, January 15, 1960.
 Howard R. DeLancy, October 1959.
 Howard W. Green, July 8, 1959.
 Rupert A. Hawk, Grinnell College, October 27, 1959.
 Julian L. Holley, Johns Hopkins University, February 1959.
 George R. Keith, January 10, 1960.
 Svend Laursen, February 1960.
 James E. LeRossignol, emeritus University of Nebraska, December 2, 1959.
 Erik Lindahl, University of Uppsala, January 1960.
 Archibald M. McIsaac, Syracuse University, January 12, 1960.
 Carl V. Noll, Hunter College, February 26, 1960.
 Vladimir G. Simkhovitch, emeritus Columbia University, December 9, 1959.

Retirements

- John W. Dietz, University of Florida, June 1960.
 Otto H. Ehrlich, New York University.
 Russell H. Moore, Ohio State University, March 1, 1960.
 Bervard Nichols, University of Pittsburgh, June 1960.
 Edward L. Smith, Hunter College, February 1, 1960.
 H. L. Wilson, University of Miami, Florida.

Visiting Foreign Scholars

- Douglas C. Hague, Sheffield University: visiting professor Duke University, 1960-61.
 Margaret Hall, Somerville College, Oxford University: visiting professor of economics, Massachusetts Institute of Technology, spring 1961.

Terence W. Hutchison, University of Birmingham, England: visiting professor of economics, University of Michigan, summer session, 1960.

Alexander M. Kerr, University of Western Australia: visiting lecturer, The University of Texas, second semester, 1959-60.

Robin Marris, Kings College, Cambridge University: visiting professor of economics, Northwestern University, fall quarter, 1960-61.

L. O'Nuallain, University College, Galway, Ireland: visiting scholar, University of Michigan.

Don Patinkin, Hebrew University, Jerusalem: visiting professor of economics, Northwestern University, fall 1960.

Michael Postan, Cambridge University: visiting professor of economics, Massachusetts Institute of Technology, fall 1960.

Emil Rado, University College, Legon, Ghana: visiting assistant professor, Williams College, 1960-61.

Mary Robertson, University of Nottingham: Duke University, summer 1960.

Hugh Rose, Exeter University, Exeter, England: visiting associate professor, Northwestern University, 1960-61.

Omar Wahby, University of Cairo, Egypt: research associate, department of economics, Iowa State University (Ames).

Promotions

Francis Bator: associate professor of economics, Massachusetts Institute of Technology.

Edwin W. Bishop: assistant professor of economics, Georgetown University.

August C. Bolino: associate professor, Saint Louis University.

George H. Borts: professor of economics, Brown University.

J. W. Brandon: professor of accounting, School of Business Administration, University of Miami.

Michael J. Brennan: associate professor of economics, Brown University.

Albert Buckberg: assistant professor of economics, Iowa State University (Ames).

Deane C. Carson: associate professor of economics, Brown University.

Paul G. Clark: professor of economics, Williams College.

A. E. Crotty: assistant professor of accounting, School of Business Administration, University of Miami.

John C. Dawson: associate professor of economics, Grinnell College.

Lev E. Dobriansky: professor of economics, Georgetown University.

Randall A. Hoffmann: assistant professor, department of economics, Iowa State University (Ames).

Holland Hunter: professor of economics, Haverford College.

Boris Ischboldin al Bakri: professor of economics, Saint Louis University.

Jacob J. Kaufman: professor of economics, Pennsylvania State University.

Kenyon A. Knopf: professor of economics, Grinnell College.

Dorothy Lampen: professor of economics, Hunter College.

John F. Lubin: associate professor of industry, Wharton School, University of Pennsylvania.

Edwin Mansfield: associate professor of economics, Carnegie Institute of Technology.

Will E. Mason: professor of economics, Pennsylvania State University.

Joseph P. McKenna: professor of economics, Saint Louis University.

Arthur C. Meyers, Jr.: professor of economics, Saint Louis University.

G. L. Moss: associate professor of accounting, School of Business Administration, University of Miami.

Carl A. Nordstrom: assistant professor of economics, Brooklyn College.

Nilan Norris: associate professor of economics, Hunter College.

Richard W. Poole: assistant professor of economics, Oklahoma State University.

K. L. Roberts: assistant professor of accounting, School of Business Administration, University of Miami.

Gunther H. Ruff: associate professor of economics, Georgetown University.

Tadeusz Siedlik: associate professor of business and economics, University of Maine.

Jerome L. Stein: associate professor of economics, Brown University.

Lawrence L. Werboff: associate professor of economics, Pennsylvania State University.

Melvin I. White: professor of economics, Brooklyn College.

J. W. Wilcox: associate professor of economics, School of Business Administration, University of Miami.

Ronald P. Willett: assistant professor of marketing, School of Business, Indiana University.

Viola Wyckoff: associate professor of economics, New York University.

Shih-Cheng Yu: associate professor of business and economics, University of Maine.

W. H. Zukowska: assistant professor of accounting, School of Business Administration, University of Miami.

Administrative Appointments

John R. Coleman: head of department of economics and promoted to professor of economics, Carnegie Institute of Technology.

Richard M. Cyert: head of department of industrial management, and promoted to professor of economics and industrial administration, Carnegie Institute of Technology.

Melvin A. Eggers: chairman, department of economics, Syracuse University.

David Felix: director, Wayne State University Center for Economic Studies.

William E. Gordon: chairman, department of economics and business administration, Park College, Parkville, Missouri.

Everett D. Hawkins: has resumed chairmanship, department of economics, Mount Holyoke College, after spending a year and a half in Indonesia.

Samuel P. Hayes, University of Michigan: chief, social science department, UNESCO Secretariat.

John L. Johnson: assistant dean, general education division, Ferris Institute, Big Rapids, Michigan.

Ernest Kohn: acting chief, division of research and statistics, New York State Banking Department.

Harold C. Krogh, University of Kansas: director of research, Society of Chartered Property and Casualty Underwriters.

Glenn Miller: assistant director, Center for Research in Business, School of Business, University of Kansas.

John P. Miller: director, social science division, Yale University.

Hollis W. Peter: director, Foundation for Research on Human Behavior.

B. U. Ratchford, Duke University: Vice-President, Federal Reserve Bank of Richmond.

Michael Schiff: area chairman, accounting, statistics and taxation departments, Graduate School of Business Administration, New York University.

James H. Stauss: dean, Grinnell College.

M. G. Taylor, University of Toronto: principal and vice president, University of Alberta.

Cyril A. Zebot: acting chairman, department of economics, Georgetown University.

Appointments

William H. L. Anderson: assistant professor of economics, University of Michigan.

Jules Backman: research professor of economics, New York University.

Robert R. Barnes: instructor in finance, Wharton School, University of Pennsylvania.

Thomas B. Birkenhead: instructor in economics, Brooklyn College.

Jacob G. Birnberg: instructor in accounting, Graduate School of Business, University of Chicago, effective October 1960.

Robert J. Connor: assistant professor of production management, Graduate School of Business, University of Chicago, effective October 1960.

Carl T. Devine: visiting professor of accounting, Graduate School of Business, University of Chicago, effective October 1960.

T. A. Finegan: assistant professor of economics, Princeton University.

Franklin Fisher: assistant professor of economics, Massachusetts Institute of Technology.

Robert W. Fogel: assistant professor of economics, University of Rochester.

Samuel Frumer: faculty lecturer in accounting, School of Business, Indiana University.

H. G. Georgiadis: assistant professor of economics, Princeton University.

Robert W. Gillespie: assistant professor, University of Illinois.

Jack K. Greenberg: instructor in industry, School of Business Administration, University of Pittsburgh.

Daniel C. Hamilton: associate professor of business economics, Graduate School of Business, University of Chicago.

John C. Harsanyi, Australian National University, Canberra: professor of economics, Wayne State University.

James B. Herendeen: research associate, department of economics, Iowa State University (Ames).

George H. Hildebrand, University of California at Los Angeles: professor of economics and industrial and labor relations, Cornell University.

William R. Hughes, Harvard University: assistant professor of economics, Wesleyan University, effective 1960-61.

Erich Isaac: assistant professor of economics, The City College, New York, effective fall 1960.

J. Hugh Jackson, dean emeritus, Graduate School of Business, Stanford University: visiting professor, University of Minnesota, spring quarter 1960.

Charles L. Jamison: visiting professor, University of Minnesota, spring quarter 1960.

Richard I. Leighton: instructor in economics, Duke University.

Paul W. MacAvoy: assistant professor of business economics, Graduate School of Business, University of Chicago, effective October 1960.

Alvin Marty, Northwestern University: assistant professor of economics, The City College, New York, fall 1960.

D. McFarland: assistant professor of economics, Princeton University.

Ronald I. McKinnon, University of Minnesota: assistant professor of economics, Syracuse University.

Janet K. Messing: instructor in economics, Hunter College.

Robert Miki: assistant professor of economics, Williams College.

Glenn H. Miller, Jr.: instructor in economics, University of Kansas, September 1960.

Franco Modigliani, Carnegie Institute of Technology: professor of economics, Northwestern University, effective September 1960.

Marc Nerlove, University of Minnesota: professor of economics, Stanford University.

M. Olson, Jr.: assistant professor of economics, Princeton University.

D. Orr: assistant professor of economics, Amherst College.

David L. Paden: visiting assistant professor of transportation, School of Business, University of Southern California.

Edith T. Penrose, Johns Hopkins University: reader in economics in London School of Economics and School of Oriental and African Studies, University of London, effective October 1960.

R. C. Pratt: assistant professor of political economy, University of Toronto.

William G. Rhoads: assistant professor of economics and assistant director of Graduate Training Program in Development Economics, Williams College.

Roderick H. Riley, formerly executive director, Joint Economic Committee of Congress: economic advisor, U. S. Information Agency.

Charles E. Rockwood: instructor in economics, Florida State University.

Karl Roskamp, Brandeis University: assistant professor of economics, Wayne State University.

Norman Schneider, University of California, Berkeley: assistant professor of economics, Williams College.

T. Y. Shen, Federal Reserve Bank of Boston: assistant professor of economics, Wayne State University.

Howard J. Sherman: assistant professor of economics, Wayne State University.

Samuel B. Stewart, former Tax Commissioner, State of Oregon: member of Brookings Institution Korean Tax Advisory Group in Seoul.

Balder von Hohenbalken: research associate, department of economics, Iowa State University (Ames).

Larkin B. Warner, Indiana University: assistant professor of economics, Oklahoma State University.

James E. Young: assistant professor of economics, University of Arizona.

Leaves for Special Appointments and Assignments

Rockwood Q. P. Chin, Berea College: lecturer, University of California, Santa Barbara, spring term 1960.

Lorne D. Cook, Pomona College: Brookings research professor, 1960-61.

John M. Culbertson, University of Wisconsin: visiting associate professor of economics, University of Michigan, second semester 1959-60.

John H. Cumberland, University of Maryland: consultant on economic development, Organisation for European Economic Cooperation, European Productivity Agency, in Paris, 1959-60.

Richard A. Easterlin, Wharton School, University of Pennsylvania: visiting professor, Stanford University, 1960-61.

Wendell C. Gordon, University of Texas: lecturer in international economic theory, University of Buenos Aires, Argentina, first semester, 1959-60.

William D. Grampp, University of Illinois: visiting professor of economics, The City College, New York, 1960-61.

George E. Lent, Amos Tuck School, Dartmouth College: visiting professor of finance, School of Industrial Management, Massachusetts Institute of Technology, spring 1960.

John H. McMichael, Wharton School, University of Pennsylvania: with International Cooperation Administration, in Costa Rica, spring term 1959-60.

Roy E. Moor, Williams College: Brookings National Research professorship, 1960-61.

Walter C. Neale, University of Texas: Fulbright senior visiting lecturer in economic development, Panjab University, Chandigarh, India.

Alpheus W. Smith, New York State School of Industrial and Labor Relations, Cornell University: visiting lecturer, University of Hawaii, February-June 1960.

Leslie L. Waters, University of Indiana: Visiting Rose Morgan professor, University of Kansas. spring semester 1960.

Resignations

Alvin L. Marty: Northwestern University.

Ray H. McClary: School of Business, Indiana University.

Charles F. Meehling: department of economics, Brooklyn College.

Richard K. Stuart: University of Maine.

Miscellaneous

Eduard Heimann: has lectured in twelve German universities; received a "Festschrift" *Zur Ordnung von Wirtschaft und Gesellschaft* (Tübingen, 1959).

Jorge Mendez Munevar: elected member of the National Planning Council 1959-61, by the Colombian Congress.

VACANCIES AND APPLICATIONS

The Association is glad to render service to applicants who wish to make known their availability for positions in the field of economics and to administrative officers of colleges and universities and to others who are seeking to fill vacancies.

The officers of the Association take no responsibility for making a selection among the applicants or following up the results. The Secretary's Office will merely afford a central point for clearing inquiries; and the *Review* will publish in this section brief description of vacancies announced and of applications submitted (with necessary editorial changes). Since the Association has no other way of knowing whether or not this section is performing a real service, the Secretary would appreciate receiving notification of appointments made as a result of these announcements. It is optional with those submitting such announcements to publish name and address or to use a key number. Deadlines for the four issues of the *Review* are February 1, May 1, August 1, and November 1.

Communications should be addressed to: The Secretary, American Economic Association, Northwestern University, Evanston, Illinois.

Vacancies

Economist: The Office of Area Development, U.S. Department of Commerce, has positions for several economists in the fields of economic and regional development, industrial location, and statistics. Salaries \$4,980 to \$8,330, depending on qualifications and experience. Master's degree required. Write to Victor Roterus, Director, Office of Area Development, U.S. Department of Commerce, Washington 25, D.C. Please submit Civil Service Form 57 (available from local post offices).

Economist, Central Intelligence Agency: Openings in the United States government are available for qualified economists interested in economic research concerning foreign areas. Positions involve the measurement of aggregative economic performance as well as detailed research on major industries and on agriculture, transportation, communications, and international trade. Prefer applicants with an advanced degree or substantial amount of graduate work in economics. Salary scales and other benefits comparable to civil service. Starting salary depends on experience and training. A minimum of 5 years U.S. citizenship is required. For application form and interview arrangements, write to: Office of Personnel, Central Intelligence Agency, 2430 E Street, N.W., Washington 25, D.C. Please give personal data and résumé of education and experience.

Economist: Unusual opportunity for regional economist in new agency. Write to: State Planning Office, State Capitol, Santa Fe, New Mexico.

Chair of Economics: Salary £S.2760 per annum. Cost-of-living allowance approximately £S.180 per annum at present. Outfit allowance £S.50. Family allowances: Wife £S.60 p.a., 1st child £S.90 p.a., 2nd and 3rd child £S.30 p.a. each (£S.1 = £1.0.6 sterling). Passages for appointee and family on appointment, termination and annual leave. Superannuation Scheme. Appointment on contract for five years with possibility of renewal. Unfurnished accommodation provided at 7½% of salary. Applications (10 copies) detailing qualifications and experience and naming 3 referees should be sent to: Registrar, University of Khartoum, c/o Inter-University Council for Higher Education Overseas, 29 Woburn Square, London, W.C. 1, England (from whom further particulars may be obtained).

Senior economist: Needed for supervisory position in economics research division of major oil company. Qualifications: Ph.D. in economics; 3-5 years of experience in economic research, government, or teaching. Familiarity with national income accounts, business regulatory problems, and applications of economic theory to business



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The American Economic Review

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SEPTEMBER 1960

NUMBER FOUR

THE FIRST TWO DECADES OF THE AMERICAN ECONOMIC ASSOCIATION

By A. W. COATS*

As this month marks the 75th anniversary of the formation of the American Economic Association it is an appropriate moment to recall the state of American economics in 1885 and to reassess the changing character and fortunes of the organization in its formative years. Earlier accounts have emphasized the theoretical and policy issues that divided the so-called "old" and "new" schools of economics [15] [16] [17] [18] [9, Ch. 9] and have unduly neglected other, less obtrusive, aspects of the story. The following pages are designed to supplement the published record and to place the Association's early history in a wider intellectual and social context.

I. The Inauguration

The American Economic Association was officially inaugurated on September 9, 1885 in the Bethesda Parish building at Saratoga Springs, New York, following discussions among a miscellaneous group of scholars, ministers and social reformers who were attending the second meeting of the American Historical Association. The initiator of this venture, Richard T. Ely, a vigorous young member of The Johns Hopkins University faculty, was following the lead of his senior colleague in the history department of that institution, Herbert Baxter Adams, who was the founder and secretary of the older association. The *dramatis personae* and the location reveal the character of the enterprise,¹ for there was at that time no independent academic discipline of eco-

*The author is lecturer in economic and social history at the University of Nottingham, England. This paper incorporates research initially undertaken for a Johns Hopkins Ph.D. thesis (1953) and continued in 1958-59 with the aid of a Rockefeller Foundation fellowship.

¹Apart from the religious element in the setting, Saratoga Springs was the regular meeting place of the American Social Science Association, under whose auspices the AEA was created [5, p. 548].

nomics with a recognized corps of practitioners; and religious inspiration and reformist zeal were to play a major role in the organization's early history. The personal link between Ely and Adams symbolizes the connection between the rising tide of economic thought and the contemporary movement towards genuine "university" education in America. Both men had embraced the Germanic conception of higher education and scholarship which formed the intellectual core of this movement; and they had received encouragement and advice from D. C. Gilman, the distinguished first president of Johns Hopkins, so that the two associations illustrate the exuberant activity of the institution at the head of the "academic procession."

Just as the process of adapting the German university to American needs and conditions required the cooperation of established scholars, administrators and trustees, so the process of launching a professional association of economists would have failed if Ely had relied exclusively on the support of the young men who had recently completed their studies in Germany. The idea of an economic association was undoubtedly German in origin; but an earlier attempt to form an association in 1883-84 had failed because it had been modeled too closely on the *Verein für Sozialpolitik* and because its sponsors had placed too much emphasis on the role of the state in economic affairs [17, pp. 133-35, 296-99]. The program of this abortive Society for the Study of National Economy had met with a chilly reception not only because its provisions entailed too specific and radical a break with the past but also, one suspects, because its progenitors, E. J. James and S. N. Patten, represented the University of Pennsylvania, an institution that was out of touch with the contemporary "university" movement and had long been regarded with suspicion as the home of protectionism and the center of opposition to orthodox political economy.²

Ely had formulated a plan for an association of "... economists who repudiate *laissez-faire* as a scientific doctrine . . ." as early as 1884, but he deferred his proposal until it was clear that the James-Patten scheme had failed [14, F. A. Walker to Ely, Apr 30 1884] [15, p. 55*n*]. When he drafted his platform for the American Economic Association in the following year, Ely toned down his references to the role of government in economic and social life; the names of James and Patten did not appear among the signatories of the "call" to Saratoga, although they were among the founder-members of the new body; and the prestige of Johns Hopkins probably added weight to his appeal.

² Some years later E. J. James recalled his efforts to form an association and remarked that some of the academic economists he consulted believed that no new body was needed since the ASSA, which had been founded in 1865, "practically performed the only available function of such an organization" [15, p. 109].

But whatever the reasons, Ely's call met with a warm response not only from the young scholars who had been impressed by the reigning German school of historical economics but also from leading historians, prominent university presidents and ex-presidents like Gilman, Andrew White, C. K. Adams, W. W. Folwell, and Francis A. Walker (who was also a distinguished representative of the older generation of economists), such outstanding liberal ministers as Lyman Abbott and Washington Gladden, and officers of the American Social Science Association.³

II. *Social Science versus Social Reform*

Notwithstanding its broad and influential support, the American Economic Association was fully established only after a struggle for recognition and support that continued intermittently throughout a seven-year probationary period. These initial difficulties cannot be explained merely by reference to the dispute between the old and new schools of economics over questions of scientific method, economic theory and public policy that were debated at length in *Science* magazine in 1885-86 and discussed in many other learned and popular journals and newspapers.⁴ Certainly this dispute had an important bearing on subsequent doctrinal developments; but there was a deeper question of principle involved, namely: how far was the Christian impulse to social reform compatible with the scholarly impartiality deemed appropriate to a scientific body? The effort to reconcile these two aspects of the Association's original purpose came to be the constant preoccupation of its officers, and had a marked impact on its character and early development. Since both the public and the academic reputation of the organization were at stake, neither of which could safely be ignored by its members in view of their peculiar and sometimes uncomfortable involvement in contemporary social issues, this matter merits careful examination.

The difficulties involved in the task of reconciling scholarly impartiality with reformist zeal were largely responsible for the gradual attenuation of the original platform or statement of principles. Ely's initial draft was significantly ". . . modified in the direction of conservatism" [16, p. 144] at the inaugural meeting, before it was incorporated in the Association's constitution, and was published with the proviso that it was not to be regarded as binding upon individual mem-

³ It has been suggested that the more conservative members of the ASSA joined the AEA [5, p. 520]. For a full account of the inaugural meeting see [18].

⁴ The argument began with E. J. James' hostile review of Simon Newcomb's *Principles of Political Economy* in *Science*, 6 (1885) pp. 470-71, which brought replies and rejoinders *ibid.*, pp. 495-96, 517-18, 538, 563 and was formally continued in the ensuing two volumes with articles by most of the leading contemporary economists.

bers. Nevertheless it still contained references to the positive role of the church, the state and science in the solution of social problems by the "... development of legislative policy"; and these references provoked the hostility of those defenders of the received tradition by whom, as Francis A. Walker later recalled, *laissez faire* "... was not made the test of economic orthodoxy, merely. It was used to decide whether a man were an economist at all."⁵

To regard this hostility simply as an expression of opposition to the proposal to increase the functions of the state would, however, be a mistake, for there were some who doubted whether it was proper for a supposedly impartial scientific body to subscribe to any policy recommendations whatsoever. Ely himself had acknowledged the dangers of partisanship when he complained, in his original draft, that "... economists have been too ready to assert themselves as advocates," although he defended the platform somewhat inconsistently on the grounds that "... it is not easy to arouse interest in an association that professes nothing" [18, pp. 7, 19]. Simon Newcomb, an early opponent, may have been unduly caustic when he remarked that in Ely's view the Association was "... intended to be a sort of church, requiring for admission to its full communion a renunciation of ancient errors, and an adhesion to the supposed new creed" [31, p. 106]; but his comment reveals the attitude of one influential section of orthodox opinion.

Thus it soon became apparent to the Association's officers that the statement of principles might prove to be a liability, for there was ample evidence that it acted as a deterrent to some of the more conservative economists.⁶ With the disappearance of the initial attitude of

⁵ [40, p. 254]. It is significant that Walker was no young upstart seeking to overthrow the ruling doctrines, but a mature and highly respected innovator working within the received tradition. His difficulties were strikingly revealed in a letter to Ely dated April 30, 1884: "Perhaps no one has had more occasion than myself to feel the need of such moral support from fellow workers in political economy as might come from formal association and concerted action. When I first started out in 1874, I suffered an amount of supercilious patronage and toplofty criticism which was almost more than I could bear. Downright abuse would have been a luxury..." [15, p. 78].

⁶ E. W. Bemis, reporting to Ely a conversation with J. L. Laughlin of Harvard, wrote: "He refuses to join the A.E.A. because it has any constitution save love of truth, for he does not know what he will believe five years hence and hence cannot belong to any class of disciples. Then too he took occasion to pour out his disapproval of an association which seemed desirous of excluding Prof. Sumner, if no one else, for though he claimed to dislike his evident dogmatism he preferred to be associated with men of opposite views and did not believe Prof. Sumner would make trouble."

All this and much more he professed to say from a love of science and a dislike to divide its force" [14, Sep 29 1886].

Similarly, E. R. A. Seligman wrote to Ely: "I met Hadley [of Yale] at the Political Economy Club last Monday. When he heard that the 'confession of faith' had been dropped, he said he would join the Association gladly, and that he proposed to take a warm interest

exclusiveness,⁷ which had been inspired by the fear that the organization might be captured by orthodox doctrinaires, there was a growing desire to enroll the Harvard, Princeton and Yale economists, who had hitherto held aloof more because of their suspicion of the platform than because of their uncompromising adherence to classical doctrine. By 1887 the proposal to modify or abandon the platform was under discussion, and John Bates Clark wrote to Henry Carter Adams [1, May 4 1887]:

I had better tell you what I suggested to Dr. Ely as a compromise policy in the matter of [the] platform. I understand Pres't Walker wishes to drop the platform wholly. I had not anticipated quite so radical a move, though I have from the first thought that the platform would ultimately cease to be necessary. It does a certain work by giving character to the association during its earlier years. My proposal was that we agree not to oppose the abolition of the platform a year hence, provided the measure be not pushed this year. The friends of the measure will ask 'why not now if ever'? My idea is that the work of the platform is essentially temporary, and that two (practically three) years will prove long enough to retain it.

In fact events closely followed Clark's proposal, for the decision to drop the statement of principles was approved by the Association's council in December 1887, and ratified at the third annual meeting twelve months later [4, p. 86]. But although some prominent members insisted that no change of principle was involved, the vehemence of their denials suggests the importance they attached to this change of policy.⁸

in us, and lend an active hand whenever possible. Put him down as a member" [3, Jan 2 1889].

One branch association, of which there were six by the end of 1888 [4, pp. 87-91], applied for membership without the statement of principles [14, Seligman to Ely, Jan 29 1888].

⁷ In his autobiography Ely frankly disclosed this attitude. Although "we were anxious to win the great body of economists . . . [we] aimed to gather together like-minded men, congenial men who it was supposed could profitably work together. Not every economist was at first asked to join, although no economist who expressed a desire to join was refused enrollment" [17, pp. 141-42].

⁸ Thus Seligman, the treasurer, wrote to Ely: "In answer to Dr. [Albert] Shaw's letter, I would say that in my opinion the change was not made in deference to any coterie—least of all the New York Nation coterie. . . . At the New York meeting President Walker stated that he was in favor of a change and had never favored the original platform because [it was] a platform. . . . None of the members present in the least desired any 'back down' but it was thought that the portions omitted were not essential, [and] might be misinterpreted . . . I do not think, nor did anyone suppose that the change could possibly be interpreted as denoting a change in the sentiment which dominates the Association. It was simply held that the welfare of the Society would best be promoted thereby . . ." [14, Jan 29 1888]. Shaw, of Minneapolis, a former student of Ely at Johns Hopkins, subsequently became editor of the *Review of Reviews*.

In this connection Ely's position is of particular interest, for despite the danger of overestimating the role of a single individual in the life of an organization, it is no exaggeration to say that in the early years of its history, the public response to the American Economic Association was largely determined by the various reactions to his work. As academic economists at that time, whether employed by state or private universities, repeatedly felt the pressure of public opinion [29, Part 2], this response could hardly be a matter of indifference to the rising economics profession. Apart from acting as the organization's initiator, first secretary (for seven years), promoter-in-chief and most ardent defender, Ely was a prolific writer whose studies of current labor, taxation and monopoly problems made him the most widely discussed economist in the country in the late 1880's and 1890's. As the most outspoken critic of the old school of American political economy, Ely drew the full fire of the diehard exponents of that doctrine; and even his closest friends among the younger generation of economists, J. B. Clark and H. C. Adams, warned that certain features of his work lent support to the repeated accusations that he was a socialist and a sentimentalist.⁹ Their concern for Ely's security of tenure at Johns Hopkins was doubtless reinforced by their desire to protect the reputation of the American Economic Association and its adherents.

Of these two accusations, the former has been stressed by subsequent historians, and was undoubtedly a most serious matter at the time. Ely's boldly sympathetic study of the labor movement appeared shortly after the Haymarket bomb incident in Chicago had aroused widespread public alarm, and the story of Newcomb's damaging unsigned review in the *Nation*, the most influential journal of the day, which described Ely as a socialist and a man ". . . seriously out of place in a university chair," has often been told.¹⁰ In the event, al-

⁹ For instance, in reply to Ely's request for comments on his work, Clark, a very mild-mannered man, asked: "Does this passage justify the accusation brought against us of confusing the boundaries of economics and ethics? . . . is political economy ever hortatory?" and again: "Is the impression when the essay is in print heightened or diminished by the use of such strong expressions? Is not the rhetoric of restrained statement, if not of understatement, better?" H. C. Adams wrote: "The political economy of Mill does not 'glorify selfishness.' The expression is unscientific. A good deal of this part of your paper seems to me to be polemic rather than critical" [14, Vol. 18, Miscel. Scrap Books, Nov 1887].

¹⁰ [9, p. 163]. Newcomb was then professor of mathematics at Johns Hopkins, and to the end of his life Ely believed that there was a local plot to expel him from the university [16, p. 146]. For contemporary references see [6, Ely to Clark, Dec 1 1886; H. C. Adams to Clark, Dec 16 1886; A. Johnston to Clark, Dec 17 1886; W. Gladden to Clark, Dec 25 1886].

In fact the current terminological confusion made it difficult to determine a writer's attitude to socialism. For example [7, pp. 566-67, 570, 577]. About this time H. C. Adams wrote in his personal diary: "I am a socialist—to tell the truth—with the very characteristic exception of accepting their plan of reconstruction" [1]. It is, therefore, hardly sur-

though this attack undoubtedly enhanced the suspicion of the Association in some quarters, it shocked economists of various shades of opinion, who rallied to Ely's support; and by persuading F. W. Taussig of Harvard to apply for membership, the first representative of the old school to do so, it directly strengthened the organization's representation among the "younger traditionalists." Indeed this proved to be but the first of many occasions on which the economists overcame personal and doctrinal differences in order to display their professional *esprit de corps* on behalf of one of their number who was threatened by outside interference.

In retrospect, however, the charge of "emotionalism" or "sentimentalism" resulting from Ely's religious fervor and his persistent stress on the inseparability of economics and ethics was more important in the long run; for it caused greater concern to his fellow economists and it contributed directly to his temporary break with the Association in 1892, a move which seriously threatened the unity of the organization. This matter has been unduly neglected by earlier commentators, although it sheds considerable light on the development of the economics profession and on the changing character of the Association, from that of an agency of social reform—a function that was dear to the hearts of the founder-members—to a more strictly scientific and scholarly body.

In order to explain the issues involved it is necessary to recall the state of American economic and social thought in the 1880's. At the centenary of the Declaration of Independence Charles Dunbar of Harvard had confessed that ". . . the United States have, thus far, done nothing towards developing the theory of political economy" which had become a body of rigid dogma both as expounded by its acknowledged leaders and by the numerous popularizers [12, p. 140]. Political economy had been assigned a subordinate role in the college curriculum, as a branch of mental and moral philosophy; its teachers, usually doctors of divinity, lacked interest and training in it, and the resulting doctrines comprised a blend of vulgarized Ricardianism and New England theology that offered little of relevance to contemporary life [32]. The intellectual (and financial) poverty of most American colleges had forced many able students to seek their advanced training abroad—in Germany rather than in England, not only because German academic life was flourishing, but also because religious influence was still strong in the best known English colleges. As J. Dorfman has shown [10],

prising that H. W. Farnam remarked, in a review of *The Labor Movement in America*: "Dr. Ely says—and he certainly ought to know—that he is no socialist. Yet much that he says sounds so much like what a good many of the socialists say, that he ought hardly to complain, if people occasionally mistake him for one" [20, p. 686].

German historical ideas had already met with a favorable reception in America in the 1870's, well before the flood of returning graduates had reached its peak. The flexibility of these ideas—in marked contrast to the received tradition—made them readily adaptable to domestic conditions and encouraged the hope that they might help to solve contemporary economic and social problems. Moreover they blended with the popular currents of scientific and evolutionary thought that were undermining the pedagogical foundations of the established economic and social creed and challenging the prevailing theological control of the college curriculum [28] [11, pp. 43-44].

In these circumstances it was inevitable that hostile critics should interpret the strong religious and ethical tone adopted by some of the new-school economists as a sign of soft-headedness and a return to less scientific thought.¹¹ Ely was indubitably the chief offender; but the early works of J. B. Clark and H. C. Adams (whose names were frequently linked with Ely's by the critics), E. B. Andrews, C. D. Wright, and other prominent members of the American Economic Association also bore the indelible imprint of their religious convictions and moral purpose; and by the late 1880's it was a commonplace of popular literature that economics must, or indeed had already become an "ethical science."

It is therefore no coincidence that the Association attracted considerable support among the liberal clergy,¹² for liberal theology and a progressive attitude to social policy were often found in conjunction with one another. Some of Ely's correspondents praised his "brotherly sentimental" economics and likened the work of the American Economic Association to that of the Home Missionary Society¹³; but it is understandable that orthodox economists like Laughlin, Taussig and Hadley wondered whether the organization was merely substituting one brand of partisan advocacy for another, more pernicious one.

When Hadley protested that the "so-called moral reaction" against orthodox economics was really an "emotional" one, he was not only

¹¹ W. G. Sumner's characteristically outspoken reference to "the whimsical people who have hobbies of one sort or another, and who cluster around the Social Science Association, come forward with projects which are the result of a strong impression, an individual misfortune, or an unregulated benevolent desire, and which are therefore the product of a facile emotion, not of a laborious investigation" [39, p. 305] may have been typical of the hard-headed conservative viewpoint. Cf. *infra*, n. 14.

¹² The Association's first published membership list, dated March 1886, included the names of 23 clergymen in a total of 181 members. The number rose to a peak of 39 by 1894 (when the total had reached 800). This is far below the 60 claimed by W. D. P. Bliss in 1892 [36, p. 9].

¹³ [14, H. F. Craven to Ely, Nov 11 1902; J. B. Sewell to Ely, Dec 30 1890]. The Ely papers include many such eulogistic references to his moral and religious influence. For a modern opinion see [8, Ch. 4].

expressing his irritation at the arrogant assumption of "a superior moral purpose" on the part of those "... trying to right visible wrongs by direct state action," and his belief that "... the harm which has been done by laws based on unemotional reasoning is but a drop in the bucket compared with that which has been done by laws based on unreasoning emotion"; he was also endeavoring to defend the economist's scientific reputation.¹⁴ Like his less conservative colleagues, Hadley was only too familiar with the poverty, insecurity and vulnerability of the little band of professional economists [15, pp. 94-95], and he was convinced that prestige and influence could be earned only by exercising sound scholarship and wise statesmanship. When he asserted that the "... economists as a body ... strongly disapprove the attempt to 'popularise' economics by giving too much weight to the conclusions of uninstructed public sentiment" [26, p. 191] he was voicing an opinion that was then (i.e., 1894) shared by most of his fellow members. Since the mid-1880's there had been a noticeable shift in the prevailing tone of American economics, and this shift cannot be fully understood without a close examination of the circumstances underlying the change of leadership in 1892.

III. *The Ascendancy of Science*

During the first seven years of its existence, the leadership of the American Economic Association rested continuously in the hands of Walker as president and Ely as secretary; and the election of Dunbar¹⁵ to the presidency has usually been regarded as a turning point in the organization's development. The transfer of leadership not only indicated that the original fear of capture by the forces of reaction had diminished; as a conciliatory gesture to a prominent representative of the old school it also constituted an armistice that marked the end of hostilities between the two warring schools and the beginning of a

¹⁴ [26, pp. 186-87]. For other conservative reactions to "emotionalism" see [13, p. 24] [30, p. 2]. It was then, and indeed still is, a central tenet of orthodox methodology that analysis and prescription should be rigidly separated, and this was one of the points of difference between the old and new schools. But it is risky to generalize about their respective attitudes to science. Walker and Patten, for example, repeatedly emphasized the distinction between analysis and policy recommendations, whereas Clark, who steadily became more conservative, was always criticized for his failure to make this distinction. There was a key debate on this issue, and on the general question of the social role of the scholar, following Hadley's presidential address in 1899 [25, pp. 62-88].

¹⁵ Dunbar had written a moderate critique of the new school in the opening number of the *Quarterly Journal of Economics* [13], of which he was the editor. He had been invited to join the AEA by Walker and Ely [37, Ely to Seligman, Apr 25 1887] and at a council meeting the next month it was agreed that a deputation should confer with Dunbar about his enrolment. Dunbar (and Hadley) had been present at the Boston meeting in May 1887 and he joined a year later [3, Seligman to Ely, Nov 21 1888; Dunbar to Ely, Dec 12 1888].

period of agreement on fundamentals and renewed emphasis on theoretical work.

Although correct in its essentials, this version tells only part—and, from the present point of view, not the most important part—of the story. Ely's resignation as secretary in 1892 was the immediate outcome of a dispute which marked the effective repudiation of the Association's original commitment to quasireligious social reform; it was accompanied by his decision to move to the University of Wisconsin, a move that symbolized the westward shift in the centre of gravity of American economics; and it was followed, later in the decade, by undercurrents of discontent in the academic ranks which threatened the unity and stability of the American Economic Association. These are matters of more than parochial interest, for they enable us to discern hitherto undisclosed links between the new, German-inspired, school of economics and the progressive reform elements in American institutionalism. Cf. [22] [21, Ch. 7].

As we have seen, there was nothing novel about the desire to widen the Association's representation among the more orthodox economists. There had been a steady if slow influx of such members from 1887 on, due both to individual decisions and to collective efforts at recruitment.¹⁶ The initial sense of insecurity had soon passed, and as early as 1887 Walker had declared his willingness to stand down from the presidency if it would serve the cause, although Ely privately expressed his fears of the continued danger of control by the Yale and Princeton men in 1888, when he sought to persuade H. C. Adams to take over some of the secretarial duties.¹⁷ The dispute of 1891-92 was,

¹⁶ Hadley and Laughlin did not accept early invitations to join the council [37, James to Ely (copy), Apr 27 1887]. Subsequently Taussig wrote to Seligman that Laughlin "intimates that he will come in if Ely goes out" [37, May 30 1892]. In fact Laughlin did not join until 1904, although as late as 1898 efforts were still being made to persuade him [3, Horace White to W. Willcox, May 25 1898].

There had been several attempts to include "the Yale circle" [37, Clark, and Ely to Seligman, Apr 25 1887]. Sumner, the most dogmatic exponent of orthodoxy, presented the most delicate problem. It would be "... unworthy of us ..." Clark wrote to Ely, either "... continuing on 'a platform of opposition to Sumner' as one of his close friends called it in conversation with me ..." [or] making undue overtures to him. This would also be making too much of him. He is a prominent man; but if he is a great man I have never done him justice. He seems to me to be chiefly what Pres't Walker once called him, a cantankerous man ... A few could assure him that he would be welcomed—provided that the fact is that he would be;—but for the executive committee in their official capacity to invite him seems to me to be too far to go ... I know, of course, that he feels excluded, and would not join without some unusual encouragement. That encouragement had better limit itself to private assurances; and these would have to be preceded by some mutual expressions of view. The case of Professor Dunbar is not in favor of more official invitations" [14, May 30 1889].

¹⁷ "Some seem to think that all things considered, I ought to keep the place and that the division of the office as I wanted is not desirable just now. I have heard a hint, I may tell you in confidence, about a combination of Yale, Harvard and Columbia to capture

however, only indirectly connected with this process, for it was occasioned by Ely's decision to hold the next annual meeting at the Methodist summer camp site at Chautauqua, New York, a decision which brought a chorus of protest from some of the leading members.

There were various reasons for this reaction: the belief that Ely had acted unconstitutionally in forcing his decision on an unwilling executive committee; the suspicion that he was putting his personal interest in Chautauqua before the welfare of the organization, since it was feared that the Association's scientific reputation might be endangered if it became too closely associated with a popular religious educational movement;¹⁸ and finally, the prospect that it would deter the leading representatives of the old school, several of whom had joined but had not yet participated in the Association's activities [19, Seligman to Farnam, June 2 1892]. In the event, the dispute was patched up. Although it was too late to alter the venue of the meeting, which had already been announced in the press,¹⁹ opposition to it was moderated by the knowledge that a public row would harm everybody, and by the fact that Ely, who had at first threatened to resign instantly if the decision were overruled, had agreed to submit his resignation at the next meeting.²⁰

the thing and run it, but that would mean ruin. The West and the South would never submit. Apart from that, a large proportion of our members support us because they suppose we stand for something positive; and were [word illegible] a little because we dropped our 'Statement of Principles'. Now if the choice of Secretary should seem to indicate a further reaction, there are intimations of a split. This would be a great pity" [14, Ely to Adams, Dec 17 1888]. Ely's fears might be dismissed as wildly exaggerated but for the fact that his prophecy almost came true six years later. *Infra*, pp. 568-569.

¹⁸ For Ely's Chautauqua activities see [17, pp. 79-87]. Among those who opposed Chautauqua George Gunton complained that "it is very much like having it held at some spiritualist camp-meeting" [37, Gunton to Seligman, Dec 26 1891]. Similarly Davis R. Dewey wrote that the objectors felt that "Chautauqua is not associated with the highest academic scholarship" [14, Dewey to Ely, Jan 10 1892]. Walker, in referring to a "serious and determined protest" from F. H. Giddings, Taussig and Seligman [and later H. C. Adams], added that he personally would like to visit "the great intellectual camp meeting of the country" [14, Walker to Ely, Dec 18 1891].

¹⁹ There is evidence that Ely had "jumped the gun," [37, Walker to Seligman, Dec 10 and Dec 19 1891] [14, Giddings to Ely, Jan 1892].

²⁰ "To my request for a call to the Executive committee or of the council," Walker wrote to Seligman "he replies that this can only mean a purpose to humiliate and insult him. and that he will straightway resign.

• • • • •

"Small loss, you say; and I don't disagree with you. The time passed, three years ago, when Ely had anything to give to the Association. Since then he has been a drag on the Association. But he promises to resign peacefully in August, whereas if he goes out now there will be a specious row. All the Socialist and semi-Socialist papers will join in attacking the Association and raising Ely to the rank of a martyr. The whole Chautauqua influence will be invoked against us. The newspapers will take the affair as 'another row among the political economists'" [37, Dec 23 1891].

• Clark agreed: "President Walker is right in deprecating the row I am sure. It would

Some members of the Association undoubtedly interpreted this outcome as a victory for the old school rather than a sign of the achievement of unity. Nor was this interpretation confined to those dissatisfied with the change. In a final, unsuccessful effort to persuade W. G. Sumner to join, his Yale colleague H. W. Farnham gave a vivid glimpse of the Chautauqua meeting:

This is the first time Yale has been represented at one of these meetings by one of its professors and I think that it created a good impression to have one of them there. At any rate they made me one of the Vice-Presidents.

There was in general quite a new deal in the officers. Dunbar was made President; a young man by the name of Ross of Cornell was made Secretary and though Ely was consoled by the office of Vice-President, this means practically the end of his régime in the association. In fact it was rather amusing to see on how many occasions he found himself in a minority of one.

In view of these changes do you not think that you would like to join? . . . I think that you would find yourself quite in sympathy with the present spirit of the society. [19, Sep 5 1892].

There was indeed some justification for the dissatisfaction of such men as Ely, E. W. Bemis, and J. R. Commons, to whom the adoption of a more scholarly and "scientific" stance entailed a withdrawal from active participation in economic and social reform, and a growing emphasis on theoretical problems.²¹ Yet there is no reason to conclude that the change of leadership, and the subtle shift of policy implied therein, was the outcome of the sinister machinations of a few diehard reactionaries, for it evidently reflected the prevailing tone of professional opinion. When the trend of American economics is seen in its social context, it becomes clear that the preoccupation with marginal utility and marginal productivity analysis in the 1890's did not merely reflect the dissatisfaction with the unfruitful methodological and doctrinal controversies of the previous decade; it also reflected the economists' yearning for scientific status and prestige. This they sought to attain by dissociating themselves from the past, and by establishing economics as an independent scholarly discipline, free from theological, ethical, historical, and sociological connotations and, above all, free from the taint of missionary zeal and political partisanship.

mean a fight with all that would, in that event, range itself on Dr. Ely's side, and that would be a great deal" [37, Clark to Seligman, Dec 30 1891].

²¹ In part the development of a more detached and cautious approach reflected the growing awareness of the complexity of current problems and the inadequacy of simplified solutions. Yet this had usually been the "conservative" attitude [cf. 39, p. 306]. For a recent comment on this see [38].

One of the by-products of this process was the emergence of sociology and economic history as separate disciplines both in the United States and Europe, a process to which such prominent members of the American Economic Association as F. H. Giddings, E. A. Ross, A. W. Small, and W. J. Ashley made notable contributions. On a sociological plane, the change in the character of the Association constitutes one element in what has recently been called the "status revolution" [27, pp. 149-55]—a movement that brought with it a rise in the prestige of the professors and a decline in the status of the clergy. By facilitating the secularization of their discipline the economists were also—not always unwittingly [34]—enhancing their professional status.

III. *Midwestern Academic Discontent*

Yet beneath the superficial harmony resulting from the collective desire to minimize controversy on methodological and policy questions there were significant notes of discord. By far the most interesting and the most neglected aspect of this situation is the emergence of a distinct regional pattern of opinion reflecting contemporary political, social and intellectual tensions which was aided by the expansion of economic training in the middle west. It is obviously impossible to consider here the problems of analyzing regional variations in social and cultural conditions and of tracing their effects upon the work in particular university disciplines.²² But as the economists could hardly avoid discussing such contemporary issues as free silver, the trusts and the tariff, matters on which there were certain clearly marked regional differences of interest, it is no surprise to find that their attitudes sometimes reflected the pressure of public opinion. The selection of faculty was influenced by local prejudices, and the number of summary dismissals and "academic freedom" cases in the 1890's made the penalties of a defiance of local sensibilities painfully clear [29, pp. 413-45].

Of course these incidents reveal that at least some economists openly resisted local prejudices, and others were doubtless protected by their own tact or by the courage and good will of their university authorities. But as the opinion spread that the American Economic Association had become more conservative, there was a swelling chorus of dissatisfaction from various mid-western centers at a time when radicalism was rife in that region, and this dissatisfaction gave considerable concern to the organization's officials, especially when the threat of a rival body appeared in 1895 with the formation of the Political Science Association of the Central States.

²² For one interpretation of the differences between eastern colleges and midwestern state universities see [33, pp. 108-17] [cf. 23, pp. 21-27].

The proposal to form this association appears to have originated with John R. Commons and his colleagues at the University of Indiana during the summer of 1894,²³ and it undoubtedly reflected a fairly widespread feeling that the existing national organizations of economists and historians were indifferent to the needs of middle-western scholars. The fear that the new body might become a serious rival to the American Economic Association seems to have been inspired less by the danger that it would be dominated by the "... Ely-Commons faction . . ."²⁴ than by the possibility that it might be captured by the predatory powers of the University of Chicago, an institution which was expanding rapidly with the aid of John D. Rockefeller's fortune and which was widely regarded as a hostile and ruthless rival to the established leaders of the academic world.²⁵ However, the determined efforts of J. L. Laughlin and H. E. von Holst failed, and the new organization was launched under the leadership of men whose first act was to pass a resolution "... distinctly affirming it as the policy of the Political Science Association to arrange its future meetings so as to avoid conflict with the two national organizations in which we are all vitally interested."²⁶

²³ [1, Commons to Adams, Jan 9 1895]. G. W. Knight, the secretary, assured Adams that "*It is not in any way a University of Chicago organization or machine, nor did the call for the conference originate with them*" [Jan 8 1895, italics in original]. J. H. Canfield, Chancellor of the University of Nebraska, also claimed authorship of the scheme [3, Canfield to J. W. Jenks, Jan 21 1895].

²⁴ [1, Adams to Jenks, Jan 7 1895]. Commons assured Adams that he had no hostile intentions towards the AEA [1, Jan 9 1895]. In a letter to Ely, however, Commons had spoken of an "... organization of Western economists . . ." who "... would be mainly your students . . .," at least initially. There was a need, he added, for such meetings of sympathizers, "... especially now that these forces of opposition seem to be marshalling together" [14, Sep 17 1894].

In a brief but revealing letter to Ely the President, Jesse Macy of Grinnell, Iowa, remarked that the Political Science Association was founded on ideas similar to those prevailing when the AEA was started, adding that the new body must not be "... swallowed . . ." by the AEA [14, Apr 19 1895].

²⁵ Among the economists this attitude was based partly on the current suspicion of business influence and, more specifically, on Laughlin's unwillingness to join the AEA and his independent establishment of the *Journal of Political Economy* at a time when strenuous efforts were being made to consolidate and reduce the number of periodical publications on economics. Taussig, for instance, referred to his and Dunbar's "... disgust at this intrusion [i.e., the JPE] into the field. It is a clear case of not having the interests of science at heart; though it may be a question whether this interest is pure and undefiled in the breast of any of us. We all have a concern for our own institutions" [37, Taussig to Seligman, Mar 17 1892]. Similarly [19, Seligman to Farnam, Jun 2 1892].

²⁶ [1, Jan 8 1895]. Yet the reports diverged somewhat. Whereas Knight claimed that "... no one was more solicitous that it should not be or even seem to be run by the Chicago men than those men themselves; especially was this true of Professor Small who, by the by, is a prince of good fellows," Bemis reported that the resolution encountered fierce opposition from Laughlin and von Holst (professor of history at Chicago) and three or four others [3, Jenks to Adams, Jan 8 1895].

The officers of the new association were president: Jesse Macy, Grinnell College, Iowa;

Although the new body did not prove to be a serious rival of the American Economic Association, it had a direct impact on the latter's policies; and the purposes behind its formation merit consideration partly because they have apparently not been published previously, and also because they reveal the difficulties facing the officers of the national economic association in their efforts to retain its hold over the profession. G. W. Knight outlined the purposes of the organization in a letter to H. C. Adams: to bring together four groups of scholars who could "... get together ..." in no existing association; to cater for many "... poor fellows ..." in the "... western region ... who as college instructors are trying to work and do their best in two or more of these fields, and who are only partially at home in any other one Association"; to discuss the "... many questions of pedagogy and methodology in connection with these fields, and *peculiar to the institutions of this section of the country*, or at least away from the sea-board ..."; and to give opportunities to men who would not or could not shine at the national meetings [1, Jan 8 1895].

It is clear that the new body succeeded in focusing attention on the westerners' grievances. Although J. B. Clark and J. W. Jenks, as president and secretary of the American Economic Association, did not at first regard it as a serious rival of their own organization,²⁷ H. C. Adams, who was invited to head the economics section of the Political Science Association, was much less sanguine. Influenced both by his position as head of the economics department in what was widely regarded as the leading state university, and by his scepticism towards recent theoretical tendencies,²⁸ he sympathized with the motives behind the new venture, and in a letter to J. B. Clark he frankly criticized the economic association's handling of the situation.

vice-presidents: A. Small (Chicago, sociology), C. H. Haskins (Wisconsin, history), Adams (Michigan, economics) and J. Woodburn (Indiana, political science); treasurer: F. W. Blackmar (Kansas, economics); secretary: G. W. Knight (Ohio, economics).

²⁷ [1, Jenks to Adams, Jan 8 1895; Clark to Adams, Jan 10 1895] [3, Clark to Jenks, Jan 12 and Jan 21 1895]. Yet Clark's initial response was "The West is stirred up. ... One thing is clear:—we must make our association so good, and so fair in its policy, that there can be no ground for leaving it, however many rivals enter the field" [3, Clark to Jenks, Jan 10 1895].

²⁸ Adams was at the University of Michigan, and was also chief statistician to the Interstate Commerce Commission. There are many examples of his attitude to theory: "To my mind the Austrian School has already exhausted itself and I am wondering whether so clear a man as Clark will be able, after committing himself to the mechanical reasoning of the School, [to overcome] its limitations" [1, Adams to Seligman, Jun 1 1896]. Also [1, Adams to Seligman, May 14 1902; Adams to K. Coman, Feb 6 1897]. However Adams recognized his own limitations as a theorist. Of Clark he admitted: "I hardly feel competent to follow him in his later development, but so far as I can understand him, I am obliged to dissent from many of the conclusions that he thinks so pertinent" [1, Adams to C. P. Emerick, Feb 5 1897].

You ask what I think of the plan of holding in alternate years sectional meetings of the Association. To answer definitely, I do not think it would do at all, that is, it would not serve the purpose we of the West had in mind when insisting that the American Economic Association should meet West of the Allegheny mountains. Perhaps the thing we aim at cannot be attained, that is to keep the A.E.A. to represent the economists of the U.S. This is a very big country and travelling is of course burdensome. But I think the men in the East show altogether too much disinclination to come West. Certainly if it is impossible to have good meetings in any of the western cities, at which a great majority of the members of the East shall be in attendance, we might as well face the issue at once and allow that there shall be three or four economic associations in the country. Of course I shall go with my section if anything of this sort seems to be inevitable.

[The Political Science Association] . . . need not be a rival of the A.E.A. . . . I am quite confident, however, although I have no direct evidence of the fact, that it was intended by those who have been responsible for starting it, to break off from the AEA [1, Jan 7 1895].

Some weeks later, after considerable discussion, it was decided to hold a joint meeting of the two associations at Indianapolis. The location is significant, not only because of its proximity to the birthplace of the Political Science Association, but also because the University of Indiana, and Commons in particular, were under severe attack in the state legislature, and it was felt desirable to show ". . . the people of Indianapolis and of the state the determination of the economists of America, no matter what their opinions, to insist upon the freedom of instruction and investigation."²⁹ At this meeting H. C. Adams was elected president of the American Economic Association, a move that, while fully justified by his standing in the profession, was no doubt influenced by the desire to conciliate the westerners.³⁰

IV. *Recession, Revival, and the Attainment of Unity*

Thus the crisis passed, and the American Economic Association records contain no further evidence of rivalry with the Political Science Association. But despite continued efforts to meet the needs of western members,³¹ the organization was hardly thriving, and its failure to

²⁹ [1, Commons to Adams, Apr 6 1895]. Also [3, Clark to Jenks, Jan 21 1895]. Commons was subsequently forced to resign [9, p. 285] although Adams had strongly defended him in a letter to President J. Swain of the University of Indiana [1, Aug 22 1894].

³⁰ The selection committee comprised Taussig (Chairman), Farnam, R. P. Falkner (Pennsylvania), Ross (Stanford) and W. A. Scott (Wisconsin).

Recent research has tended to enhance Adams' reputation [2, pp. 3-55] [35, Ch. 7].

³¹ From 1898 the annual meetings were recommended in the *A.E.A. Handbook* partly because they ". . . counteract any tendency to particularism which geographical separation and the diverse traditions of American colleges might be deemed to foster." In this issue a geographical distribution of members was included for the first time.

expand caused serious concern between 1894 and 1899, the period immediately preceding Ely's presidency.³² There was a significant shift of emphasis which may have reflected the belief that "... political economy is swinging back to a renewed attention to practical or business affairs."³³ Although there appears to have been no serious effort to recruit new members until 1899, the 1896 *Handbook* made a general appeal for recruits, and from 1894 onwards the introductory description of the Association's character and purpose increasingly emphasized the width of its support among business and professional men.³⁴ In 1899, as an outcome of this tendency, there was a serious examination of the desirability of electing a businessman as president in succession to A. T. Hadley; but this proposal was rejected, partly because it would have represented too sharp a reversal of the former emphasis on the scholarly character of the Association. As J. B. Clark put it,

... we should remain a scientific body. ... At bottom even the phillistines will have more respect for such a body than they would for one that should put a man of affairs at its head. I may be wrong, but I dread any yielding to the view that economic wisdom resides outside of the schools and inside of the counting house [3, Clark to C. Hull, Dec 23 1899].

As it transpired, Ely's long-delayed return to the fold, followed by his unexpected election to the presidency,³⁵ provided a happy, if tem-

³² In 1894 the treasurer complained that membership was far below that of the American Statistical Association [3, F. B. Hawley to Jenks, Nov 5 1894]; and five years later Seligman (then treasurer) informed Willcox (then secretary) that the membership of the American Historical Association was both higher (1200) than that of the AEA, and increasing, adding that "... if we once cease to advance the crisis is not far off" [3, Feb 15 1899]. Other letters of 1899 refer to the "crisis" and the need for "careful attention and nursing." Membership had fallen from 781 in 1893 to 661 in 1894; and had only risen to 685 by 1898 and 745 by late 1899.

³³ [24, p. 55]. Giddings called on members "... to take a more prominent stand on questions of public policy" and "... to make the A.E.A. an increasingly powerful influence upon public opinion ... we must not impose upon ourselves a creed, or promulgate dogmas; but we should make it clear to the people that there is a vital difference between scientific and unscientific views of those great subjects" [24, p. 56 sentence order altered].

³⁴ Early in 1899 a circular was drafted for prospective members including the following phrases from Hadley's 1898 presidential address: "If we fail in our influence in public life we fail in what is the most important application of our studies ... The largest opportunity of our economists in the immediate future lies not with students but with statesmen. ... [In future the Association] will be of interest to thoughtful businessmen, newspaper men and holders of public office" [1, Willcox to Adams, Feb 16 1899]. Somewhat bluntly it was added that the limits of college membership had been reached!

³⁵ Although Ely had presided at the 1893 meeting in Chicago, he had subsequently held himself aloof until 1899, when he offered a paper. Indeed, he even allowed his membership dues to lapse for "5 or 6 years" [3, Willcox to Giddings, Dec 2 1897]. At the 1899 meeting Hadley, the president, selected him as chairman of the nominating committee on the assumption that in view of his recent absences he would hardly be a candidate for office. But, to the secretary's surprise, he was elected president [3, Willcox to S. M.

Lindsay, Jan 2 1900; Willcox to Hull, Jan 6 1900].

porary, solution to the dilemma. Whatever the estimate of Ely's scholarship, his inexhaustible energies and remarkable administrative gifts were enlisted at a most opportune time. Under his businesslike guidance and largely owing to his personal efforts, membership expanded sharply,³⁶ interest revived, and the Association prospered.

Yet complete unity was not attained until 1904 when, following a University of Chicago decision to invite the historical and economic associations to a joint meeting in their city, J. L. Laughlin at last applied for membership.³⁷ Trivial as this incident may now appear, it seemed a matter of some importance to the officials of the American Economic Association. It dispelled the fear that Laughlin might use his considerable influence as head of the Chicago department of economics to spread distrust and hostility towards the Association,³⁸ and by adding the support of the only contemporary economist of note who had hitherto held aloof, it enabled the economists to present a united front in their efforts to enhance the public prestige and influence of their professional discipline. In retrospect it is clear that Laughlin's action marked the final disappearance of the suspicion of the organization which it had encountered, in varying degrees, since its inception, and betokened its permanent establishment as a strictly scientific and scholarly body.

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³⁶ Membership rose from 745 in Dec 1900 to 950 in Dec 1901 during Ely's term of office, and to a peak of 1011 in 1902. Thereafter it remained at about this level until 1909, when the then secretary T. N. Carver conducted another membership campaign.

Of Ely's presidency Hull, the treasurer, wrote to Taussig: "I think that Ely, too, has the idea of distinguishing his administration by adding to the membership of the Association, and to judge from his initial success as Secretary, I fancy his design is not altogether hopeless of execution" [3, Mar 8 1900]. At the conclusion of Ely's term of office Hull wrote: "I feel that we really owe more as an organization to you than to any other President whom we have had since I have known anything about our affairs" (i.e., since 1893) [14, [Hull to Ely, Nov 22 1901]. Similarly [14, Fetter to Ely, Jan 11 1902].

³⁷ [14, Small to Ely, Jan 10 1904]. Jameson, secretary of the American Historical Association and professor of history at Chicago, spoke of Laughlin's genuine cordiality towards the AEA and the difficulty "... of persuading several scores or hundreds of members that his attitude is and would be a cordial one" [3, Jameson to Seligman, Nov 29 1902].

³⁸ However unjust to Laughlin, this feeling was real enough. Wesley Mitchell, then a promising young man, was especially invited to read a paper in 1902 in an effort to protect him from "bad influences" at Chicago [3, Fetter to Seligman, Oct 18 1902; Willcox to Hull, July 23 and 27 1900].

The abolition of the council, proposed in 1904 and effected in 1905, was a final explicit acknowledgement of the disappearance of the possibility of domination by a sect or group of reformers [15, p. 8].

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OPERATIONS RESEARCH

By ROBERT DORFMAN*

Once upon a time, within the memories of men who think they are still young, there was no such thing as operations research, or at least no such phrase. Today the Operations Research Society of America has more than 2,300 members, the Institute of Management Science has more than 2,600 members (there is considerable overlap, of course) and at a recent international conference on operations research no less than sixteen countries from four continents were represented. It is a flourishing movement.

The proper way to begin an inquiry into this movement would be to define it. But this is difficult; for operations research is not a subject-matter field but an approach or method. And, even after a study of hundreds of examples of work classified as operations research, it is by no means clear just what the method is other than that it is scientific (like all respectable methods), because operations analysts are typically resourceful and ingenious men who tackle their problems with no holds barred. I propose, nevertheless, to advance a definition; but it will help to prepare the way for that hazardous attempt if I try to convey the flavor of operations research by sketching a pair of contrasting caricatures, one of a conventional business consultant and one of an operations analyst.

(Suppose that a soap company seeks advice as to whether its advertising budget next year should be larger than, the same as, or smaller than this year. They might engage a business consultant who, in this case, would be a specialist in either advertising or marketing. He would have had substantial experience with advertising the same or similar products and would have at his finger tips a good many relevant data and, besides, would be familiar with all the standard sources of such data. In addition he would be aware of the maxim that it takes five cents worth of advertising to sell a dollar's worth of soap, though he would not necessarily take it very seriously. With this background at his disposal he would marshal the pertinent facts. He would examine the experience of his client and would correlate sales with advertising

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expenditures for past years, doing it by eye if his research budget were stringent or by least-squares if he anticipated a fee that justified a more imposing report. He might well consider data by states or marketing areas, for both his client and competing firms, and might also analyze sales and advertising exposure broken down by city size. If the project were at all large he would almost certainly have a field survey made, finding out from a random sample of housewives what soaps they bought and what advertisements they were aware of. Finally, with this information all carefully organized, he would determine the level of advertising expenditure appropriate to the budgeted level of sales. In all likelihood this figure would turn out to be fairly close to 5 per cent of anticipated gross sales, partly because you can't go far wrong by sticking close to an established norm, and partly because nearly all his data will be in this range with the exception of a few observations relating to attempts to launch brands that, for one reason or another, did not catch on, or relating to advertising campaigns that had been successful beyond reasonable hope of duplication. In short, he would arrive at a recommendation based on a reasoned scrutiny of past experience.

But now suppose that the soap company turned to an operations analyst for advice. Typically, this man or firm would have no particular experience with either soap or advertising, and would feel the need of none. More likely than not, the analyst would be a graduate physicist or mathematician. With this background at his disposal he would formulate the problem. The first words he would say to himself might be: "Let p_{xt} be the probability that the x th household will buy my client's soap during the t th week.¹ Then p_{xt} is the product of two factors: p'_{xt} , the probability that they will buy any soap during the week, and a_{xt} , the probability that if they buy any soap it will be my client's brand. Assume that p'_{xt} cannot be influenced by my client's advertising but that a_{xt} can be. In fact, suppose that the Weber-Fechner law applies so that the rate of increase of a_{xt} with respect to advertising expenditure is inversely proportional to the level of advertising expenditure already attained, i.e.,

$$\frac{da_{xt}}{dE} = \frac{c}{E}$$

where E is the level of advertising expenditure and c is a constant of proportionality. Then, integrating this differential equation, $a_{xt} = \log kE^c$ where k is a constant of integration to be determined from the data. Then $p_{xt} = p'_{xt} \log kE^c$ and total expected sales can be estimated by integrating this expression over the entire population (i.e.,

¹ In this and most later examples the mathematical details are not essential to the general discussion and may be skimmed.

all values of x). Thus, assuming p'_x and k to be given data, total sales can be estimated as:

$$s_t = \log kE^c \int p'_x dx$$

and

$$\frac{ds_t}{dE} = \frac{c}{E} \int p'_x dx.$$

Thus the optimal level of advertising can be found by finding the value of E at which the profit per dollar's worth of sales multiplied by this derivative equals 1, the cost of an additional dollar's worth of advertising.²² In short, he would arrive at a recommendation based on logical deduction from simple first premises which are a plausible approximation to the laws underlying the phenomenon being studied.

Each of these approaches is "scientific" according to its own canons, but they are quite different. We can characterize this difference by saying that the operations analyst, in contrast with the conventional business analyst has a strong predilection for formulating his problems by means of formal mathematical models. By a model I mean a symbolic description of a phenomenon in which its observable characteristics are deduced from simple explanatory first principles (i.e., assumptions) by manipulating symbols in accordance with the laws of some formal logic (usually ordinary mathematics).

Argument *ad hominem* is generally regarded as unscholarly, but in trying to characterize operations research it seems important to note who the operations analysts are. According to a survey conducted by the American Management Association [2], more than 40 per cent of operations analysts are engineers by training, another 45 per cent are mathematicians, statisticians, or natural scientists. It is only natural that the point of view in which these men are schooled should permeate operations research. The essence of this point of view is that a phenomenon is understood when and only when it has been expressed as a formal, really mechanistic, quantitative model, and that, furthermore, all phenomena within the purview of science (which is probably all the phenomena there are) can be so expressed with sufficient persistence and ingenuity. A second characteristic of men of science, amounting to a corollary of the first, is their preference for symbolic, as opposed to verbal, modes of expression and reasoning. These characteristics I take to be the style of operations research, and I define operations research to be all research in this spirit intended to help solve practical,

²² In constructing this fable I have followed the spirit of the only operations research study of advertising with which I am acquainted, but have carefully diverged from that study in all details.

immediate problems in the fields of business, governmental or military administration or the like.

There is an important corollary to the tendency of operations analysts to cast their thinking in terms of formal mathematical models. Operations research is not a descriptive science but a prescriptive one. Therefore the deduction of adequate descriptive models is only part of the task of the operations analyst. In the end he must come up with a recommendation for action and this requires that he know what the operation in question is intended to accomplish. We rather side-stepped this issue, in the fable at the beginning of this essay, by assuming that the objective of the advertising was to attain the maximum possible excess of gross sales over the sum of production costs and advertising expenditure. But the conscientious operations analyst does not so glibly assume the objective of the operation he studies; and the extensive literature devoted to studying the objectives of business enterprise shows that he is wise to be circumspect about this point. In the soap example, it may well be highly important to maintain total sales at a level that makes it worth while for retailers to stock the brand in question, even if the marginal net return is negative. The long run may have to be considered, if the brand has to establish or maintain a market position in the face of vigorous competition. In short the objectives of an operation are likely to be complicated, obscure, and composed of several incommensurable aspects. A major part of the task of the operations analyst is to construct a "measure of merit" for the operation he studies to accompany his formal description of it. The logical precision of the model enforces corresponding precision in expressing the objectives that the operation is intended to attain. The orthodox business consultant, on the other hand, is under no such pressure to formulate precisely the goals of the enterprise he studies.

When the operations analyst has formulated the model of his undertaking and the goals it serves he is still nearer the beginning of his analysis than the end. He must still particularize his model by estimating the values of the various "given" parameters that enter into it. For this purpose he employs, usually, more or less advanced statistical methods. Then he must solve the model, that is, find explicit relationships between the parameters under the control of his client, on the one hand, and the measure of merit on the other. When this has been done he is in a position to determine the optimal values of the decision parameters, to make his recommendations, and to try to persuade the management to adopt them.

In the next two sections we shall discuss a number of aspects of the problem of model formulation. Then we shall examine some of the problems and pitfalls of determining measures of merit or objective

functions. The concluding section is devoted to the conditions for successful operations research and to some general conclusions and remarks.

I. *Some Standard Models*

There are three aspects to model building, closely related to each other but requiring different skills. The first is straightforward description; expressing the situation under study in terms of the symbolism adopted. This involves inventing symbols for the various components of the system and writing down the relationships connecting them. Usually the symbolism adopted is algebraic and the basic relationships take the form of equations and inequalities. Frequently, though, block diagrams are used. Then the components are represented by labeled boxes and the relationships by lines connecting the boxes. Less frequently, a logical symbolism is employed, with the elements of the problem conceived of as classes or members of classes and the relationships being those of class inclusion, overlapping, and the like. In any event, the process is one of translating the real world situation into a set of abstract and simplified symbols. The result is an essentially tautological and sterile description of the problem, for it yields almost nothing more than definitions and identities.

The second stage we can call creative hypothesizing. This is the stage at which the motivational, behavioral, and technological assumptions are introduced. It entered the soap example in two ways. First in the selection of relevant variables, as when we decided that the price of the soap, competitors' advertising expenditures, the level of national income, the season of the year, and many other conceivable variables could all be omitted. Our fictitious operations analyst made the same kind of conjectural judgment when he decided that the probability that a household's total purchases of soap of all brands was uninfluenced by his client's advertising. But the most vigorous exercise of creative hypothesizing occurred when he conjectured the form of the relationship between advertising and the conditional probability that any soap purchased would be his client's brand. The first aspect of model formulation is a craft but the second is an art and introduces the crucial assumptions of the model.

The final aspect of model formulation is quantification: the assignment of numerical values to the parameters of the model. In the soap example these parameters are c and p'_{ai} , the latter being a different number, presumably, for each class of household. This last aspect is clearly a more or less involved problem in statistical estimation.

It is clear that the operations analyst works, basically, as an applied mathematician with some statistics and numerical analysis thrown in.

His principal tools, aside from his own judgment and imagination, are algebra, the calculus, differential equations, probability theory and statistics. In addition he has some special-purpose tools of his own.

In principle, each time a problem is referred to an operations analyst he could construct a tailor-made model to fit the case, and in practice he does so a large proportion of the time. But fortunately problems of essentially similar form arise repeatedly in widely differing contexts. Consider, for example, the problems of providing facilities for checking customers out of a supermarket, accommodating aircraft arriving at an airport, servicing locomotives at a repair depot, and meeting the needs of telephone subscribers. In each case the problem is to provide tolerably prompt service to a demand whose timing cannot be predicted exactly, by means of expensive facilities. This problem, in all its variants, is the problem of queuing theory. Similarly, the problem of allocating limited resources among a number of uses pervades businesses and administrative organizations of all kinds. This family of problems is the concern of programming, linear and otherwise. And in like manner, game theory, inventory theory, servo-mechanism theory all study types of problems that are likely to occur throughout a wide variety of business and administrative circumstances. The operations analyst has at his disposal a large and growing kit of such tools and is thus provided with an efficient apparatus whenever he recognizes that a particular problem fits into one of these tidy categories. Naturally these ready made models are the most communicable, teachable, and talked about aspects of the craft and tend to receive a disproportionate emphasis both in the public image of operations research and in professional instruction. They will also receive disproportionate emphasis here.

1. *The Linear Programming Model*³

Essentially linear programming is a mode of expressing the problem of allocating scarce resources that has the peculiar virtue of lending itself to statistical estimation and numerical solution. It applies when the activities of an enterprise are limited by a number of resources in limited supply and when these resources are to be allocated to a number of activities each of which absorbs them in proportion to its level of utilization. Each of the activities also contributes to the attainment of the objectives of the enterprise in proportion to its utilization. The problem is to discover a set of activity levels attainable within the resource limitations which leads to the maximum possible attainment of the objectives.

³ For a more complete discussion of linear programming from an economist's point of view see Baumol [6] or Dorfman [14].

One of the most frequent and successful practical applications of linear programming is to the scheduling of oil refinery operations.⁴ We can convey the flavor of the method by laying out a highly simplified example of this application. Suppose a refinery has 1000 units of blending capacity which it uses to produce regular and premium motor fuel (to be denoted by subscripts 3 and 4 respectively) from two grades of blending stocks (to be indicated by subscripts 1 and 2). Introduce four decision variables x_{13} , x_{14} , x_{23} , x_{24} where x_{ij} denotes the number of barrels of blending stock i devoted to the production of motor fuel j . Now consider the consequences of decisions regarding these four variables. The total input of blending stock 1 is $x_{13} + x_{14}$ and that of blending stock 2 is $x_{23} + x_{24}$. If the two stocks cost \$1.26 and \$1.68 per barrel respectively, the total cost of fuel inputs is:

$$1.26(x_{13} + x_{14}) + 1.68(x_{23} + x_{24}).$$

The total output of regular-grade gasoline is $x_{13} + x_{23}$, that of premium-grade is $x_{14} + x_{24}$. If the regular grade is worth \$4.20 per barrel at the refinery and premium grade is worth \$5.04 per barrel, the value of product is:

$$4.20(x_{13} + x_{23}) + 5.04(x_{14} + x_{24}).$$

By subtraction we find the excess of the value of outputs over the value of inputs to be:

$$2.94x_{13} + 3.78x_{14} + 2.52x_{23} + 3.36x_{24}.$$

We suppose that the objective of the plan is to make this number as large as possible.

Now turn to restrictions on choice. Suppose that the only quality specification to be considered is octane rating. The octane rating of any blend is a weighted average of the ratings of its components. Thus suppose that the octane rating of blending stock 1 is 75 and that of blending stock 2 is 93. Since regular-grade motor fuel is a blend of x_{13} barrels of stock 1 with x_{23} barrels of stock 2, its octane rating is:

$$\frac{75x_{13} + 93x_{23}}{x_{13} + x_{23}}.$$

Similarly the octane rating of the premium fuel is:

$$\frac{75x_{14} + 93x_{24}}{x_{14} + x_{24}}.$$

Now suppose that regular fuel must have an octane rating of at least 82 and premium fuel a rating of at least 88. Then we have the constraints on the decision variables:

⁴ See Charnes, Cooper, and Mellon [12] and Manne [28] for more realistic examples.

$$\frac{75x_{13} + 93x_{23}}{x_{13} + x_{23}} \geq 82,$$

$$\frac{75x_{14} + 93x_{24}}{x_{14} + x_{24}} \geq 88,$$

which are equivalent to:

$$-7x_{13} + 11x_{23} \geq 0,$$

$$-13x_{14} + 5x_{24} \geq 0$$

respectively.

Finally suppose that each barrel of regular fuel produced requires 1 unit of blending capacity, and each barrel of premium fuel requires 1.2 units. Then, since 1000 units are available in all, we have the capacity constraint:

$$(x_{13} + x_{23}) + 1.2(x_{14} + x_{24}) \leq 1,000$$

Gathering all these formulas we have the purely formal problem: Find x_{13} , x_{23} , x_{14} , x_{24} so as to make:

$$2.94x_{13} + 3.78x_{14} + 2.52x_{23} + 3.36x_{24}$$

as large as possible, subject to the restrictions:

$$7x_{13} - 11x_{23} \leq 0$$

$$13x_{14} - 5x_{24} \leq 0$$

$$(x_{13} + x_{23}) + 1.2(x_{14} + x_{24}) \leq 1,000.$$

The rest is arithmetic calculation.

Besides being of substantial practical importance, this example has a number of pedagogical virtues. In the first place it illustrates the flexibility of linear programming. Note that the numbers we had to choose, x_{13} , x_{14} , x_{23} , x_{24} were "activities" in only a very strained sense. In this context, what we would ordinarily think of as an activity would be blending one of the grades of motor fuel by a specified chemical formula (e.g., blending a barrel of regular fuel by using .6 barrels of blending stock 1 and .4 barrels of blending stock 2). Linear programming cannot take account of more than a finite and fairly small number of activities (say 200) when defined in the ordinary way, but with our particular choice of variables we have admitted to consideration an infinite number of activities, viz., all physically possible blends. The resource scarcities, also, represent resource scarcities in only a very extended sense. The "1000" is the quantity of a genuine limiting resource, but the two zeros of the right-hand side of the restricting inequalities are artifacts resulting from the manipulation of the quality specifications. This flexibility—the fact that the words "activity" and "resource limitation" do not have to be taken at all literally in setting

up a linear programming problem—is largely responsible for the widespread applicability of the method. Any problem that can be expressed by means of linear equations and inequalities is amenable to linear programming, whatever the physical interpretation of the variables and relationships. Indeed there are methods for incorporating nonlinear relationships, at the cost of substantially increased difficulty in computation.

One important limitation of linear programming has always been that the variables whose values are to be determined must be continuously variable, but recently methods have been developed for solving problems where the decision variables must be integers (e.g., the number of aircraft flights per day between two points).⁵ Another important limitation, on which less progress has been made, is that linear programming formulations do not allow for uncertainty.

Let us return to the blending example for another remark. A key theorem of linear programming states that there always exists an optimal solution in which the number of decision variables with positive values does not exceed the number of constraints, in this case three. Now suppose, for example, that the positive variables turn out to be x_{13} , x_{23} , x_{24} . This indicates that the premium fuel would be 100 per cent pure blending stock 2. But this cannot be optimal since the quality specification for premium fuel would be overfulfilled and money could be saved by adding in some of the cheaper blending stock. Clearly, no admissible combination of three positive values can be optimal in this problem, so there must be a solution with only two of the decision variables at positive value. This means that only one of the two products is made, i.e., the refinery produces either regular or premium fuel but not both. Which product should be made can be ascertained readily by computing which of the two yields a greater gross revenue per unit of blending capacity absorbed. Thus the arithmetic turns out to be even more trivial than it promised to be at first glance.

An even more common application of linear programming than the refinery blending problem is the so-called transportation problem, which arises whenever a standardized commodity is to be shipped from a variety of sources to a variety of destinations.⁶ Newsprint companies use it to decide which of their customers should be supplied from each of their mills, oil companies use it in assigning bulk distribution plants to refineries, the National Coal Board in England uses it to allocate markets among mines, Henderson applied it to an appraisal of the economic efficiency of the coal industry in this country [20]. There has accumulated a large literature which deals with such complications as

⁵ See Gomory and Baumol [19] and Markowitz and Manne [30].

⁶ For a typical small-scale example see Vazsonyi [39, p. 26 ff].

differences in production costs at different supply points, limitations on the capacity of shipping routes, fixed charges for opening or constructing shipping routes, and a cascading of problems such as where distribution involves a chain of factory to warehouse (of limited capacity) to retail store.

All the problems in this wide family are surprisingly easy to solve (though I wouldn't recommend that you try a large one with pencil and paper) for a number of technical reasons, among them the fact that all the restraints take the form of simple sums like: for any factory the sum of shipments to all its customers cannot exceed its capacity. As a result transportation problems involving literally thousands of origins and destinations can be solved readily. It seems that even more efficient methods of solution result when the linear programming point of view is abandoned and the problem is conceived of as routing a flow through a network, so that shipments of commodities are analyzed as if they were currents in electric circuits or flows of liquids through systems of pipes.⁷ To use electrical terminology, transportation costs are analogous to resistances, and differences in the value of the commodity at different points are the voltages.

2. *Information Theory*

Information theory rests on the discovery of a quantitative measure of information, originally by R. V. L. Hartley and put into its current form by C. E. Shannon [36]. The unit of information is the "bit," which is the amount of information in a message that tells which of two equally likely possibilities is the case. Thus the telegram "It's a boy" conveys (approximately) one bit of information to a receiver who knew previously that a birth had occurred. Or consider a kind of message that is more common in industry: a requisition sent by a production department to a store-room. It contains at least three kinds of information: (1) which item is desired. This selects one of a large number of not-equally-likely possibilities, but its information content is measurable in bits by a formula that we do not have space to explain. (2) The quantity desired—again a selection from a large number of not-equally-likely possibilities. (3) Which department desires the items—again quantifiable in the same way. The formulas for information quantity are constructed in such a way that the information content of the message is the sum of these three components (and any others, such as the date desired, that may be present). Of course, there are many messages whose information content cannot be measured. For example, I do not

⁷ The network approach to transportation problems is due mostly to Ford and Fulkerson. See [17] [18].

suppose that the information-content of this essay can be quantified.⁸ But equally obviously, the smooth running of any organization depends on the flow of messages that are sufficiently standardized so that their information content can be estimated numerically.

The metric for information content is accompanied by measures of the information-carrying capacity of channels and the information-processing capacity of centers. A center is any person or group who originates messages or receives and acts on them. A channel is any group of devices or persons that transmits messages between centers. E.g., a channel might be typist-messenger boy-receptionist-secretary; or microphone-transmitter-radio waves-receiver-loud-speaker).

The capacity of a center depends on the kinds of messages it processes, the kind of action or decision required, and the technical characteristics of the center. The capacity of a channel depends on the amount of interference present in it (i.e., irrelevant signals, technically called "noise"), the forms of the messages it handles, the rate at which it can transmit the elementary symbols of which messages are composed, its susceptibility to error, etc. The capacities of the physical components of a channel (e.g., electronic amplifiers) can often be estimated from engineering considerations, the capacities of the human components have to be derived from statistical studies.

The relevance of the form of message to the capacity of a channel is particularly significant. It is contained in the notion of "redundancy," i.e., the ratio of the actual information content of the message to the maximum amount that could be packed into a message of the same length (subtracted from unity, of course). Thus the information content of the vital statistic telegram mentioned above is entirely contained in the zero-redundancy message "B" or the somewhat more redundant "Boy." Redundancy of form is expensive, but it is also useful, up to a point. Thus if a channel is subject to error (as all are) redundancy often saves the day. The message "F" conveys no information, but "Foy" is practically as good as the correct message. Mail order companies recognize the virtue of redundancy when they require that an order include the name of the item desired along with the catalogue number. The optimal amount of redundancy in a system, i.e., the optimal form of message, is an important field of application of information theory. For example, the ordinary method of transmitting television signals is more than 99 per cent redundant (because it is practically certain that the spot following a bright spot will also be bright; the only information needed after a few bright spots is when the first dark spot

⁸ But an upper limit can be placed on it by using the estimate that text written in English contains about 1.5 bits per letter.

occurs). One of the more important advances that made color television feasible, since color TV requires much more information per second, was a less redundant scheme for encoding the signals for transmission.

The relevance of information theory to communications engineering is evident but would not qualify it as a model for operations research. Its contribution to operations research derives from its bearing on the structures of organizations. Here the essential principle is that an organization will not function smoothly if the amount of information to be sent on any channel or processed at any center exceeds the capacity of that channel or center. Thus the information structure of an organization can be depicted by representing it as a kind of network with the centers shown by capacitated nodes and the channels by capacitated links. Then the required flow of information between any pair of centers can be compared with the maximal attainable flow.⁹ Bottlenecks as well as underutilized channels can then be detected and corrective actions indicated. Possible ameliorative measures are: (a) increasing physical channel capacity (e.g., increasing the number of trunk lines), (b) reducing the redundancy of messages (which is always unduly high in organizations that have not given conscious attention to this problem), (c) routing messages indirectly via underutilized channels. (d) increasing the capacities of centers (e.g., installing more counters in the store-room), and (e) reassigning functions away from overloaded centers.

3. *General Systems Analysis*

General systems analysis or cybernetics is fairly closely related to information theory and indeed incorporates it. Like information theory, it visualizes an organization in terms of a block diagram with centers connected by lines of communication. The separation of centers entails that no center will have all the information available to the organization (contrast with the ideal entrepreneur or firm of economic theory), and that there will be delays (lags) in transmitting information to centers where decisions are made and in transmitting and executing instructions from executive centers. This circumstance gives rise to the problem of systems analysis: to deduce from the structure of information flows in an organization and the decision rules employed by its various centers how the organization will respond to changes in its environment and conditions. As a corollary, systems analysis is concerned with methods of ascertaining the information flows and decision rules in organizations

⁹ From this point of view Paul Revere's communication system (two lanterns, one if by land, two if by sea) was just adequate. There was one bit of information to be transmitted and his channel had a capacity of one bit per night. It was insufficiently redundant, however, and if there had been much noise (mist, gusty winds) the course of the Battle of Lexington might have been other than it was.

—which often differ from those in the official organization chart—and with finding optimal information flows and decision rules.

The fundamental assumption of systems analysis is that all decision rules can be thought of as responses to discrepancies between the actual state or performance of the organization and some “standard” or desired state, and are intended to reduce those discrepancies. Now it has been noted that many mechanisms behave in just this way (the conventional examples are gyro-pilots on ships and thermostats attached to home heating systems) and, indeed, so do the nervous systems of most organisms. Thus automatic control devices, neuromuscular responses, and organizations are all recognized as instances of control by feedback, i.e., by comparison of actual state with desired state. The mathematics and concepts developed for the study and design of electronic control devices thus become applicable to the study of nervous systems and organizations, indeed the “servomechanism” becomes the model employed in all such studies.

Consider a very simple example which evades all the interesting but complicated mathematical concepts that dominate this area of operations research. Suppose a firm has the policy of keeping 100 units of some item in inventory. Each Monday the stock-room reports to the purchasing department its actual stock as of the close of business the previous Friday. If the stock is less than 100 units the purchasing department orders an amount equal to $\frac{3}{4}$ of the discrepancy, which is delivered 8 to 10 days later. Sacrificing realism to simplicity we also assume that if the stock exceeds 100 units the purchasing department disposes of $\frac{3}{4}$ of the excess, say by sale to a subsidiary at a sacrifice price. This disposal (or negative delivery) also takes place in the week following the report of the discrepancy. This very bad inventory policy can be portrayed as in Figure 1. This diagram is interpreted as follows.

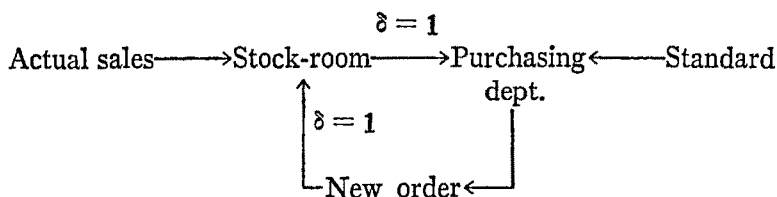


FIGURE 1. FLOW DIAGRAM FOR A STOCK CONTROL SYSTEM

The condition of the stock room is influenced by sales and new orders, the formula being:

$$I_{t+1} = I_t + R_t - S_t$$

where

I_t = inventory at the beginning of the t th week

R_t = replenishment stocks received during the t th week
(which may be negative)

S_t = sales during the t th week.

The condition of the stock-room is reported to the purchasing department with a delay to the following week, symbolized by $\delta = 1$. The purchasing department compares this report with the standard (100 units) and issues a replenishment order according to the formula:

$$\text{Order} = \frac{3}{4}(100 - I_t)$$

This order reaches the stock-room the following week and influences the terminal stock according to the inventory balance equation. Now:

$$R_t = \text{Orders in week } t - 1 = \frac{3}{4}(100 - I_{t-1}),$$

so we can write the difference equation for inventories as:

$$I_{t+1} = I_t + \frac{3}{4}(100 - I_{t-1}) - S_t$$

or:

$$I_{t+1} - I_t + \frac{3}{4}I_{t-1} = 75 - S_t.$$

This is a familiar kind of difference equation from which we calculate how the level of actual inventories will respond to any pattern of sales. The solution, skipping over the tedious details, is:

$$I_t = k(.866)^t \cos(55t + \phi)^\circ + 1.06 \sum_{\tau=0}^{t-1} (.866)^\tau \sin 55\tau^\circ (100 - \frac{4}{3}S_{t-\tau})$$

where k and ϕ are disposable constants to be assigned in accordance with the initial conditions. As would be expected from a second-order difference equation, this solution displays a combination of exponential and trigonometric behavior. It consists of two terms. The first term is a damped cycle, but the notable aspect of it is that it does not depend at all on the sales or the desired level of inventories. It is a damped oscillation caused by the lags and decision processes in the system itself. One of the virtues of systems analysis is that it calls attention to such built-in tendencies of organizations to oscillate. Though this firm allows a bad inventory policy, it is not as bad as it might be; for at least the inherent oscillations it generates do tend to die out.

The second term in the formula is a peculiar weighted average. To interpret it imagine first that sales were equal to zero for a long enough sequence of weeks so that the transient died out and inventories no longer changed. Then we should have:

$$I_{t+1} = I_t = I_{t-1}$$

and, substituting in the difference equation we should find $I_t = 100$, as

desired. But if sales were steady at any nonzero level, the steady-state level of inventories would be:

$$I_t = 100 - \frac{4}{3} S_t.$$

Thus each of the terms in parentheses in the weighted average is the steady-state level of inventories corresponding to the level of sales τ weeks previous to the date for which the calculation is made. The whole term averages these historical steady-states with weights that decline generally as we recede into the past. This term, also, is characteristic of the responses of a feed-back system—their current behavior is a weighted average of responses to past stimuli. The scheme of weights, in this case $(.866)^t \sin 55t^\circ$, is one of the important traits characterizing an organization or mechanism.

Our example was so simple that we determined the inherent oscillations and the weighting function by straightforward, elementary means. In this respect the example was atypical of systems analyses. More generally, fairly advanced mathematics (in particular, Laplace transform methods) are required; and still more generally an explicit general solution cannot be obtained at all, though the formulation without solution may be illuminating. We shall return below to the treatment of problems that defy explicit solution.

4. *Inventory Models*

Churchman, Ackoff and Arnoff state, "More O. R. has been directed toward inventory control than toward any other problem area in business and industry" [13, p. 195], and this may be true. The literature devoted to inventory problems is enormous and even the early work in the field, contributions published thirty years ago and more, displays the mathematical-engineering approach characteristic of operations research. The purpose of all this work, of course, is to determine most-profitable inventory policies. It takes it for granted that inventories are held to facilitate production operations (including within "production" the activities of purely trading firms). Though holding inventories is costly it is generally essential to the economical conduct of other operations. The methods we are now concerned with seek to determine the efficient levels of these buffer stocks.¹⁰

Because of the enormous variety of inventory problems—style goods vs. standardized goods, goods purchased from outside suppliers vs.

¹⁰ Quite clearly these studies of inventory problems have only slight connection with the aspects of inventory behavior that typically attract the attention of economists. Economists typified by Abramovitz, Hawtrey and Metzler concentrated on the aggregative consequences of inventory fluctuations and on the speculative motives for changes in stocks, with relatively light attention to the more work-a-day motives. Whitin, however, has tried to bridge the gap [40].

goods manufactured within the firm, goods obtained under conditions of increasing, decreasing or constant costs, seasonal goods vs. nonseasonal ones, predictable vs. unpredictable demand, situations where stock-out penalties are severe vs. those where they are negligible, etc.—there can be no simple, generally applicable body of sound inventory doctrine. Instead, “inventory theory” consists of a battery of techniques for deducing efficient inventory policies adapted to specific circumstances. These techniques can be divided into three classes that I shall call static analyses, stationary-state analyses, and dynamic analyses, in order of increasing sophistication.

Static analyses concentrate on average behavior in a single period considered in isolation. A typical and important result is the “square root law,” which can be deduced, in a very simple context, as follows. Suppose that the average weekly requirement for some inventoried item is n units, that it costs $\$k$ to place an order for this item over and above the cost of the goods ordered, and that the carrying costs are $\$c$ per unit per week. Suppose that reorders are placed in time so that the new stock arrives just when the old stock is exhausted. (The reader can see the difficulties hidden in this assumption when delivery lags are at all appreciable.) Then the only decision to be made is the quantity to order when an order is placed. Call it x . On these assumptions, n/x orders will be placed per week, on the average, giving rise to an average weekly order cost of $\$kn/x$. The size of the inventory will range from x units, just after new stock has been received, down to zero units just before replenishment, averaging at $\frac{1}{2}x$ units. The associated carrying cost is then $\$\frac{1}{2}cx$ per week and the total weekly cost of maintaining the item in inventory is $\$kn/x + \$\frac{1}{2}cx$. The optimal value of x is the one that minimizes this cost, and is easily found to be

$$x = \sqrt{2 \frac{kn}{c}}.$$

Thus the optimal size of order and of average inventory, in these circumstances, varies in proportion to the square root of (i) the rate of consumption or sales, and (ii) the ratio of ordering cost to carrying cost.

The reader is surely unpleasantly aware of how many simplifying assumptions were made, implicitly and explicitly, in the course of this derivation. Most of them can be dispensed with, at the cost of increasing the complexity of both the argument and the result. Schlaifer [34, Ch. 15], for example, presents a full discussion of a case in which demand during the interval between placement and receipt of an order is a random variable, so that there is substantial danger of being out of stock before the replenishments are received. The general approach, the

calculation of average results, is the same but the technical difficulty of the analysis is magnified manyfold.

An essential prerequisite for using static analysis is that it be possible to divide up time into a sequence of independent periods. These periods need not all be of the same length or even of predictable length, but they must be independent in the sense that no decision or event in any period affects the situation in any subsequent period. In the preceding analysis, for example, we can define a period as extending from one date at which the stock is precisely x , the order quantity, to the next date at which this is the stock. Then periods will be of varying length, depending on fluctuations in rate of consumption, but at the outset of each period the position of the firm will be precisely the same as at the outset of any other period and the consequences of any inventory policy can be synopsized by taking the average results of that policy in a single period. That is just what we did.

Unfortunately it is not always possible to divide time up into such independent periods. For this there are several reasons. It may not be possible to order new stock just when the level of the inventory makes it desirable. Thus, canneries must contract for produce once a year, ships can replenish only when in port, etc. Sometimes even when irregular ordering is technically possible it may be undesirable. It may be administratively necessary to establish a periodic ordering routine, or it may be economical to consolidate orders for a variety of items into a single requisition. In such cases a schedule of ordering dates has to be established, and they become the natural points of division between time periods because they are the dates on which decisions have to be made. When a regular ordering cycle is established the periods cannot be independent because each period's initial stock is influenced by what went on before.

But independence may fail even when irregular ordering is permitted.¹¹ For example, if stock is exhausted orders may be backlogged. Then when new stock is received its utilization does not begin *ab initio* but is influenced by the extent of the backlog. Thus in many actual situations inventory policies have to be devised without making the assumption that time can be sliced into a sequence of independent periods. Then it is invalid to appraise an inventory policy by its average results in a single period; more complicated techniques have to be used.

Stationary-state analyses comprise one family of such techniques. If we contemplate a long sequence of periods extending into the future we can consider the probability distribution of the state of the firm (e.g., the size of inventories and backlogs) at the outset of each period. In the first period, of course, the initial state is known. The probability

¹¹ Howard Raiffa taught me this.

distribution of the initial state of the second period depends on the initial state of the first period, the inventory policy followed, the probability distribution of demand (which we shall for simplicity assume to be the same in all periods) etc. The third period probability distribution depends on the second period probability distribution, the inventory policy, the probability distribution of demand, etc.¹² If we continue in this manner it fortunately happens that the influence of the initial conditions of the first period gradually dies out and the probability distribution of initial states generally ceases to change from period to period. In other words, a stationary state is attained.

Once the stationary state has been reached we can apply the averaging procedure, in two steps. First we consider each possible initial state in turn and compute the average results of the inventory policy in a period that begins with that initial state. Then we average those results over all possible initial states, giving each a weight equal to its probability. The result is an over-all average profit (or cost, if that is desired) per period in the long run.

TABLE 1—PROBABILITY DISTRIBUTION OF MONTHLY DEMAND FOR A PART

Number of times demanded	0	1	2	3
Probability	.5	.3	.1	.1

All of this sounds very complicated, and is, but a simple example may clarify it. Consider the problem of a machine shop that stocks a part that is expensive to store and is infrequently demanded. When it is demanded the shop gets a job if the item is in stock and nets, let us say \$15; but if it is out of stock the sale is lost. The carrying cost for the part we take as \$3.00 a month. Inventory is taken on the first month and if an order is placed the new stock is received on the first of the following month in time to be included in that month's inventory. The probability distribution of demand is given in Table 1.

With these data the consequences of any inventory policy, in the stationary state, can be computed. Three possible inventory policies are listed in Table 2. We shall consider policy I in detail, it being the least tedious. First note that if this policy is followed there can never be more than one part in stock. Next write down a table of "transition probabilities." These give the probabilities of either possible inventory level, 0 or 1, at an inventory date when the inventory level at the previous date is given. These are displayed in Table 3.

¹² It may also depend directly on events in the first period, as when delivery lags are longer than a period or when usage in any period affects the probability distribution of demand in several subsequent periods. We have ample precedent for neglecting this kind of complication.

TABLE 2—THREE POSSIBLE INVENTORY POLICIES; NUMBER OF PARTS TO ORDER WHEN CURRENT INVENTORY IS GIVEN

Current Inventory	Number of Parts to Order Under		
	Policy I	Policy II	Policy III
0	1	1	2
1	0	1	1
2	0	0	0
3 or more	0	0	0

This table states that if inventory is zero on one inventory date it will surely be one on the next (since one part will be ordered and none will be sold), and that if inventory is one on any date then the chances are 50-50 that it will be zero or one on the next date. Now let P_0 and P_1 denote the probabilities of the two inventory levels and assume that the stationary state has been reached, so these probabilities are constant over time. Then, from the first column of Table 3, the probability of having no items in stock on any date is half the probability of having one in stock on the previous date, or $P_0 = .5P_1$. From column 2, the probability of having one item in stock is the sum of two contingencies: none in stock on the previous date plus one in stock on the previous date and still unsold. Thus $P_1 = P_0 + .5P_1$. This, of course, is the same equation as before. But the two probabilities have to add up to unity. Thus $P_0 = 1/3$, $P_1 = 2/3$ are the steady-state probabilities of the two possible inventory levels.

If inventory is zero, there are neither sales nor inventory costs. If inventory is one, inventory costs of \$3 are incurred and there is a 50 per cent chance of making a sale, so that average profit is $1/2\$15 - \$3 = \$4.50$. The long-run average profit per period resulting from this policy is then $1/3(0) + 2/3\$4.50 = \3.00 . The other two policies in Table 2 can be assessed similarly. For policy II the steady state probabilities are 1/11, 5/11, 5/11 for stocks of 0, 1, 2 respectively, with an average profit per period of \$4.09. For policy III the probabilities are 1/9, 3/9, 5/9 and the average profit is \$4.00. Thus policy II is the best of the three.

TABLE 3—TRANSITION PROBABILITIES FOR POLICY I

Previous Inventory	Current Inventory	
	0	1
0	0	1
1	.5	.5

This brings up three comments. First, the policy is a bit peculiar. It tells us that when stocks are down to zero we should order one part and then, whether or not it is still unsold at the end of the month, order another. Why not order the two together? Because inventory carrying costs are too high to justify carrying a second item in stock when there is sure to be a first item, but are not too high to risk carrying a second item when there is only a 50 per cent chance of having a first. Second, there is no unique optimal stock level in this example. If the part is out of stock we should order up to a stock level of one; if the stock level is one we should order up to a stock level of two. Now, much of inventory theory presumes that there is an optimal stock level and attempts to find it. In this case such a search would not discover the optimal available inventory policy.¹³ Third, we have considered three policies and have found the best of the three, but how do we know that there is not a still better policy? Stationary-state analysis will not tell us, because the computations require the table of transition probabilities and these, in turn, require that the inventory policy be given. This mode of analysis is convenient for assessing the consequences of any given policy; it is inconvenient for discovering an optimal policy except in some special cases.¹⁴ Thus we must advance to full-fledged dynamic analysis, which meets this need.

Dynamic inventory analysis rests on two ideas, both most thoroughly expounded by Bellman [7]. The first is a recurrence relationship connecting optimal inventory policies for different planning horizons. Suppose that on a given inventory date the stock is N and it is desired to take into account the consequences of any decision for H periods into the future. (H may be infinite but it will do no harm to talk as if it were finite.) Since we plan to take account of consequences that may extend over a considerable length of time, time preference becomes relevant. Therefore, assume that the current valuation placed on a dollar t periods in the future is $\$a^t$, i.e., assume a rate of discount of $(1-a)/a$ per period.

Suppose it is decided to order x units. The consequences of this decision can be divided into two parts: its effects on the present period and its effects on the $H-1$ subsequent periods. The effects on the current period are the average level of profits in a period with initial inventory N if x units are ordered. This value can be ascertained by the kind of calculation we have already illustrated. Denote it by $r(N, x)$. The effect on later periods is a bit more complicated. One cause of com-

¹³ Dvoretzky, Keifer and Wolfowitz [16] present a full discussion of the conditions in which an optimal stock level exists.

¹⁴ Situations in which an optimal stock level exists are among such special cases. Morse [31, Ch. 10] discusses the use of stationary-state analysis for finding optimal stock levels.

plication is that we do not know what the initial inventory of the second period will be. We can, however, calculate its probability distribution from the probability distributions of demand and deliveries in the first period. Accordingly, let $p_M(x)$ be the probability that the inventory inherited from the first period will be M if x units are ordered in the first period. We now assume, and this assumption will be justified later, that we already know the optimal inventory policy for an $H-1$ period horizon. That is, we know the maximum discounted profit that can be obtained in $H-1$ periods beginning with an inventory of M . Denote this number by $f_{H-1}^*(M)$. Then, taking the discount factor into account, the total present value of profit in H periods beginning with an inventory of N and ordering x units is $r(N, x)$ plus the average $H-1$ period profit taken over all values of M , or:

$$f_H(N, x) = r(N, x) + a \sum_M p_M(x) f_{H-1}^*(M).$$

Let x^* be the value of x that maximizes this expression. Then the maximum value of $f_H(N, x)$ is $f_H(N, x^*)$, and this is $f_H^*(N)$ in the notation previously introduced. We thus have the recurrence formula:

$$f_H^*(N) = \max_x \left\{ r(N, x) + a \sum_M p_M(x) f_{H-1}^*(M) \right\}$$

where all the numbers inside the braces are known. Thus we can find the value $f_H^*(N)$ and the corresponding optimal order quantity, simply by trying all permissible values of x and noting the one that maximizes the value in the braces. This may be laborious but it is a practicable undertaking by hand for small problems and by electronic computer for large ones.

The second basic idea of dynamic inventory analysis is that if we determine optimal inventory policies successively for planning horizons of 1, 2, 3, \dots , periods, the sequence of policies will converge to the long-run optimum.

To see how this approach works let us return to the machine-shop example. For a planning horizon of one period $f_1(N, x) = r(N, x)$. But in this example, the placement of an order does not affect the current period, i.e., $r(N, x)$ is a constant in x . Let us then take the optimal

TABLE 4— $f_1^*(N)$ OR $r(N, 0)$ FOR THE MACHINE-SHOP EXAMPLE

N	$f_1^*(N)$	N	$f_1^*(N)$
0	\$0	4	\$0
1	4.50	5	-3.00
2	4.50	6	-6.00
3	3.00		etc.

value of x to be zero, whatever N may be. Thus $f_1^*(N) = r(N, 0)$. Table 4 is a table of this function computed from the previous data on costs and the probability distribution of demand.

Next consider a planning horizon of two periods, using a discount factor of $a = .99$. Applying the basic formula for the consequences of ordering x units when N are on hand,

$$f_2(N, x) = r(N, x) + .99 \sum_M p_M(x) f_1^*(M).$$

For every possible value of N we now require the value of x that makes this expression as large as possible. The ensemble of these values of x will be the optimal inventory policy for the two-period case. The calculation is protracted, so we illustrate it for a single value, $N = 1$. In this case there is a probability of .5 that the part on hand will be sold. Thus $r(N, x) = \$4.50$ for all values of x and $M = x$ with probability .5 and $M = x + 1$ with probability .5. Substituting these results in the formula:

$$f_2(1, x) = \$4.50 + .99[\frac{1}{2}f_1^*(x) + \frac{1}{2}f_1^*(x+1)].$$

The values of $f_1^*(x)$ and $f_1^*(x+1)$ for all values of x can be read off from Table 4. Thus we find that $f_2(1, x)$ assumes its maximum value when $x = 1$, so that

$$f_2^*(1) = f_2(1, 1) = \$4.50 + .99(\frac{1}{2}\$4.50 + \frac{1}{2}\$4.50) = \$8.955.$$

The calculation for other values of N is similar. Then, having found an optimal inventory policy and its results for $H = 2$ we advance to $H = 3$, which can be analyzed using the results for $H = 2$ and the fundamental recurrence formula. After that we go on to $H = 4, 5$, etc. Table 5 summarizes the results of a number of these computations which, remember, are readily mechanized. The entries in this table, of which we computed the one for $N = 1, H = 2$, are the optimal size of order for the given initial stock and planning horizon. Note that in this instance the optimal policy is the same for all horizons of three months

TABLE 5—OPTIMAL INVENTORY POLICIES FOR A NUMBER OF INITIAL INVENTORIES AND PLANNING HORIZONS
(Number of parts to order with given initial stock)

Initial Inventory	Planning Horizon in Months				
	1	2	3	4	5
0	0	1 or 2	1	1	1
1	0	1	1	1	1
2	0	1	0	0	0
3	0	0	0	0	0
4	0	0	0	0	0
5	0	0	0	0	0

and greater. The convergence of the short-horizon optimal policies to the long-run optimum is not always this fast, of course.

As this illustration shows, dynamic inventory analysis leads straightforwardly to the discovery of the optimal inventory policy for given data. Its major drawback, aside from its laboriousness, is that it does not lend itself to analysis. It just gives the answer without disclosing how the optimal policy would change in response to changes in any of the data. If any of the data (such as prices, probability distribution of demand, delivery lag, etc.) should change there is nothing to do but to perform the whole computation over.¹⁵

II. Ad Hoc Models, Simulation

1. Two More Models: *Queuing and Sequencing*

Limitations of space and time fortunately prevent me from extending this catalogue of methods used in operations research. There are, however, two more important families of models that must at least be mentioned: queuing theory models and combinatorial models.

Queuing problems occur very widely: the unanswered letters in your desk are a queue, so are the aircraft waiting to land at a busy airport and the machines standing idle waiting for the repairman. In general, any backlog of tasks waiting to be performed is a queue. (The tasks may be very simple, like admitting passengers through a bank of turnstiles, or they may be complicated, like treating casualties after a battle.) Queues are generally unpleasant to the people who are waiting for the service, and frequently expensive. On the other hand the provision of additional or improved serving facilities to reduce or eliminate the queue is also expensive. The task of queuing theory is to determine the optimal quantity and characteristics of the facilities that serve a queue, having regard to both of these costs. It performs this task by studying the way in which the serving facilities influence the probability distribution of the length of the queue, the probability distribution of waiting times, etc., treating the arrivals to the queue as a given datum.

Queuing problems are closely related to inventory problems. Both models are concerned with accumulations (of tasks or of stocks as the case may be), with accretions to them and with subtractions from them. In inventory problems the accretions are subject to at least partial control, through reordering policy, but the subtractions are governed by a random process that is largely beyond the control of the firm or organization that maintains the inventories. In queuing problems the

¹⁵ Exceptions to this dictum occur when the probability distribution of demand and other data are sufficiently tractable. A few instances are given in Arrow, Karlin and Scarf [4].

reverse is true: the accretions are random and beyond direct influence while the subtractions can be controlled through control of the serving facilities. Thus it is not surprising that the analysis of queues depends on the same general principles and methods as the analysis of inventories though, of course, there are substantial differences in detail.

The most interesting general conclusion yielded by queuing theory is the following: Consider a queue attended by a single server. Suppose that tasks arrive, on the average, one in every a minutes and require, on the average, b minutes to perform. Then in a fairly long interval, of length T , say, approximately T/a tasks will arrive, the server will be busy Tb/a minutes, and the average proportion of the time that the server will be busy will be b/a . This ratio, known as the utilization factor, is the key to the behavior of the queue. If it exceeds one, and the system doesn't break down, then obviously the length of the queue will grow towards infinity and so will the average waiting time. It is a little shocking to learn that this same result holds if the utilization factor equals unity, but this fact seems more reasonable when we recall that the queue is caused by the fact that the number of arrivals in any time interval is a random variable and the variance of this variable increases indefinitely as the length of the interval increases. Finally, if the utilization factor is close to unity, the average waiting time will be finite but large. It follows that if the queue is to be kept short, the utilization factor must be substantially less than unity, i.e. the server must be idle a large proportion of the time. Thus, ostensibly idle capacity is essential to prompt service. Old-fashioned efficiency experts will please take notice.

Combinatorial or sequencing models are used most frequently to describe production problems in which the decision to be made concerns the order in which certain operations are to be performed. Typical applications are decisions as to the order in which the parts of a complicated product are to be assembled (e.g., it is obvious that you should put on your socks before your shoes, but less obvious whether the socks should precede the trousers) and, in a job-shop, the order in which jobs should be performed (e.g., if black and white paint are to be mixed on the same machine it is usually better to mix the white paint first). Another group of combinatorial problems concerns the assignment of tasks. Thus, on a moving production line the various operations should be assigned to the various stations so that (a) operations are not performed in an awkward order, and (b) the operations assigned to all stations should require, as nearly as possible, the same length of time. The hallmark of a combinatorial problem is that it cannot be formulated as the choice of a value of a quantitative variable. Thus the most powerful tools of mathematical analysis, algebra and the calculus, do

not apply and problems that appear quite innocent on the surface are likely to prove very intractable. Nevertheless combinatorial problems can frequently be solved, sometimes, surprisingly, by an adaptation of linear programming.

2. Ad Hoc Models

I am fighting against giving the impression that this list of ready-made models, or any similar list, can include the bulk of the conceptual frameworks used in operations research. On the contrary, most operations research work employs *ad hoc* models, exemplified by the one in the soap advertising fable. In order to redress somewhat the balance of emphasis I must therefore sketch, very cursorily, two illustrations of *ad hoc* approaches, both taken from the very valuable compendia, *Operations Research for Management* [26] [27].¹⁶

The first illustration concerns the efficient utilization of the vehicular tunnels operated by the Port of New York Authority, one of the prominent users of operations research [33]. The problem was to determine the traffic conditions conducive to the largest feasible flow of traffic through the tunnels, in vehicles per hour. The volume of traffic, V , in vehicles per hour, is the product of traffic density, D (vehicles per mile), and speed, S , in miles per hour, as can be seen by noting that if traffic moves at S miles per hour all the vehicles within S miles of a given point will pass that point in an hour. Speed and density are closely related. It was assumed on the basis of previous empirical observations that the relationship was linear and, indeed, traffic counts in the fast lane of the Lincoln Tunnel led to the regression equation $S = 42.1 - .324D$ with a correlation coefficient of $r = .97$. Multiplying both sides of this equation by D therefore yields $V = DS = 42.1D - .324D^2$. Differentiating this expression with respect to D and setting the derivative equal to zero produces an optimal density of $D = 65$ vehicles per mile, corresponding to a speed of 21 miles an hour and a vehicular spacing of 81 feet. How to persuade drivers to adopt that speed and spacing is another problem, not dealt with in this report. A minimum speed of 20 miles an hour was posted and had a discernible effect in increasing the flow of traffic.

This project, of course, employed a very simple model, invoking only the empirical functional relationship between speed and density and the definitional relationship of those two variables to volume. The bulk of the work in this case was observational and statistical: observing and analyzing traffic conditions in the different tunnels and lanes. But the model, primitive though it was, was at the center of the project, dictating what was to be observed and how the results were to be used.

¹⁶ Summarized with the kind permission of the publishers, The Johns Hopkins Press.

My second example is inherently more tedious to describe because I have selected it in order to illustrate the kind of elaborate technical and technological analysis that is a part of the work of operations research. I shall therefore, in the interest of brevity, do considerable violence to the actual facts of life. The reader who wishes a more accurate description of the project is referred to the original report by Dunlap and Jacobs [15].

The project dealt with strip mining of phosphate rock. This is accomplished by enormous power shovels called draglines, costing over \$7 million each, with buckets of as large as 30 cubic yards capacity, and earth-moving capabilities of as much as 1,500 tons an hour. In operation they excavate a linear strip of ore that can be several hundred feet wide by taking a certain stand, excavating the ore that can be reached conveniently from that stand, then backing up a distance along the strip, excavating again, and so on. The shape of the hole dug at each stand may be visualized as a piece of pie with the point cut off or as a segment of a doughnut. But, and now we come to the problem, both the geometrical shape and the dimensions of the excavation made at each stand are variable within very wide ranges. The problem was to ascertain the optimal shapes and dimensions to employ, depending on the width of the ore vein, the thickness of the ore vein, and its depth below the surface. More specifically, the problem was to find the mode of operation of the dragline that would maximize the tonnage excavated per hour.

The opposing considerations, severely simplified, were as follows. On the one hand, a dragline is an unwieldy vehicle, to say the least. It moves slowly, cannot dig while it is being moved, and what is more to the point, the time required to prepare it for moving and to unlimber it after each move is considerable. Thus it is desirable to move it as infrequently as possible. On the other hand, as we shall soon see, the rate of excavation per hour is adversely affected if the area excavated at each stand is too large. The analysis of this second effect was the heart of the problem. To introduce the quantitative concepts let V denote the volume excavated at a single stand. Then, as you may recall from elementary calculus, if the shape of the excavation is a segment of a doughnut

$$V = \int_{r_1}^{r_2} \int_{\theta_1}^{\theta_2} \int_{z_1}^{z_2} r \cdot dz \cdot d\theta \cdot dr.$$

In this formula r denotes the distance from the dragline cab to an elementary volume of ore and varies from r_1 , the radius of the inner rim of the doughnut, to r_2 , the radius of the outer rim. θ indicates the angle from the cab to the elementary volume of ore, measured from the center line of the strip being excavated. It varies from θ_1 , the maximum

angle of swing to the right, to θ_2 , the maximum swing to the left. Finally, z is the depth below ground level of the elementary volume of ore, varying from z_1 , the depth of the top of the vein, to z_2 , the depth of its bottom. Of these variables, z_1 , z_2 and the width of the vein are determined by geological happenstance while r_1 , r_2 , θ_1 , θ_2 are subject to decision within wide ranges. The problem is thus to determine optimal values of the last four variables and, moreover, since it is not necessary to cut in doughnut shape, the entire geometric configuration of the excavation. This last, nonquantifiable, range of possibilities helps make the problem really fascinating, but we shall for simplicity continue to pretend that the excavation will be doughnut-shaped.

We now introduce the considerations that determine the time required to excavate a volume V from a given stand. The elementary cycle of operation begins when a load has just been discharged at the dumping point on the rim of the excavation. It consists in swinging the bucket back to the elementary volume of ore to be excavated, filling the bucket, swinging back to the dumping point, and emptying the bucket. The time required for the filling and emptying operations does not depend on the location of the unit volume of ore being excavated; we denote it by t_d . The time required for swinging to and from the point of excavation does depend on where the point is. We denote it by the function $t(r, z, \beta - \theta)$, where β is the angle from the dragline to the dumping point and all other variables have been defined. Finally, the number of times that the bucket must be returned to the point (r, z, θ) is the ratio of the elementary volume at that point, $r \cdot dz \cdot d\theta \cdot dr$ to the bucket capacity, denoted by D . Assembling these expressions, the total time required to excavate volume V is:

$$T = \frac{1}{D} \int_{r_1}^r \int_{\theta_1}^{\theta_2} \int_{z_1}^{z_2} [t_d + 2t(r, z, \beta - \theta)] r \cdot dr \cdot d\theta \cdot dz.$$

This formula gives the time actually spent in excavating the volume V . In addition, in order to obtain this volume the dragline must be moved into position, which means that it must be shut down after finishing at its previous stand, moved, and unlimbered. The time required for actual moving is irrelevant since the dragline will have to travel the entire length of the strip no matter what the mode of operation, but the time consumed in shutting down and setting up has to be charged against the V cubic feet excavated at the stand. Denoting this time by t_p , the total time required to obtain the V cubic feet at a stand is $t_p + T$ and the volume excavated per hour is $V/(t_p + T)$. This is the figure of merit, the quantity to be maximized by proper choice of operating parameters. If V and T are small, the critical ratio will be small because of t_p in the denominator. If V and T are large, the ratio is approximately V/T , and may be small because of the time con-

sumed in swinging the bucket back and forth over large angles. It appears that there is likely to be some intermediate optimum.

Clearly this formulation of the problem, the identification of relevant variables, the rejection of minor variables, and the determination of how the relevant variables affected the results, was attained only after careful observation of the actual operation and hard, careful thinking. There was additional empirical labor, also. The formula, it will be remembered, involved the swinging-time function, $t(r, z, \beta - \theta)$. This function had to be determined statistically by observing a great many excavations with different operators and working conditions. Other relationships, not mentioned in my summary, also had to be estimated empirically.

Even after the formula relating cubic yards per hour to the operating parameters had been established, substantial difficulties remained. As I set it up, there were 4 parameters to be decided; in the actual problem there were 10. Now, finding the maximum of a complicated function with respect to 8 or 10 decision parameters is a formidable undertaking. (This contrasts with the vehicular tunnel example, where the maximizing step was trivial.) The report does not state how the optimal decision parameters for different geological conditions were determined, other than that "it was necessary to carry out the total set of solutions on a high-speed computer"¹⁷ [15, p. 191].

And still, when the optimal modes of operation had been determined, the task was not completed. The recommendations had to be applied by foremen who are not skilled in substituting in mathematical equations or even in using ten-dimensional tables. Thus the recommendations had to be translated into a number of usable guide-charts and nomograms together with an extensive manual of instructions. One of the analysts spent three months with the operation after the conclusion of the study, training the personnel in the application of the results. All this effort seems to have been worth while since it led to an increase of some 40 per cent in output per hour.

3. *Simulation and Gaming*

This extended example may suggest the extreme mathematical, statistical, and technical difficulties that confront the operations analyst. They occur whether special-purpose or general-purpose models are employed. I have already mentioned that in the area of general systems analysis the equations describing the performance of an organization defy solution more often than not. The same is true of inventory problems and queuing problems. Even linear programming, whose salient

¹⁷ This kind of vagueness, enforced by the proprietary nature of many of the data, mars much of the literature of operations research.

virtue is the ease with which it lends itself to solution, is constantly pressing against the limitations of the most modern and powerful computing machines.

As a result, the operations analyst, like every other research worker, lives nearly always near the end of his tether. He simplifies his problems as much as he dares (sometimes more than he should dare), applies the most powerful analytic tools at his command and, with luck, just squeaks through. But what if all established methods fail, either because the problem cannot be forced into one of the standard types or because, after all acceptable simplifications, it is still so large or complicated that the equations describing it cannot be solved? When he finds himself in this fix, the operations analyst falls back on "simulation" or "gaming."

Simulation is to the operations analyst what a pilot model or experiment is to a natural scientist. If you can't predict how a machine will behave the only thing to do is to try it out and see. The operations analyst cannot usually try out an enterprise of the characteristics he is considering, but he can frequently duplicate it, at least approximately, on paper. To see how this works, pretend that we had failed to solve the machine-shop inventory problem of the last section. Then we should have to analyze it by simulation.

The most popular methods of simulation use high-speed computing machines. To simulate our inventory problem we should select a starting inventory at random, read the data of the problem into the machine and instruct the machine to follow some specific inventory policy. The machine would then look at the given starting inventory and decide in accordance with the assigned inventory policy whether to place an order for replenishment and if so how large. Then it would draw a random number from the range 0 to 9 inclusive. If the number were in the range 0-4 it would interpret this to mean that no parts were demanded in the first month, if it were in the range 5-7 it would assume that one part was demanded, an 8 would mean that two parts were demanded, and a 9 would represent a demand for three parts. Whatever the result, the machine would satisfy the demand as far as possible, subtract those sales from the initial inventory, increase the inventory by the replenishment stocks received in response to orders placed, if any, print out the results of interest, and go on to perform the same calculations for the second month.

All this would take about a thousandth of a second.¹⁸ In this way the machine would generate a thousand months of synthetic experience with the assigned policy, equivalent to nearly a century, in a second.

¹⁸ So much celerity is, unfortunately, extremely rare. Half a minute to a minute of machine time (costing \$5 to \$10) per cycle would be more typical.

When a sufficient amount of experience with a given inventory policy had been accumulated the machine would calculate the average inventory, average number of sales per month, average number of refusals, average reorder and inventory carrying cost, etc. Then it would turn to a new inventory policy and perform the same calculations.

In this way very large samples of synthetic experience can be obtained quickly and estimates can be obtained of all desired characteristics of probability distributions that are too complicated to be calculated mathematically. Analyses by simulation can do even more than that. The machine can be programmed so that after it has tried a few inventory policies assigned by the analyst it will then review the results and decide which would be the most promising inventory policy to try next. Then it could try the policy it has selected, again review the results, concoct a new promising policy, try it, and continue this process of trial and revision until it could not find any avenue that promised improvement within the range of inventory policies that it was permitted to explore. All this a calculating machine can do quickly, accurately, and without human intervention. What more could be desired?

Well, unfortunately, a great deal more. The result of a simulation is always the answer to a specific numerical problem without any insight into why that is the answer or how the answer would be influenced by a change in any of the data. In our inventory example, a change in the probability distribution of demand, in the length of the delivery lag, in the reorder cost or in net profit per sale would presumably change the solution, but a simulation with given data gives no hint of size or direction of changes in inventory policy resulting from such changes in data. Thus each variant of a problem analyzed by simulation has to be solved by a separate computation; and computation is expensive. In practical affairs, of course, it is usually more important to know how to respond to changes in conditions than how to behave optimally in any single set of circumstances. This is so because, first, circumstances do change, and second, because we never do know precisely what circumstances are but have to base decisions on estimates and, therefore, have to know how consequential errors in these estimates are.

There is a second serious limitation, also. Above I said that a computing machine could be programmed to search iteratively through a family of possible inventory policies and, through a guided process of trial and error, pick out the best. This is an oversimplification if the problem is at all complicated, say complicated enough to warrant simulation. Most decision problems tackled by operations research involve a number of interrelated variables. In the inventory example the variables are the numbers of parts to be ordered when the inventory is at each of its possible levels. In transportation problems the variables are

the quantities to be delivered by each supply point to each consumer. A review of the other models we have discussed will show that typically they are multidimensional and that much of their difficulty stems from the wide variety of possible solutions to be contemplated. When explicit methods of solution cannot be found, therefore, we find ourselves in an area known as "analysis of response surfaces," about which a few words have to be said.

Suppose that we are interested in maximizing profit or minimizing cost, or something else of the sort, where the profit or cost in question depends on a number of variables under our control but where the manner of dependence is so complicated that we cannot actually write it down in the form of an equation. Then we can select any set of values of the variables under our control and, say, by simulation, estimate the value of the profit corresponding to that selection. This profit, together with the values of the variables that gave rise to it is "a point on the response surface," and, subject only to limitations of patience and expense, we can calculate any number of such points that we please. What we now need is some procedure for finding the set of values of the decidable variables that gives rise to the highest possible profit, by means of a practicable number of simulations or similar computations. All that simulation provides is a method for finding single points on the response surface and the best that can be said about finding optimal points is that research on this problem is being prosecuted vigorously [9] [10]. As things stand at present no fully satisfactory general-purpose method is known.

Be that as it may, simulation comes nearer to solving the unsolvable than any other device known, a fact that justifies fully its importance as a tool of operations research. Except in problems as trivial as our inventory example it is, however, a difficult tool to use well. It entails two main kinds of technical difficulties, those relating to the exploration of the response surface, which we have already discussed, and statistical problems such as deciding how large a sample to take of each set of circumstances and policies. E.g., in our inventory example the statistical problem is how many months of synthetic experience should be accumulated with each inventory policy examined. With a given research appropriation, more policies can be examined if each examination employs a smaller sample, but the disadvantages of using unduly small samples is evident. These are formidable problems in technical mathematical analysis and statistics and have an important bearing on the cost and precision of the analysis, but do not have much influence on the result, or, at least, should not.

A device quite similar to simulation in form but entirely different in objective is "gaming" or "operational gaming." Formally, gaming is

simulation with human intervention. To "game" our inventory example we should omit the inventory policy from the machine program and replace it by the following routine. Each time that a new initial inventory is computed, the machine would print out the results of the previous period's operations, including the terminal inventory, and wait. Then the subject, which might be either an individual or a team, would decide on the basis of his best judgment how many parts to order. This would be read into the machine which would then compute the results of the period's operations, print them out, and wait for the next decision. In working through an operational game of this sort, the subject might or might not be informed of the basic data, for example the probability distribution of demand. If he is not informed of some of the relevant data he would be expected to deduce them as experience accumulates.

This device will not, of course, disclose the optimal policy to be followed, but it can serve any of several other purposes. It can be used to investigate how close an experienced subject can come to the mathematical optimum in given circumstances and how long it takes him to do so. It can be used to test how changes in the circumstances and in the data available change the performance of subjects, and in this way to throw light on the practical value of additional information in the real life situation being simulated. Thus, in the inventory example, if the probability distribution of demand is not known such an experiment could reveal how much should be spent on market research or other methods for ascertaining it.

Gaming can also be used as a psychological-experimental device for studying executive behavior. Thus, in one set of such experiments it was found that executives started by basing their decisions on rough rules of thumb and revised them, in the light of experience, much too slowly in the sense that when they changed their policies they usually moved them only a small fraction of the distance between the current policy and the optimum, and only very rarely overcorrected. Further it was found that this conservative bias tended to be more marked in proportion to the importance of random and unpredictable elements in the game.

Gaming can also be used to study the optimum of some decisions when other decisions are too complicated or are based on considerations too vague to be formalized. In this application, various policies for making the decisions to be optimized are programmed into the machines while the unmanageable decisions are made by a team as required. Finally, gaming can be used as a pedagogical device.

Gaming is fun but, if a skilled team is required, very expensive. The expense frequently precludes sufficient replication to generate reliable

probability distributions of consequences; and even where expense is not prohibitive, the memories, learning processes, and tendencies to habit formation on the part of the teams make much replication impracticable. Even if the replication problem can be surmounted there is a more fundamental difficulty in using gaming as a research tool. In any series of repetitions of a game, the teams will either base their decisions on some well-defined policy or, more usually, will "play it by ear," using their best judgment as experience accumulates. In the former instance the results of the experiment will be an assessment of the consequences of the policy used but, as we have seen above, any well-defined policy can be assessed more cheaply and conveniently by programming a computing machine to follow that policy and conducting a simulation experiment. In the latter instance it is hard to say what the results mean since no expressible or reproducible policy was followed. The results will reflect an amalgam of the potentialities of the situation, the approximations used in constructing the game, the abilities of the teams under somewhat unnatural conditions, and the vagaries of chance. If a large number of teams is used, the gaming may produce an evaluation of how well an "average" team will perform under the conditions of the game, but still another dimension of sampling variability crops up in this application [38].

As a result of these problems, the literature does not indicate that gaming has played a significant role in solving operations research problems. It seems to hold more promise as a device for executive training, for investigating the psychology of decision making, and for stimulating effective planning by confronting managers vividly with various contingencies they should be prepared to meet. Even in these last applications, however, it does not seem feasible to impose rewards and penalties cruel enough to approximate the pressures of real-life decision problems.

III. *The Objective Function*

A review of the models we have considered will show that each can be divided into two parts: a part describing the structure of the operation and the relationships among the variables (both controllable and uncontrollable), and a part that evaluates the consequences of any choice of variables in terms of profit, cost, or some other measure of desirability. We shall refer to the first part as the constraints and to the second as the objective function or criterion. Most operations research problems take the form of searching for the values of the decidable variables that maximize or minimize the value of the objective function while satisfying the constraints.

An economist can sympathize readily with this habit of formulating

problems as if the purpose were to maximize this or minimize that, but he is also aware of the pitfalls in this approach. In the first place, it is by no means axiomatic that the purpose of an operation can be expressed as maximizing anything less vacuous than desirability all things considered. The objectives of business enterprise are obscure. Among recent economic writers, Baumol [5] has argued that businessmen typically seek to maximize sales volume subject to a constraint on the rate of profit, Lanzillotti [24] found that predominantly they seek to attain some target rate of profit, Simon [37] maintains that they are "satisficers" attempting to reach some aspiration-level but not trying to maximize anything in particular. It seems clear that short-run profit maximizing is neither a sensible nor a prevalent business objective but, beyond that, what objectives are prevalent is a matter for conjecture.

In any specific context, then, the operations analyst has the task of ascertaining his client's objectives, and this task is complicated by the fact that his client is not likely to be very clear on the matter himself nor, since the client is generally not a single person, entirely of one mind. An appealing way out is to ask the client what his objectives are. B. O. Koopman, in a thoughtful presidential address to the Operations Research Society of America [23], stigmatized this practice as "authoritis" and included it in his catalogue of prevalent fallacies in operations research. His point was that no businessman, still less a second vice-president in charge of planning and research, can be expected to answer such questions with enough precision or authority to provide a sound basis for research.

The question is far from academic because typically a business firm watches manifold consequences of its operations including rate of profit, value of its shares, sales volume, share of market, public image, reputation of product, *et hoc genus omne*, and is not willing to increase its performance in any of these respects at unlimited sacrifice in the others. What is needed for operations research is a reasonably precise idea of how the varied goals trade off against each other, but in fact the interrelations are extremely complex. It is a frequent experience in operations research, when the job is done and the recommendations are presented, to have them greeted with, "Oh, but I guess we forgot to tell you that in this company we are very anxious to keep our reputation for prompt deliveries by having a separate warehouse in every major market area, even if some of them run at a loss." This sends the analyst back to his drawing board. Even though it does endanger the analyst's rapport with his client, it might not be so bad if it caused only a revision or two in each project, but the number of controlling policies that amount to collateral objectives can be very large and so well understood within the organization that no one bothers to mention them until they

are elicited by deep probing or a final report that violates them. A case in point is our example applying linear programming to the scheduling of petroleum blending operations. Our conclusion was that the refinery should produce either regular or premium fuel, but not both. This recommendation would almost certainly be unacceptable since refineries are generally committed to producing a full line of products. Linear programming, which tends to yield extreme (all or nothing) solutions, is especially likely to recommend insufficient variety of products or action.

Typically where there are manifold objectives, as there nearly always are, some of them are treated within the company as constraints, or limits on the concentration of effort on the attainment of some of the others. In Baumol's experiences, for example, the rate of profit, which economists traditionally regard as a primary objective, tended to play the role of a constraint on efforts to maximize sales volume. How does one tell the difference between a constraint and an objective? In principle, I suppose, it cannot be done. John Dewey taught us long ago that means and ends, constraints and objectives, might roughly be separated for purposes of discussion but were so intimately intertwined that they were fundamentally indistinguishable.

Conceding this, a practicable distinction between constraints and objectives might go as follows: A requirement is a constraint if (a) it must not be violated at any cost however high or with any probability however low, and (b) there is no gain or advantage in overfulfilling it. On the other hand, a requirement is one of the objectives of the firm if it can be violated, though at a cost or penalty, or if there is an advantage in overfulfilling it. Mixed cases can occur. Thus, if deliveries must be made within two weeks at all cost, and if there is an advantage in quicker deliveries, the maximum delivery time is a constraint while, say, the average delivery time enters into the objective function. If this distinction be accepted, it will be seen that only technological requirements will qualify as constraints (e.g., the output of a refinery cannot exceed the volume of crude oil inputs); the attainment of all other requirements is part of the objective. In other words, every practicable design of an operation is subject to failure in sufficiently adverse circumstances; one designs so as to balance the risk of failure against the cost of decreasing that risk. Thus the use of policy constraints, though prevalent and perhaps inevitable, must entail some loss in attainment of the "real" (alas, inexpressible) purposes of the enterprise.

To get on with the discussion, suppose that it is possible to decide which of the consequences of an operation are constraints and which are objectives. The problem remains of combining the various objectives into a single objective function to be maximized. The difficulties

encountered here are familiar to economists for most part, but worth summarizing. There is, first of all, the problem of comparing consequences that occur at different times. The comparison of a number of courses of action whose consequences extend over time requires either the comparison of a number of detailed time paths, which is too difficult for most human minds, or some discounting procedure that accords relative values to costs, revenues, etc. at different dates.

The fact that the consequences of decisions are uncertain gives rise to a similar difficulty. The result of a decision or action is not, strictly, a predictable cost or income but a probability distribution of costs or incomes. Hence the comparison of the desirability of consequences presumes that we are able to compare the desirability of probability distributions. Still a third difficulty of the same general type arises from the conundrum that we have already discussed at some length, namely that not all of the consequences of a decision are commensurable in any convenient unit. A more specific example of this difficulty arises in inventory theory. If inventories are adequate or overadequate to meet demand, the consequences take the form of sales, carrying charges, and the like, all of which are easily measured in dollars. But if inventories are inadequate to meet demand, the consequences will lie in a different realm: there will be disruption of production processes, loss of customer's good-will, and so on. Ultimately these consequences too may be reflected in dollars and cents, but the measurement of such indirect monetary effects is a research project of forbidding difficulty. In queuing problems, again, the different consequences flowing from the same action tend to be incommensurable.

I have grouped together these three difficulties—time discounting, risk preference, and incommensurability of consequences—because they all have to be handled in about the same way. One conceptual approach is to try to construct preference maps. Fisher's theory of interest, for example, applies this construction to the time discount problem. A number of treatments of the risk problem, for instance Shackle's [35] and Markowitz's [29] are based upon it, and its relevance to the incommensurability problem is obvious. This device, however, is little more than a way of formulating the issue, because preference maps are almost impossible to ascertain empirically.

Another approach is to take the position that these complicated, multidimensional consequences are not the ultimate objectives served by the operation but are only intermediate ends to some ultimate, uni-dimensional goal such as maximizing the current net worth of the enterprise. This approach requires that we be able to measure the influence of each aspect of the intermediate consequences on the ultimate goal and, as suggested above, this task is generally prohibitively difficult.

It amounts, of course, to attempting to construct the over-all utility measure that lies behind the preference map utilized in the first approach, and presumes that there is such a measure.

Still another approach is to apply a Paretan criterion as far as it goes and then to turn the task of further evaluation and choice back to the client. The analyst who follows this approach limits himself to seeking efficient decisions, i.e., decisions that cannot be improved in any dimension without an offsetting sacrifice in some other, and leaves it to the client to decide which of all efficient decisions he prefers. Of course, there are other devices, too, familiar to economists since the problems are familiar ones, which are more effective in eliminating grossly inappropriate decisions than in optimizing anything to a very fine degree. A typical one would be to work through the consequences of a decision, find that it would be optimal if the rate of time preference were, say, at least 15 per cent per annum, and then put it up to the client to decide whether his rate of time discounting is in fact that high.

The problem of evaluating risk preferences has received more attention lately than the other two problems because it lies at the heart of statistical decision theory. A number of principles for choosing among policies whose consequences are uncertain have been proposed, all plausible, none free of serious defects; but to discuss them would take us far afield. Illuminating surveys of thinking about decision-making under uncertainty can be found, for example, in Arrow [3] and Luce and Raiffa [25, Ch. 13].

Fortunately, this galaxy of unsolvable problems is less obtrusive in narrow operational contexts than in broad strategic ones. It is easier to ascertain the objectives of a department than of a firm, of a section than of a department, of a particular phase of the work than of a section. There are several reasons for this. First, the narrower the scope of an operation, the narrower is the range of consequences and the greater is the likelihood that all of them will be commensurable, usually in terms of dollars. Linear programming models are cases in point, though queuing and inventory models are contrary instances. Second, departmental and subdepartmental decisions frequently concern matters that have short time horizons; often all the consequences are practically immediate. Nearly all the models discussed above except the inventory models illustrate this assertion. Finally, the range of uncertainty associated with detailed operating decisions is generally smaller than that surrounding more comprehensive choices and, besides, the range of uncertainty engendered by each operating decision is small enough in relation to the size of the enterprise and such decisions are numerous enough so that they can be treated appropriately from an actuarial point of view.

For all these reasons the objectives of operations conducted at the middling and lower levels of an enterprise are likely to be fairly well defined. There is, however, one special danger that arises when an objective function is devised for a part of an organization or a specific operation. This is the danger that economies and diseconomies external to the department in question will be neglected. When an objective function is adopted for, say, a department, that department and the operations analysts who advise it will be induced to make the choices that seem optimal in the light of that function. Therefore it is important that such partial objective functions conduce to decisions that are consistent with the over-all goals of the enterprise. Unfortunately, it is just about impossible to find performance criteria that meet this requirement in all circumstances. A lurid example of this difficulty arose in wartime operations research in the air force, where the efficiency of bombing groups was judged in large part by the percentage of their bombs that fell within a thousand feet of the targets they aimed at. This standard encouraged bombing commanders to refrain from releasing bombs when visibility was poor. The effectiveness of the bombing groups went down while their paper scores went up.

The same conflict between real and paper performance occurs in business operations. Suppose, for example, that a manufacturing department is given the objective of producing its assigned output at minimum average cost. Then the manager being judged by this criterion has a strong incentive (a) to avoid overtime work even though other departments or customers may urgently require his output, and (b) to shave his specifications in a way that will increase the proportion of rejects at a later stage of fabrication. Any simple criterion of departmental performance must therefore be hedged by some artificial standards or constraints—like the subsidiary objectives discussed above—for example, quality standards and regulations about permissible delays in delivery. The shortcomings of such standards have already been mentioned.

This is probably as good a place as any to call attention to another pitfall in the selection of objective functions or performance criteria. This is the seductive simplicity of critical ratios like cost per unit, output per man-hour, or the one I used in the dragline example, cubic yards per operating hour.¹⁹ Such ratios are invariably dangerous, as Hitch and McKean, particularly, emphasize [21]. Consider the dragline case. The criterion I suggested would discourage excavating to the very edge or bottom of the vein, even though it would be profitable to do so, since doing so would reduce the time-rate of excavation. There

¹⁹ In fairness to Dunlap and his associates I must point out that a somewhat more sophisticated objective function was used in the actual study, though the published report did not define it precisely [15, p. 182].

are two fallacies tucked away in my criterion. First, it assumes that dragline time is the only scarce resource, and encourages economizing it at the expense of all other resources, such as unexcavated ore. Second, it is a ratio and, as economists are well aware the objectives of an operation are almost invariably to maximize some difference (some measure of benefit minus some measure of cost) rather than any ratio. Profit maximization is not the same thing as unit cost minimization.

Thus there are dangers in assigning objectives to parts of an enterprise, but they are far more tractable than the problems encountered in trying to establish objectives for an enterprise as a whole. Because of the relative concreteness of the purposes of parts of an enterprise, suboptimization is a valuable concept and applicable even when over-all goals must remain vague. Suboptimization will be treated briefly in the final section.

IV. *The Role of Operations Research*

Such is the nature of operations research. I hope that I have made an adequate case for the assertion that its essence lies in a strong tendency to tackle administrative problems via formal models constructed in the spirit of the natural sciences. We turn now, and finally, to the role of operations research in business and economic administration.

If an experienced operations analyst were asked to describe the problem he would most like to meet, I suspect that he would mention four characteristics: first, the objective of the operation should be clearly defined, second, the operation should be describable by a reasonably manageable model, third, the data required for estimating the parameters of the model should be readily obtainable, and fourth, current operating practice should leave plenty of room for improvement. These are the characteristics of the ideal problem, from the analyst's point of view, and sometimes he encounters it, but more often he must be content with projects that fall short of perfection in one respect or another. Each of these characteristics deserves a little discussion.

For an operation to have a clearly defined objective it is not necessary, of course, that the businessman or other client be able to write down its objective function at a moment's notice. It does require that the analyst be able to obtain a consensus on the purpose of the operation, specific enough so that he can judge how conflicts in goals are to be resolved. With a little care to assure that the objective function does not conflict with higher-level goals and that the measure of cost is appropriate, the definition of objectives should present little difficulty at the operating levels. More trouble is likely to occur at the executive levels, where decisions are likely to have widespread and incommensurable ramifications. Glen Camp, in fact, warns against undertaking

such problems: "... best results will be obtained if the scientist meticulously avoids the evaluation of intangibles" [11, p. 630]. The narrower and more specific the problem, then, the more likely it is to possess this characteristic.

The second characteristic of a promising project was that it be possible to formulate a manageable model to describe the impact of various possible decisions on the objective function. Again we may cite Glen Camp: "The function of the operations research team is to assist the responsible authority of an organization by clarifying those uncertainties in the factors on which action is based, and *only* those, which can be clarified by scientific study" [11, p. 629]. Now, how is one to tell, in a preliminary survey of a problem, whether its essence can be caught in a manageable model or whether, in Camp's words, it can be clarified by scientific study?

Model building is the analyst's primary skill and contribution, and he cannot expect when he approaches a problem to find that his work has already been done for him. Thus the question is not whether a model exists ready-to-hand, but whether one can be built in reasonable time. The answer depends basically on whether or not the problem involves kinds of relationships that have not been established by previous scientific study or, as I shall say, whether or not it involves gaps in fundamental scientific knowledge. Consider, for example, the advertising fable that we used to characterize the operations analytic approach. In that fable the analyst boldly extemporized a model of the relationship of advertising expenditure to sales. It was, of course, a shot in the dark. No one really knows what the relationship in question is. The problem involved a gap in scientific knowledge.

When he encounters such a gap, the operations analyst has a choice of three options: he can proceed on the basis of a bold conjecture, he can undertake the substantive research necessary to fill the gap, or he can abandon the problem as unsolvable at the current state of knowledge. Much of the analyst's skill lies in determining which option to choose in given circumstances. A bold conjecture is refreshing, but an insubstantial foundation for an important decision. Abandoning the project is manifestly distasteful. Undertaking fundamental research entails the usual hazard that it may not be successful, plus an unwelcome delay in arriving at a useful recommendation.

In practice, this third option is frequently chosen and frequently well advised. Much of the work of the practicing analyst is the work of filling gaps in substantive knowledge, just as much of the value of the model-building approach resides in disclosing and defining those gaps. There is much testimony to indicate that the most valuable results of operations research are by-products. Again and again businessmen have

stated gratefully that they learned more about their business by answering an analyst's questions, supplying data for his model, and checking over the model with him than they did from his final report. (Is the analogy with psychoanalysis a coincidence?) Similarly the substantive research undertaken as part of an operations research project is often of great value, quite apart from the value of the final recommendations.²⁰ Thus the attempt to construct a model may be worthwhile even in unpromising circumstances.

But not always. Frequently the gaps in knowledge that prevent constructing a reliable model are already perfectly well known to the client, and not of a kind to be filled by short-term research. The advertising fable is, very likely, a case in point. Such gaps in knowledge are frequently what induce the client to call in the operations analyst. If he could fill them, he could solve the problem himself, and his hope is that the magic of operations research will help him to reach a well-founded decision in spite of certain areas of ignorance. Such hopes are doomed to disappointment. Operations research is not a substitute for substantive knowledge, but a way of employing it, nor can the operations analyst be expected to complete scientific knowledge to order as required by his clients.

If gaps in substantive knowledge prevent the formulation of a complete model, clearly the analyst cannot hope to ascertain the optimal decision. He may then address himself to a more modest, but still useful goal, as pointed out by P. M. S. Blackett in one of the earliest and most important papers on operations research methodology [8]. He can seek to discover a policy that is better than the current one, though not necessarily best. This approach is one that economists are schooled in. Blackett recommended that instead of attempting to ascertain the objective function as a function of the various decision variables, the analyst undertake the much easier task of estimating its partial derivatives with respect to each decision variable (essentially the net marginal productivities of the decision variables) in the neighborhood of the currently used values of those variables. If any of those partial derivatives is substantially different from zero (i.e., if any marginal productivity is substantially different from the corresponding marginal cost) then a direction in which current policies can be improved is apparent.

Just as the operations research approach is not peculiarly adapted to solving fresh scientific questions, so it is not well adapted to discovering fresh lines of action or technological innovations (with an exception to be noted below). A range of possible actions or decisions is

²⁰ For a famous and fascinating illustration see Thorntwaite, "Operations Research in Agriculture" [27, pp. 368-80].

built into the model from the outset; the solution of the model yields the best solution within that range. For example, linear programming yields the optimal combination of the activities listed in the matrix; it will not disclose the existence of some still better activity not foreseen in advance. In short, the technique of operations research is designed to uncover better ways of organizing the use of given resources by means of given techniques; it does not extend the range of technical possibilities.

This is not to say that operations analysts have not been instrumental in technical innovations. They are typically imaginative and resourceful men, unfettered by traditions of which they are frequently unaware, and often do suggest courses of action that would never occur to men schooled in the habits of an enterprise or branch of technology. But the methods of operations research are of little help in the field of substantive invention though the practitioners often do have patents to their credit.

There is, however, one field of operations research that does bear directly on the process of technical invention, namely "requirements studies." In a requirements study an operation is surveyed in order to determine the characteristics of desirable technical innovations. Models are built which incorporate as yet nonexistent hardware and the performance characteristics of the hardware are varied (on paper) in order to ascertain their influence on the over-all operation. Thus a set of specifications for new equipment can be established and the gain flowing from meeting those specifications can be estimated. This type of analysis has been most prevalent in establishing military requirements—the RAND Corporation was studying the usefulness of artificial satellites as early as 1946—and has also been used by commercial airlines in planning for new equipment. Thus studies are now in progress on the usefulness of supersonic passenger aircraft.

The third characteristic of a promising operations research project was that an adequate fund of experience be available to permit statistical estimation of the parameters required by the model. This requirement will be satisfied most adequately by repetitious, even routine, types of operation. Morse and Kimball, for example, state, "Patrol or search is an operation which is peculiarly amenable to operations research. The action is simple, and repeated often enough under conditions sufficiently similar to enable satisfactory data to be accumulated" [32, p. 38]. Nearly all our examples, indeed, have been of this sort. They concerned scheduling a refinery, which is done at least once a month, reordering inventories, similarly periodic, and so on. In all such repetitive decisions, the necessary statistics are accumulated over time as an administrative by-product if not as part of a formal reporting sys-

tem. If the requisite statistics are not available, the situation is analogous to that which occurs when there is a gap in scientific knowledge, discussed above, except that gathering statistics is less of a gamble than undertaking to establish a new scientific relationship.

If the problem being studied is nonrepetitive, even the statistical outlook is more doubtful since, after all, statistics require a population from which a sample can be drawn. The statistical characteristic also, therefore, is more likely to be fulfilled at the operating levels of an enterprise than at the highest executive levels, since operating decisions are much less likely than broad policy and strategy decisions to be *sui generis*.

The final characteristic of a promising operations research study was that current operations admit of substantial improvement by means of the kinds of decisions discoverable by studying the operation of a model. This last characteristic, unfortunately, works somewhat in opposition to the first three. If an operation is repetitive, well recorded, intended to serve a well-defined goal, and of a kind in which the influences of decisions on the attainment of the goal do not transcend available technical knowledge, then it is not likely that current practice will fall far short of the most efficient practice attainable. And, indeed, the usual upshot of a linear programming analysis or a transportation problem study is to find a plan that will reduce costs by 2 or 3 or 5 per cent. To be sure, in a large operation, 5 per cent is not contemptible. But neither is it dramatic; and in view of the approximations used in reaching such solutions and the possible errors in the statistical estimates it cannot even be certain that such small savings are genuine rather than paper results. This finding stands in unhappy contrast to the state of affairs during the second world war, on the basis of which Morse and Kimball wrote, "... successful application of operations research usually results in improvements by factors of 3 or 10 or more" [32, p. 38]. This is as if a successful operations research project can be expected to treble the capacity of a factory or divide its unit costs by three.

The contrast between the peacetime and wartime yields of operations research is explained by the fact that the second world war was largely a race of technological improvements. Efficient submarines led to the development of airborne search-radar; airborne search-radar induced the invention of snorkel-equipped submarines. Technological innovations followed each other so quickly that a new device was in the field before trial-and-error methods could discover efficient means for employing its predecessor. Operations research proved to be a very effective means for accelerating the discovery of effective ways of using novel equipment. In more stable circumstances, however, the situation is otherwise. Blackett, also relying on wartime experience, wrote "... in

the course of repeated operations by many different participants, most of the possible variations of tactics will be effectively explored, so that any large derivatives will eventually be discovered, and given intelligent control, improved tactics will become generally adopted" [8, p. 33]. On the other hand, considerable room for improvement may remain even under fairly stable technological conditions, as the dragline example showed. The explanation in that instance probably lay in the numerousness and the complexity of the decision variables, which precluded efficient exploration of possibilities by unsystematic means.

These considerations suggest that in just those kinds of business operation in which the first three requirements for productive operations research are likely to be met, the requirements for the discovery of efficient methods by more traditional means are also likely to be met, and there may not be very much improvement left for the operations analyst to discover. The major exception to this conclusion is problems of adapting to new circumstances or of employing novel techniques or instruments. In those cases, operations research can often speed up the process of adaptation. An optimal situation for operations research is one in which conditions are changing too rapidly for experience to be assimilated by informal, unsystematic methods, but slowly enough to permit the formulation of a model applicable to both the recent past and relevant future, and to permit the accumulation of the data needed for estimating the parameters of the model.

All in all, conditions auspicious for operations research are more likely to be met at the middling and lower levels of an organization than at the topmost ones: the clarity of objectives, the simplicity of relationships, and the availability of technical knowledge and statistical data all point in this direction. Thus the device of "suboptimization" recommends itself. Suboptimization is defined by Hitch and McKean, its principal expositors, as the "... process of choosing among a relatively small number of alternatives by an administrative level other than the highest" [21, p. 172]. More explicitly it is the organizational policy in which the higher echelons of an organization assign tasks to the lower echelons and establish performance criteria for them, and then leave the lower echelons free to perform the tasks in the best way they can as judged by the criteria established. That, after all, is how a market economy works. Suboptimization is a new word for decentralization, and its advantages are the familiar advantages of decentralization. From the point of view of operations research its major advantage is that it enables the relatively manageable problems of detailed operation to be solved separately from each other and from the more intractable problems that arise on the higher executive levels.

The foregoing summarizes the circumstances in which operations

research is likely to be successful in the sense of disclosing significantly improved policies and practices. But operations research can be successful in other senses also. We have already noted the educative value of collaborating with an analyst and looking at problems from his viewpoint. We have seen that operations research often suggests and sometimes accomplishes valuable substantive research. In many instances the contribution of operations research is to improve the planning process itself, without improving the quality of the plans.

Consider planning petroleum refinery operations, which now is quite prevalently accomplished with the use of programming models. Before programming was introduced, monthly refinery schedules were developed by a planning section in the refinery engineer's department and required about two weeks of computation by highly trained engineering personnel. After a programming system is introduced, the same plans are developed in three or four hours by clerical personnel and computing machine operators under the general supervision of an engineer. The new planning system has at least three advantages over the old one, even though the resultant plans are not appreciably superior to those developed by tedious hand calculations using the same data. First, it is vastly cheaper in terms of both monetary cost and drain on scarce, highly trained manpower. Second, because of its speed, more timely data can be employed. Before the mechanized planning made possible by operations research, the forecasts for, say, the March plan had to be based on data available on February 14; after mechanization the closing date for forecast data becomes February 26. Thus, even where programming does not produce superior plans given the same data, the speed with which it can be performed permits the use of improved data. Third, the probabilities of errors in both arithmetic and judgment are greatly reduced when formalized, mechanized planning supersedes informal, skilled-judgment methods. The programming procedure includes a built-in guarantee that the resulting plan is optimal, avoiding the hankering worry that a misjudgment was made somewhere in a protracted computation.

In more general terms, where plans or decisions are based on large masses of data and complicated interrelationships—where, for example, a large number of operations have to be coordinated—the model developed by an operations research study provides a framework within which the data can be assembled, analyzed, and digested in a swift, mechanical, error-free, and inexpensive way. Such a model makes the planning process itself more efficient and reliable.

Finally, consider the really tough and important problems where there is no objective basis for making a usefully precise evaluation of the consequences of possible actions or policies. Contending with such

imponderables is the primary responsibility of the high executive, a responsibility that cannot be delegated, not even to an operations analyst. Nevertheless, an operations research study of such a problem can help the executive by organizing the data, focussing the issues, bringing out the implications of possible assumptions and hunches, delimiting the range of the inevitable uncertainty. Any detached, analytic, skeptical viewpoint can help clarify such problems, and the analyst has such a viewpoint to contribute.

There is much room for folly, though, when an operations analyst participates in conjecturing answers to unanswerable questions. The executive is under a strong temptation to pass the buck to the analyst, and the analyst is tempted just as strongly to accept it. When there is no "right" decision, there is a tendency to adopt one that can be justified—for who can be blamed for following a recommendation supported by an imposing dossier? And what dossier is more imposing, these days, than an operations research report? Thus the analyst may find that his simplifying assumptions, perhaps made for exploratory purposes, have become the basis for weighty decisions, even decisions important to the safety of the nation.

It is all very well to inveigh against the executive who permits his judgment to be overborne by elaborate calculations that he does not understand. Though the executive must retain the responsibility, he must also accept much on faith, and when his analyst assures him that the recommendations are well-founded, what is he to do? The businessman cannot audit the technical reasoning. Though the analyst can remain detached and impartial as regards the affairs of his client, he is as liable as any man to fall under the spell of his own handiwork. I see no satisfying way to resolve this difficulty. Glen Camp, as we saw, advised analysts to abstain from such dangerous enterprises but also, as we remarked, the analyst can serve usefully in smoothing the way for a decision. The accumulation of experience in the use of operations research will probably help some, particularly by reducing the pressure on the analyst to produce clear-cut recommendations.

It appears, in summary, that operations research is best adapted to dealing with routine, semitechnical, quantifiable problems, and that it can also contribute in a larger realm by showing businessmen how to view their problems from a sophisticated scientific standpoint. It has developed powerful methods for solving the day-to-day problems of middle management and, I think, can fairly claim to be an indispensable tool at that level. Operations analysts aspire higher, of course.²¹ But when they will attain a special competence in dealing with larger, more conjectural problems is itself a very conjectural question.

²¹ See for example Ellis A. Johnson [22] and Russell Ackoff [1].

We noted at the outset that operations research is dominated by, and takes its style from, men trained in the natural sciences. There is, however, a large handful of practising operations analysts who were trained as economists and, permitting myself a parochial evaluation, these men have contributed to the development of the science far out of proportion to their numbers. The economist comes to operations research with a number of important ideas already instilled, among them an appreciation of the importance of substitution, a sophistication about the objectives of enterprises, an awareness of the importance of marginal trade-offs, and most important, a realization that physical quantities are subsidiary to "values" in a decision process. He also inherits from his training a number of disabilities, including ignorance of the technical side of business and industry and a belief in the existence of production functions. On balance it appears that the older science has more to contribute to the younger than the other way round, as is right and proper. But still we can learn from our juniors, and we fail to do so at our own risk.

The most important lesson operations research has to teach is how much we are asking of businessmen when we ask them to behave "rationally." Even when businessmen would dearly like to do so, it turns out that the most powerful tools of mathematics cannot, for example, help them discover a "rational" inventory policy; and that is only one small part of the businessman's problem. Since the profit-maximizing or risk-minimizing course of action is undiscoverable, he must perforce rely on hunches and rules of thumb. It is by no means clear what rational behavior consists of in such circumstances. On the other hand it turns out that business performance is frequently quite close to the rational optimum for problems of the sort that operations research is able to solve.

Thus the lesson of operations research appears to be a heavy score against the belief that firms maximize anything, either in the short run or the long. Instead we must conceive of actual firms as operating according to certain rules of thumb that would be rational if life were much simpler than it is and that are not disastrous under actual circumstances. It makes one tolerant of such practices as pricing by conventional mark-ups, costing with conventional overhead burdens, and investing in accordance with tried and proven pay-off periods. These practices are what businessmen must follow until operations research provides them with better standards. The success of operations research testifies to the willingness of businessmen to abandon operation by rule-of-thumb when a better alternative becomes available. The best we can say is that businessmen would like to behave "rationally" and are eager to be taught how.

For many purposes of economic analysis the current crude image of the firm, as a responsive extension of the personality of a fully informed, aggressive entrepreneur, is probably adequate. But for some other purposes—the understanding of inventory and investment behavior, for example—we must recognize the firm for what operations research has disclosed it to be: often fumbling, sluggish, timid, uncertain, and perplexed by unsolvable problems. Since its discriminating power is low it responds only to gross stimuli; since its decision processes are uncertain the timing and vigor of its responses are unpredictable. It reacts in familiar ways to the familiar and avoids the novel as long as it dares. We need economic theories that incorporate these ingredients. They will remain valid until operations research has made much more progress against businessmen's problems.

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PATTERNS OF INDUSTRIAL GROWTH

By HOLLIS B. CHENERY*

An increase in per capita income in a country is normally accompanied by a rise in the share of industrial output. The accepted explanation for this relationship is the change in the composition of demand, of which the decline in the share of food (Engel's Law) is the most notable feature. However, this over-all relationship does not necessarily apply to every individual country. Within limits, the changing composition of domestic demand can be offset through foreign trade. A country having a continuing comparative advantage in primary production may therefore reach a high level of income without an increase in the share of industry in total output. Because of the diversity of natural resources, we should not expect to find uniform patterns of growth in all countries.¹

Evidence from several sources suggests a closer and more pronounced relationship between levels of income and industrial output than would be predicted from the change in demand alone. Kuznets' comparison of some 50 countries [13] shows a marked increase of manufacturing with rising per capita income, as did the earlier analyses of Bean [1] and Clark [2]. The fact that patterns of trade change systematically with rising income levels is equally well established [9]. Historical studies also show considerable uniformity in the rise of industry as growth proceeds [7] [10] [15]. Finally, modern growth theory contains arguments against continued specialization in primary production, stemming from the uncertainty of export demand and the interdependence among sectors of production [14] [16].

In searching for additional explanations of the rise of industry, it is natural to look for systematic changes in supply conditions as well as

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¹ A strong criticism of the supposed necessity for industrialization to achieve a rising income is given by Viner [18, Ch. 3].

in demand with rising income. Here two factors are of general importance: (1) the over-all increase in capital stock per worker; (2) the increase in education and skills of all kinds. Since, moreover, the proportions in which labor, capital, and skills can be combined vary from sector to sector, the change in factor supplies causes a systematic shift in comparative advantage as per capita income rises.

The purpose of this paper is to incorporate changes in both demand and supply conditions into a more general explanation of the growth of individual sectors of production, which can then be used to explain the observed patterns of industrial growth. Section I derives "sector growth functions" from a general equilibrium model which allows for changes in the composition of demand and in factor proportions. A simplified version of this model is used in Section II as a basis for regression analyses of production and import data for a large number of countries. In Section III, these results are used to establish the existence of significant growth patterns for all branches of industry. The relative importance of changes in demand and supply is then determined for each sector. The variability of growth patterns among countries is investigated in Section IV, and the importance of size, natural resources, and other factors is indicated. Section V takes up the policy implications of the analysis.

I. Determinants of Sector Growth

The assumptions normally used in formal growth models do not explain differences in sector growth rates. The general equilibrium models of Walras, von Neumann, Leontief, Samuelson and others customarily omit the elements which would lead to persistent differences in growth rates: limited natural resources, changing factor supplies, nonhomogeneous consumption functions, economies of scale, and even international trade. These models imply the optimality or even necessity of proportionate expansion of all sectors in the long run, a growth pattern which is only observed when per capita income does not increase.²

To use the Walrasian model as a basis for an analysis of growth patterns, several modifications are needed. First, some allowance must be made for the principal factors leading to nonproportional growth rates. The model must next be reduced to a form in which the explanatory variables are measurable characteristics of national economies. Finally, measures of these characteristics that are available on a comparable basis for many countries must be sought.

Since the Walrasian model omits both international trade and intermediate goods, it assumes domestic production to be identical with final

² General-equilibrium models are reviewed in Dorfman, Samuelson, and Solow [6].

domestic use. Adding the missing elements gives the following accounting identity.³

$$(1) \quad X_i = D_i + W_i + E_i - M_i$$

where:

X_i is domestic production of commodity i ,

D_i is domestic final use of i ,

W_i is use of i by other producers,

E_i is the export of i ,

M_i is the import of i .

Instead of having one determinant of the level of production, we therefore have four: three components of demand and one alternative source of supply. Although these four elements depend ultimately on some of the same explanatory variables, there is a different relation for each. The equation for the level of production will therefore be derived by combining the functions for the four components.

The Walrasian model takes factor supply functions as given and treats production levels, commodity prices, and factor prices as endogenous variables. Equilibrium values of these variables are determined from the simultaneous solution of equations for demand, factor use, and price formation. Total income is the sum of factor returns.

For the present analysis, income per capita (Y) is taken as an explanatory variable, thus avoiding the necessity of predicting income levels from factor inputs. The factor supplies are classified as labor (L), physical capital (K), human skills (S), sector-specific natural resources (R_i), and total natural resources (R). The size of the country, as measured by its population (N), is included as an exogenous variable.

The expectation of some degree of uniformity in patterns of growth is based on the existence of certain similarities in supply and demand conditions in all countries. These may be called "universal factors," which are distinguished from more variable "particular factors." Among the universal factors are: (1) common technological knowledge; (2) similar human wants; (3) access to the same markets for imports and exports; (4) the accumulation of capital as the level of income increases; (5) the increase of skills, broadly defined, as income increases. The present analysis is based on the assumption that these elements are much the same for all countries. From the similarity of the first three universal factors it follows that differences in production costs and commodity prices are determined primarily by differences in factor prices.

³To avoid a subsequent change in notation, all variables will be measured on a per capita basis, as is also done in the statistical analysis.

The general nature of the functions for the four determinants of sector output is as follows:

1. *Final use of each commodity* (D_i) is assumed to be determined mainly by per capita income. Houthakker's study [11] supports this assumption for household consumption, and it is consistent with the evidence for government consumption and investment although here the variation among countries may be larger. Following Houthakker, I assume a logarithmic function, since his results show fairly constant income elasticities for each commodity. The function suggested for final domestic use per capita is then:

$$(2) \quad \log D_i = \log \alpha_{i0} + \alpha_{i1} \log Y$$

where Y is per capita national income, α_{i1} is the income elasticity of demand for commodity i , and α_{i0} is a constant. In this and other equations, the subscript 0 refers to the constant term; the subscript 1, to the income coefficient; and 2, 3 . . . , to other explanatory variables.

2. *Intermediate demand for a commodity* (W_i) depends on output levels in the sectors using it, on the substitutability of other inputs for it, and on the extent of variation in relative prices of inputs. Here also, international comparisons [5] suggest that it is legitimate to ignore price effects in a first approximation and to make the Leontief assumption that producers' demands depend only on their levels of output. The function for intermediate use is then:

$$(3) \quad W_i = \sum_j a_{ij} X_j$$

where the a_{ij} are input-output coefficients.

If there were no foreign trade, equations (1) to (3) would uniquely determine per capita output as a function of the level of income. Specific resource supplies would limit the level of income achievable but would not affect the composition of output.⁴

3. *Imports and exports.* If all countries had the same per capita endowments of natural resources, the five universal factors would produce a regular change in the pattern of imports and exports as income increased. This will be called "Case A." With given resources and a constant ratio of labor force to population, a higher per capita output would come about only through an increase in capital and skills (including organization, etc.). If these both increased proportionately with income, there would be a variation in the ratio of capital and skills to labor, but at a given income level factor proportions would be the same in all countries. Relative prices and the patterns of trade would thus change systematically with rising income.

⁴ But the introduction of technological alternatives and of substitution in demand would permit some variation in the pattern of production, even in a closed economy, unless relative prices were the same in countries having the same income levels.

For exports of a given commodity (E_i) I assume a similar demand facing all countries. An export function for Case A can be derived from these assumptions as follows:

$$(4a) \quad K = K(Y); \quad S = S(Y)$$

$$(5a) \quad P_i = P_i(P_k, P_l, P_s) = P_i(K, S) = P_i(Y)$$

$$(6a) \quad E_i = E_i(P_i) = E_i(Y)$$

where all quantities are per capita, P_k , P_l , and P_s are the relative factor prices, and P_i is the commodity price. Under these assumptions, relative factor prices depend only on factor proportions, which in turn depend only on the level of income.

The cost of producing import substitutes would also be determined by equation (5a). At given import prices, domestic prices determine the commodities which can be more economically imported. The volume of imports, however, also depends on the three components of demand. An import function for Case A may therefore be written as:

$$(7a) \quad M_i = \mu_i(D_i + W_i + E_i)$$

where

$$\mu_i = \mu_i(Y, N)$$

Here μ_i is the fraction of total supply that comes from imports. For imported goods, the size of the domestic market has been introduced as an additional determinant of the cost of local production, and hence of the fraction imported, since many such goods are subject to economies of large-scale production.

When the assumption of uniform resources in all countries is abandoned (Case B), these functions become more complicated. Differences in the total supply of natural resources among countries imply a corresponding variation in the capital and skills required to produce a given per capita income. For Case B, equations (4a) must be replaced by:

$$(4b) \quad Y = \phi(K, S, R)$$

Second, differences in sector-specific resources (R_i) as between countries must be allowed for. Relative prices are no longer determined by the level of per capita income alone. The price (or cost) function (5a) may be restated as:

$$(5b) \quad P_i = P_i(P_k, P_s, P_l, P_r) = P_i(K, S, R_i)$$

Finally, differences in the supply of natural resources affect the terms of trade for different countries (measured by the labor and capital cost of earning or saving foreign exchange) and hence the extent to which it is economical to export or to substitute for imports in the manufacturing sectors.⁵ To reflect this fact, R , a measure of the total supply of na-

⁵For example, lack of natural resources causes Japan and Italy to import larger amounts of raw materials and hence to substitute domestic production for imports of

tural resources, should also be added to equations (6) and (7) under Case B:

$$(6b) \quad E_i = E_i(K, S, R, R_i)$$

$$(7b) \quad M_i = \mu_i(D_i + W_i + E_i)$$

$$\text{where} \quad \mu_i = \mu_i(K, S, R, N, R_i).$$

4. *Production levels* are determined from total demand in the same way as import levels, by multiplying by the fraction produced domestically $(1 - \mu_i)$. For both Cases A and B this gives:

$$(8) \quad X_i = (1 - \mu_i)(D_i + W_i + E_i).$$

Using equations (2), (3), and (6a) or (6b), the components of demand can be eliminated from equation (8) to give a function for sector growth containing only the exogenous variables and production levels in other sectors.

For Case A, this function is:

$$(9a) \quad X_i = [1 - \mu_i(Y, N)][W_i(X_1, \dots, X_n) + D_i(Y) + E_i(Y)].$$

In applying this function, it is convenient to consider output (X_i) as composed of two parts, the "normal" output for the country's size and income level, \hat{X}_i , and a deviation from normal, ΔX_i . Then $X_i = \hat{X}_i + \Delta X_i$. Under Case A, $\Delta X_i = 0$ for all i , and $X_i = \hat{X}_i$.

Substituting this expression into equation (3) gives:

$$(10) \quad W_i = \sum_j a_{ij} \hat{X}_j + \sum_j a_{ij} \Delta X_j = \hat{W}_i + \Delta W_i$$

where \hat{W}_i is the normal value of intermediate demand for a given size of country. For Case A, $\Delta W_i = 0$ since $\Delta X_j = 0$ for all sectors. The sector growth function then depends only on income and size:

$$(11a) \quad X_i = [1 - \mu_i(Y, N)][\hat{W}_i(Y, N) + D_i(Y) + E_i(Y)].$$

Under the less restrictive assumptions of Case B, resource differences cause variations in intermediate demand at a given income level, and the growth function becomes:

$$(11b) \quad X_i = [1 - \mu_i(K, S, R, N, R_i)][\hat{W}_i(Y, N) + D_i(Y) + E_i(K, S, R, R_i) + \Delta W_i(\Delta X_1, \dots, \Delta X_n)]$$

II. Statistical Estimation of the Sector Growth Functions

Although either time-series or cross-section data could be used for the estimation of the sector growth functions, the latter have very substantial advantages. For any particular country, it is not possible to separate the effects of universal and particular factors, and technology and trading possibilities change very substantially over a long period.

manufactured goods to an abnormal extent. The opposite may be said of Ceylon and New Zealand.

Among countries, however, size and income level are practically uncorrelated and the effects of the two can easily be separated statistically. Trading and technological possibilities are also much more similar at a given moment in time. Finally, data for international comparisons are more widely available. The estimation of sector growth functions in the present paper is therefore based on data for a number of countries, mainly for years between 1950 and 1956.⁶

Since no satisfactory measure of resources, either specific or in the aggregate, is available for any substantial number of countries, I shall base the statistical analysis on equation (11a), in which the only explanatory variables are income and population. The effects of the other variables in equation (11b) will be evaluated by an analysis of the residual variation.

The regression equations. There are two possible approaches to the use of equation (11a). Given data on the three components of demand, the more accurate procedure would be to estimate a separate function for each element and then to combine the results. However the number of countries for which a breakdown of demand into these three components can be made is relatively small. The alternative of estimating production and imports as single functions of income and size has the advantage of making possible the use of a much larger sample. It is therefore followed here.

A desirable form for the regression equation is suggested by equation (2) for final demand, which is logarithmic. (Intermediate demand, W_i , is a linear combination of all the final demands.) Since preliminary tests also showed that the logarithmic form fitted much better than a linear function for most sectors, I used a linear logarithmic regression equation in which per capita value added depends on per capita income and population:

$$(12) \quad \log V_i = \log \beta_{i0} + \beta_{i1} \log Y + \beta_{i2} \log N$$

where V_i is per capita value added, β_{i1} is the growth elasticity

$$\left(\frac{dV_i}{V_i} \middle/ \frac{dY}{Y} \right)$$

and β_{i2} is the size elasticity

$$\left(\frac{dV_i}{V_i} \middle/ \frac{dN}{N} \right).$$

A similar function is assumed for imports:

$$(13) \quad \log M_i = \log \gamma_{i0} + \gamma_{i1} \log Y + \gamma_{i2} \log N.$$

⁶ In a parallel study, [4], the same theoretical framework is applied to the analysis of Japanese growth patterns from 1914 to 1954.

It will be shown below that the separate estimation of these two equations also yields an estimate of the import ratio in equation (11a), μ_i , as a function of Y and N .

The two elasticities in these equations include both supply and demand effects. Since factor proportions as well as demands vary with rising income, β_1 and γ_1 , are called growth elasticities rather than income elasticities. Similarly, the size elasticities, β_2 and γ_2 , represent the effects of larger domestic markets both on the cost of production and on the demand deriving from import substitution in other sectors. β_{10} and γ_{10} are the values of V and M at $Y = \$100$, $N = 10$ million, which are taken as units of measurement.

*The regression analyses.*⁷ The sample of countries used for the analysis of manufacturing output is given in the first 38 countries of Table 1. These are countries for which both national income data and a postwar industrial census are available.⁸ I have used value added instead of total output as the dependent variable because the former is less affected by variations in product mix. The values given for the explanatory variables apply to the year of the industrial census. Results of the regression analyses for fifteen sectors of manufacturing are given in Table 2.

In order to extend the analysis to nonmanufacturing production and to check on the results of the preceding calculations, a second sample containing production data from national income sources was used. This sample includes 13 additional countries for which it is possible to calculate the average breakdown of the national product by major sectors in 1950-1955. Values of the dependent variable in each regression analysis were obtained by applying these percentages to the United Nations estimates of average per capita income in 1952-54. The regression coefficients are shown in Table 3.

For estimating the import function of equation (13), data are available for many more countries, and a sample of 63 was used. Commodities imported were classified according to the categories in the census of production in order to make possible the addition of imports and domestic output in each sector. An average for 1952-54, the years to which the national income estimates apply, was taken for all countries, since it seemed more important to have uniform trade conditions than to match the dates of the industrial censuses. The regressions are given in Table 4, which also includes primary products.

⁷ Additional statistical data and discussion of the methodology used are contained in a mimeographed appendix to the present paper, available from the Research Center in Economic Growth, Department of Economics, Stanford University.

⁸ Countries whose production data could not be reconciled with the two-digit international classification of industries or for which comparable national income estimates are not available (mainly communist countries) were excluded. Prewar censuses were used for Italy and the United States because the postwar census does not give comparable data.

TABLE 1—BASIC DATA FOR SAMPLES 1 AND 2^a

Country	Year	Popula- tion ^b (N)	Per ^b Capita Income (Y)	Average Share of Major Sectors in GNP, 1950-1955						Source
				IA Agri- culture	IB Min- ing	I Total Pri- mary (3+4)	II Indus- try	III Trans- port	IV Serv- ices	
		(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	
1. India	1950	358.29	58	48.4	0.9	49.3	16.6	—	—	(c)
2. Kenya	1956	6.15	66	49.7	0.9	49.9	13.5	7.0	29.6	(c, i)
3. Pakistan	1953	80.06	68	58.3	0.2	59.0	11.2	2.8	27.0	(c, i)
4. South Korea	1956	21.80	74	42.3	1.2	43.5	14.7	1.9	39.9	(c)
5. Egypt	1950	20.39	112	38.9	2.1	41.0	19.4	5.0	34.6	(c)
6. Ceylon	1951	7.74	117	53.4	0.1	53.5	13.3	7.4	25.8	(c)
7. Rhodesia & Nyasaland	1955-56	7.12	118	18.8	32.3	51.1	15.8	—	—	(g, i)
8. Iraq	1954	4.95	126	23.9	26.4	50.3	19.3	—	—	(f)
9. Peru	1954	9.21	137	32.7	11.9	44.6	19.9	—	—	(c)
10. Honduras	1955	1.66	143	52.3	1.6	53.9	14.0	5.0	34.6	(c)
11. El Salvador	1951	1.92	147	52.4	0.5	52.9	—	—	—	(e)
12. Turkey	1950	20.95	156	45.0	1.4	46.4	16.8	7.1	29.7	(d)
13. Guatemala	1946	2.50	164	45.5	0.4	45.9	21.2	—	—	(i, j)
14. Japan	1953	86.70	181	23.4	3.0	26.4	29.7	7.0	43.9	(c, i)
15. Brazil	1950	51.98	184	27.6	0.6	28.2	23.3	10.8	37.7	(e)
16. Mexico	1951	26.54	229	20.2	4.1	24.3	23.3	4.2	48.2	(e)
17. Colombia	1953	12.11	242	39.0	2.6	41.6	16.8	6.4	35.2	(e)
18. Italy	1938	43.60	250	25.1	1.2	26.3	39.6	6.3	27.8	(c)
19. Union of South Africa	1949-50	12.33	280	15.4	12.5	27.9	25.9	8.2	38.0	(c, g)
20. Chile	1952	6.30	285	14.9	5.4	20.3	20.6	7.3	51.8	(c)
21. Costa Rica	1950-51	.85	287	44.3	0.1	44.4	15.0	—	—	(d, j)
22. Lebanon	1955	1.43	327	19.7	0.1	19.8	16.0	4.6	59.6	(c, j)
23. Puerto Rico	1949	2.19	335	—	—	—	—	—	—	(c, j)
24. Ireland	1953	2.95	423	32.5	1.0	33.5	25.1	—	—	(c, h)
25. Netherlands	1950	10.11	448	12.0	1.0	13.0	41.9	8.5	36.6	(c, h)
26. Argentina	1950	17.19	542	18.1	1.1	19.2	28.7	—	—	(c, j)
27. Germany (West)	1954	49.52	563	9.9	3.0	12.9	47.1	7.7	32.3	(c, h)
28. Israel	1956	1.81	565	12.2	0.6	12.8	28.5	7.7	51.0	(c, j)
29. Finland	1955	4.24	727	24.7	0.2	24.9	41.1	7.2	26.8	(c, j)
30. Norway	1952	3.33	732	14.5	1.6	15.9	36.3	17.0	30.8	(c)
31. United Kingdom	1951	50.30	757	5.2	3.5	8.7	44.9	8.4	38.0	(c)
32. Denmark	1954	4.41	762	20.5	0.2	20.7	36.5	9.0	33.8	(c)
33. Belgium	1955	8.87	824	8.4	4.9	13.3	45.8	9.0	31.9	(c, j)
34. Sweden	1952	7.13	943	3.0	2.0	10.0	48.0	7.0	35.0	(h)
35. New Zealand	1952-53	2.01	958	23.7	0.9	24.6	29.9	8.9	36.6	(c)
36. Australia	1955-56	9.31	972	13.3	2.7	21.0	36.4	—	—	(j)
37. United States	1939	130.88	1,065	5.5	1.7	7.2	34.7	6.1	52.0	(c)
38. Canada	1952	14.43	1,291	12.1	3.9	16.0	38.5	7.6	37.9	(c)
39. Burma	1952-54	19.05	50	43.9	1.4	45.3	14.2	1.7	38.8	(c)
40. Bolivia	1952-54	3.13	55	28.9	25.4	54.3	19.8	—	—	(d)
41. Belgian Congo	1952-54	12.15	70	27.0	21.4	48.4	17.5	8.3	25.8	(c, i)
42. Thailand	1952-54	19.56	80	49.1	1.7	50.8	14.9	4.2	30.1	(c)
43. Ecuador	1952-54	3.46	150	38.9	2.1	41.0	19.4	5.0	34.6	(c)
44. Philippines	1952-54	21.04	150	42.7	1.4	44.1	14.8	2.9	38.2	(c, j)
45. Nicaragua	1952-54	1.17	155	40.5	0.5	41.0	—	—	—	(e)
46. Dominican Republic	1952-54	2.29	160	42.5	0.5	43.0	18.2	—	—	(i, j)
47. Portugal	1952-54	7.99	200	25.1	0.8	29.9	36.9	4.8	28.4	(c, h)
48. Greece	1952-54	7.82	220	35.2	1.1	36.3	24.2	6.5	33.0	(c, j)
49. Austria	1952-54	6.95	370	15.3	3.0	18.3	50.1	5.1	26.5	(c)
50. Venezuela	1952-54	5.44	540	8.0	27.0	35.0	24.2	—	—	(c)
51. France	1952-54	42.86	740	16.0	1.8	17.8	31.2	9.0	42.0	(c, i)

^a Sample 1 consists of countries 1 through 38, Sample 2 includes all countries except Puerto Rico. Sectors are defined from the International Standard Industrial Classification as Primary (0, 1) Industry (2, 3, 4, 5), Transport (7), Services (6, 8, 9).

^b Population and per capita income are given for census years only for countries 1-38 and for 1952-54 for the remainder. Population in millions from U.N. *Statistical Yearbook*. Per capita income in 1953 dollars from United Nations [17] and other U.N. publications.

^c Production data from U.N. Statistical Office, *Tables of International Comparisons of National Accounts Items, 1950-1955*; (mimeo.) 1959.

^d Production data from U.N., *Yearbook of National Accounts Statistics, 1958; 1959*.

^e Production data from U.N., E.C.L.A., *Producto Bruto, Inversión Bruta y Estructura de la Producción Industrial, 1958* (mimeo.).

^f Production data from U.N., *Economic Development in Middle East, 1956-57; 1958*.

^g Production data from U.N., *Economic Survey of Africa since 1950; 1959*.

^h Production data from O.E.E.C., *Industrial Statistics, 1900-1957; 1958*.

ⁱ Production data from Kuznets, [13].

^j Production data from country sources.

TABLE 2—REGRESSIONS OF PRODUCTION ON INCOME AND SIZE: MANUFACTURING SECTORS^a

ISIC No.	Sector	β_0	Growth Coefficients		Size Coefficients		\bar{R}^2	$S_{Y.YN}$	No.	Countries Omitted From Sample 1
			β_1	S_{β_1}	β_2	S_{β_2}				
(20-21)	Food and beverages ^c	3.85	1.129	.088	.001 ^b	.058	.846	.178	31	(1-3-6-18-25-28-33)
(22)	Tobacco	.51	0.928	.234	.234 ^b	.156	.344	.469	32	(1-2-4-14-18-28)
(23)	Textiles	1.00	1.444	.133	.401	.085	.770	.306	38	None
(24)	Clothing ^c	.50	1.687	.127	.065 ^b	.083	.837	.267	35	(1-3-6)
(25-26)	Wood, etc.	.35	1.765	.146	.080 ^b	.100	.815	.312	34	(1-3-18-20)
(27)	Paper	.04	2.692	.245	.518	.157	.784	.540	34	(6-10-26-28)
(28)	Printing	.32	1.703	.126	.177	.084	.854	.272	32	(1-20-25-26-28-33)
(29)	Leather	.09	1.642	.164	-.026 ^b	.103	.743	.372	37	(7)
(30)	Rubber	.06	1.998	.234	.438	.168	.713	.518	32	(1-18-20-23-24-28)
(31)	Chemicals	.51	1.655	.129	.257	.076	.846	.271	37	(15)
(32a)	Petroleum products	.12	2.223	.327	1.040	.222	.650	.742	32	(15-18-22-25-26-28)
(32b)	Petroleum products ^d	.06	1.568	.288	.670	.246	.592	.522	21	(same + 1-6-7-10-11 13-20-21-23-24)
(33)	Nonmetallic minerals	.39	1.617	.155	.164 ^b	.101	.747	.358	37	(22)
(34-35)	Metals, etc.	.34	2.143	.234	.419	.149	.726	.524	32	(6-17-20-23-24-33)
(36-37)	Machinery, etc.	.09	2.799	.231	.315	.120	.834	.498	30	(6-9-10-17-18-20-24-33)
(38)	Transport equipment	.18	2.327	.263	.256	.165	.717	.580	31	(6-9-17-18-20-23-33)
(20-39)	All sectors	8.83	1.620	.089	.085 ^b	.057	.900	.205	38	—
(20-39)	All sectors ^c	11.92	1.441	.069	.199	.045	.931	.145	35	(1-3-6)

^a Symbols: β_0 is a constant computed for $Y=\$100$ and $N=10$ million; β_1 and β_2 are the regression coefficients and S_{β_1} and S_{β_2} their standard errors; \bar{R}^2 is the coefficient of determination (corrected for degrees of freedom); $S_{Y.YN}$ is the standard error of estimate.

^b Coefficient not significantly different from zero at 95 per cent confidence level.

^c Sample excludes India, Ceylon, and Pakistan, for which census covers only establishments having 20 or more employees.

^d Sector (32b). Eleven zero-entry countries were treated as a separate population and omitted from the sample in order to allow for the existence of a large minimum size of plant. The estimate for the remaining countries is thereby improved.

Growth interpretation of the cross-section results. Historically, the growth of a country takes place in an environment in which trading possibilities and technology are constantly changing. The growth functions derived from cross-section analysis, on the other hand, represent the adaptation of countries at different levels of income to conditions of technology and trade existing at one time. Ideally, they may be thought of as indicating the path that a typical country would follow if its income increased so rapidly that conditions of trade and technology were relatively constant.

Over the past century, the change in the share of major sectors in the national product of the presently advanced countries has been quite similar to the pattern derived from cross-section analysis.⁹ Detailed comparisons of production trends in individual sectors have not yet been made for many countries, but a preliminary analysis of growth patterns in the United States and in six Latin American countries¹⁰ showed considerable similarity to the cross-section results given here. Pending further analysis of production series, it seems justifiable to

⁹ Kuznets concludes that "the direct evidence on long-term trends in the industrial structure of national product is thus remarkably consistent with that provided by the association of international differences in industrial structure and in level of per capita income" [13, p. 17].

¹⁰ Undertaken by the present author in collaboration with the Economic Commission for Latin America.

TABLE 3—REGRESSION OF PRODUCTION ON INCOME AND SIZE: MAJOR SECTORS^a
(1950-55)

Sector	β_0	β_1	S_{β_1}	β_2	S_{β_2}	\bar{R}^2	$S_{Y.YN}$	No.
I. Primary production	46.49	.494	.043	-.090	.032	.751	.122	48
a. Agriculture	38.98	.474	.062	-.082 ^b	.045	.574	.173	48
b. Mining	1.79	.935	.227	.129 ^b	.166	.244	.635	48
II. Industry	16.95	1.362	.039	.046 ^b	.029	.963	.109	48
a. Manufacturing ^c (factory only)	11.92	1.441	.069	.199	.045	.935	.145	35
b. Construction	4.06	1.152	.074	-.055 ^b	.051	.882	.180	34
III. Transportation and communications	4.64	1.288	.066	-.048 ^b	.053	.918	.161	36
IV. Other services	32.70	1.066	.038	.014 ^b	.030	.958	.093	36

^a Based on average percentage breakdown of national income in 1950-55 from Table I applied to average per capita national income in 1952-54. β_0 is a constant computed for $Y=\$100$ and $N=10$ million. β_1 and β_2 are the regression coefficients and S_{β_1} and S_{β_2} are their standard errors; \bar{R}^2 is the coefficient of determination (corrected for degrees of freedom); $S_{Y.YN}$ is the standard error of estimate.

^b Coefficients not significantly different from zero at 95 per cent confidence level.

^c Manufacturing from census data (Table 2).

TABLE 4—REGRESSION OF IMPORTS ON INCOME AND SIZE^a
1952-1954

ISIC No.	Sector	Income Coefficients			Size Coefficients		\bar{R}^2	$SM.YN$	No.
		γ_0	γ_1	S_{γ_1}	γ	S_{γ_2}			
0	Agriculture	1.17	1.396	.138	-.239	.091	.650	.432	60
1	Minerals								
11, 13	Crude petroleum, gas and coal	.07	2.363	.402	-.001 ^b	.259	.420	1.081	46
12, 19	Mining	.17	1.563	.177	.075 ^b	.117	.668	.450	39
2, 3	Manufactured goods								
20-21-22	Food, beverage, and tobacco	1.36	1.003	.141	-.374	.093	.552	.443	60
23	Textiles	2.05	.555	.119	-.536	.078	.547	.377	62
24	Clothing	.18	.866	.203	-.757	.126	.543	.524	45
25-26	Wood and furniture	.24	1.320	.154	-.406	.095	.677	.393	44
27	Paper	.43	1.118	.068	-.380	.043	.862	.203	56
28	Printing	.03	1.444	.285	-.331	.139	.506	.476	29
20	Leather	.15	1.143	.130	-.470	.084	.689	.361	49
30	Rubber	.24	.578	.118	-.540	.079	.584	.348	53
31	Chemicals	1.18	.956	.079	-.407	.051	.808	.242	57
32	Petroleum products	.88	1.007	.144	-.438	.093	.576	.432	55
33	Nonmetallic minerals	.28	.853	.112	-.478	.075	.649	.337	58
34-35	Metals	.96	1.192	.102	-.228	.064	.754	.300	53
36-37	Machinery	2.28	.964	.115	-.367	.071	.667	.336	55
38	Transport equipment	1.48	.790	.340	-.507	.214	.707	.313	54
	All imports	20.40	.987	.069	-.281	.045	.808	.217	62

^a The sample includes 14 countries of income less than \$100, 15 between \$100 and \$200, 16 between \$200 and \$400, 11 between \$400 and \$800, and 7 over \$800.

Import data for 1952-1954 from U.N. *Yearbook of International Trade Statistics*.

Symbols: γ_0 is a constant computed for $Y=\$100$ and $N=10$ million; γ_1 and γ_2 are the regression coefficients and S_{γ_1} and S_{γ_2} are their standard errors; \bar{R}^2 is the coefficient of determination; $SM.YN$ is the standard error of estimate.

^b Coefficients not significantly different from zero at 95 per cent confidence level.

interpret the cross-section results as normal growth functions, although experience with cross-section analysis in other fields of economics suggests caution in making use of this hypothesis.

III. *The Process of Industrialization*

Industrialization involves a number of changes in the economic structure, including: (1) a rise in the relative importance of manufacturing industry; (2) a change in the composition of industrial output; and (3) changes in production techniques and sources of supply for individual commodities. The first two will be measured from the regression analyses. I shall then try to determine the relative importance of changes in demand and supply in causing the growth of each industrial sector.

The rising share of industry. The preceding analyses give two separate measures of the rise of industry: (a) the increase in manufacturing alone, determined from census data (Table 2); (b) the increase of "industry" (manufacturing, construction, electric power, handicraft), derived from national income estimates.¹¹ The growth elasticity of manufacturing (1.44) is higher than that for all industry (1.36), of which it comprises some 75 per cent. The coefficient of determination (R^2) is high for both regressions: .931 for manufacturing alone and .963 for all industry.¹²

The changing shares of the major sectors in the national income are shown in Figure 1, which I interpret as the contemporary pattern of growth. The principal feature of this pattern is the rise in the share of industrial output from 17 per cent (12 per cent for manufacturing alone) at an income level of \$100 to 38 per cent (33 per cent for manufacturing alone) at a level of \$1000. The share of transportation and communication also doubles over this range, while primary production declines from 45 per cent to 15 per cent. The regression analysis confirms Kuznets' conclusion [13] that the share of services (other than transport) in national product does not vary significantly with the level of per capita income, since the regression coefficient for income is not significantly different from one at a 95 per cent confidence level.

¹¹ Censuses of manufacturing normally cover establishments of more than four persons and omit handicraft and other very small-scale manufactures. For low-income countries, the latter component is quite important in a few sectors.

¹² If the share of industry is taken as the dependent variable, the regression equation becomes:

$$\frac{V_m}{Y} = \beta_0 Y^{(\beta_1 - 1)} N^{\beta_2}$$

The estimates of the parameters are unchanged in this form, but \bar{R}^2 is reduced from .96 to .64 because the variance of the dependent variable is lower. The growth elasticity for the share of industry ($\beta_1 - 1$) is thus .36 with a standard error of .04.

Figure 2 shows the extent to which individual countries deviate from the normal relation between income level and industrial output (countries are numbered as in Table I). The standard error of estimate is equivalent to 28 per cent of output. The extremes are indicated approximately by the two dashed lines corresponding to outputs 50 per cent above and below normal. In a steadily growing economy, industrial output will increase by this amount over a twenty-year period if per capita income grows at 1.5 per cent a year. In these terms, there is

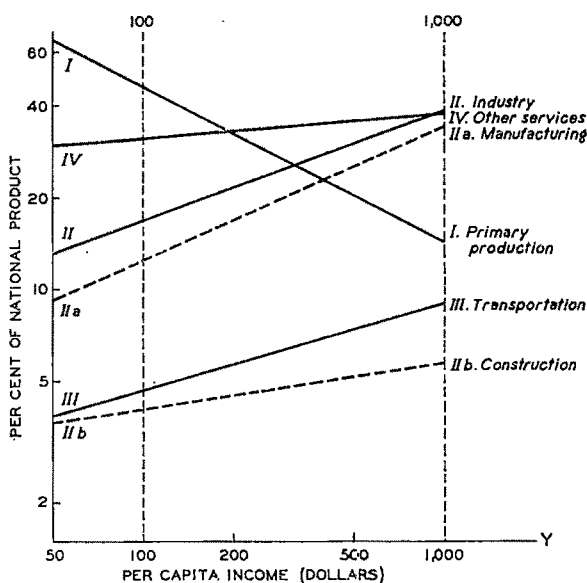


FIGURE 1. THE SHARE OF MAJOR SECTORS IN NATIONAL PRODUCT
(Logarithmic Scale. Population 10 million.)

no country in which industrial development is either advanced or retarded by much more than twenty years.

Figure 2 also shows that the linear logarithmic regression equation fits quite well for the range of incomes from \$100 to \$1000 to which I shall restrict my analysis. The fit would be somewhat better if size differences were allowed for. As suggested in the appendix, the regression would also fit the highest-income country, the United States, quite well if purchasing power exchange rates were used, since its actual exchange rate is substantially overvalued in terms of European purchasing power. Although no sector of the economy can have a growth elasticity greater than unity over an indefinite income range, the linear logarithmic function gives an adequate description of industrial growth over the range so far experienced in the world.

The pattern of industrial growth. The change in the composition of industrial output is just as marked as the change in the pattern of output as a whole. In Table 5, the regression equations from Table 2 have been used to determine normal output levels for three groups of industries, classified according to the nature of the demand for their products as: (A) investment and related products, (B) intermediate goods, and (C) consumer goods. As will be shown in the next section,

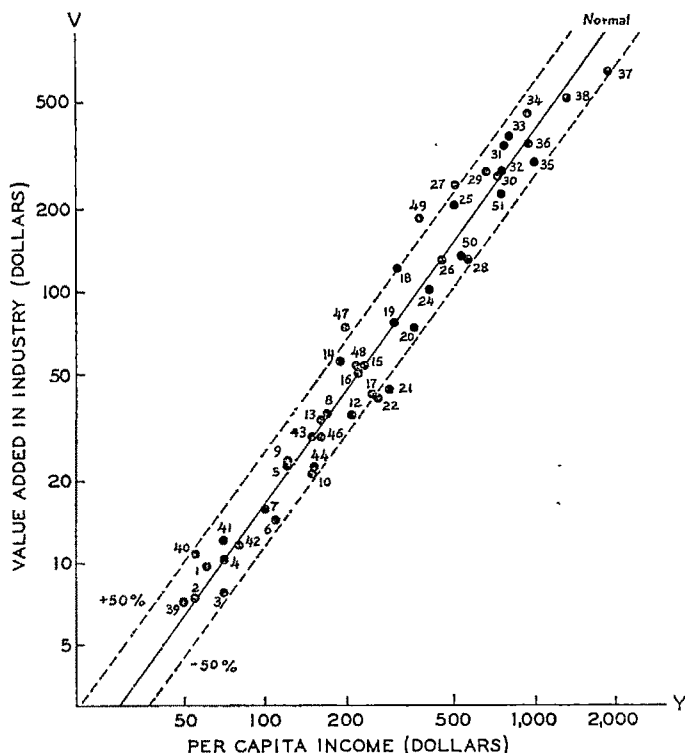


FIGURE 2. INDUSTRIAL OUTPUT AND INCOME LEVEL
(Logarithmic Scale)

there is considerable overlapping in these categories due to aggregation. Apart from metals and nonmetallic minerals, which go predominantly into investment goods, I have kept intermediate goods as a separate category because their growth characteristics are somewhat different from either of the other two groups. The opposite procedure is followed by Hoffman [10], who divides group B fairly arbitrarily between the other two and omits several mixed sectors.

The difference in growth elasticities between investment goods and consumer goods is almost as great as the difference between agriculture

TABLE 5—INCREASE OF MANUFACTURING OUTPUT WITH INCOME^a

ISIC No.	Industry Sector	Growth Elastic- ity β_1	Size Elastic- ity β_2	Normal Output at:			Ratio 600/100
				\$100	\$300	\$600	
Group A. Investment and Related Products							
36-37	Machinery	2.80	.32	.08	1.84	12.82	151.4
38	Transport equipment	2.33	.26	.18	2.28	11.44	64.6
34-35	Metals	2.14	.42	.34	3.62	15.97	46.6
33	Nonmetallic minerals	1.62	.16	.39	2.30	7.05	18.1
Subtotal		2.16		.99	10.04	47.28	47.8
Per cent				12.0%	23.6%	34.5%	
Group B. Other Intermediate Goods							
27	Paper	2.69	.52	.04	.76	4.94	124.1
32a	Petroleum products	2.22	1.04	.01	.13	.59	53.7
30	Rubber	2.00	.44	.06	.53	2.13	35.5
31	Chemicals	1.66	.26	.51	3.16	9.95	19.4
23	Textiles	1.44	.40	1.00	4.90	13.31	13.3
Subtotal		1.50		1.62	9.48	30.92	14.3
Per cent				19.7%	22.3%	22.6%	
Group C. Consumer Goods							
25-26	Wood products	1.77	.08	.35	2.46	8.36	23.6
28	Printing	1.70	.18	.32	2.06	6.71	21.1
24	Clothing	1.69	.07	.50	3.21	10.31	20.5
29	Leather products	1.64	— .03	.09	.53	1.65	18.9
20-21	Food, beverages	1.13	.00	3.85	13.29	29.07	7.6
22	Tobacco	.93	.23	.51	1.42	2.70	5.3
Subtotal		1.31		5.62	22.97	58.80	10.5
Per cent				68.3%	54.0%	42.9%	
Total A-C				8.23	42.49	137.00	15.6
20-39	All manufacturing	1.44	.20	11.92	57.99	157.40	13.2

^a For a population of 10 million. Calculated from regression equations in Table 2.

and industry. At an income level of \$100, 68 per cent of manufacturing consists of consumer goods and only 12 per cent of investment goods.¹³ At income level \$600, the share of group A has increased to 35 per cent of all manufacturing, while group C has fallen to 43 per cent. Group B maintains a fairly constant share of the total. (The interval from \$100 to \$600 will be used throughout to illustrate the effects of industrialization.)

The regression of value added on income and population also gives

¹³ The errors due to aggregation between groups B and C are approximately offsetting (see Table 6).

a reasonably good fit for almost all sectors. The coefficient of determination, R^2 , is below .70 in only two cases, tobacco and petroleum products, and its median value is .78. A similar result was found for the import regressions, where the median R^2 is .68. Except for the three sectors (food, clothing, printing) in which imports are a very small fraction of total supply, equation (11a) therefore gives almost as good an explanation of imports as of production. The import results are the more striking because a simple regression on either income or size shows a very low correlation. The scale variable explains about a quarter of the variation in production levels, but about half that in imports.

These results confirm the existence of a fairly uniform pattern of change in production and imports of industrial products as income rises. They also suggest that equation (11a) provides a useful first approximation for the study of the causes of sector growth. Among the variables in equation (11b) that are omitted from (11a), the effect of sector-specific resources is most noticeable. Resource variation probably accounts for the lower correlation coefficients in mining, agriculture, and petroleum products. The importance of other omitted factors will be investigated in Section IV.

The causes of industrialization. The present analysis sheds some light on the factors causing the industrial sectors to grow more rapidly than the rest of the economy. It is possible to distinguish three causes of industrial growth: (1) the substitution of domestic production for imports; (2) growth in final use of industrial products; (3) growth in intermediate demand stemming from (1) and (2). Only the first of these factors can be measured directly from the regression results. Additional information from a smaller sample of countries will be used to give an indication of the further breakdown between final and intermediate demand.

The phenomenon to be explained is taken to be the positive deviation from proportional growth for each industry. My procedure will be to calculate the deviation from proportional growth in each sector and then to use the sector growth function to explain the source of this deviation.

For the income range from \$100 to \$600, proportional growth consists of a six-fold increase in each element of demand, production and imports. From equation (11a), we can write:

$$(14) \quad X^p = \lambda X^0 = \lambda(1 - \mu^0)(W^0 + D^0 + E^0)$$

where the superscript 0 indicates the initial income level, p applies to proportional growth, and λ is the increase in income ($Y^1/Y^0=6$). From this definition, the following expression can be derived for the deviation of the actual production level from proportionality:

$$(15) \quad \delta X = (X^1 - X^p) = (1 - \mu^0)(\delta W + \delta D + \delta E) + (\mu^0 - \mu^1)Z^1$$

Here the symbol δ refers in each case to the deviation from proportional growth; X^1 and Z^1 are the values of production and total supply at the upper income level.

To apply this expression, we first need to measure the normal values of X and Z from the regression analysis. X^0 and Z^0 are derived from the value added given in Table 5, using the average ratio of value added to total output (v_i) for each income level:

$$(16) \quad X_i = \frac{V_i}{v_i}$$

Z^0 and Z^1 are taken as the sum of production and imports (with constant population):

$$(17) \quad \begin{aligned} Z^0 &= X^0 + M^0 \\ Z^1 &= X^1 + M^1 = \left(\frac{v^0}{v^1}\right)X^0\lambda^{\beta_1} + M^0\lambda^{\gamma_1}. \end{aligned}$$

On the basis of equation (15), the three causes of nonproportional growth can be stated as:

(1) Import substitution: $(\mu^0 - \mu^1) Z^1$.

This expression measures the difference between the growth in output with no change in the import ratio and the actual growth.

(2) Nonproportional increases in final demands:

$$(1 - \mu^0)(\delta D + \delta E) = (1 - \mu^0)(D^1 - D^0 + E^1 - E^0)$$

where, from (2) $D^1 = D^0\lambda^{\alpha_1}$

(3) Nonproportional increases in intermediate demand:

$$(1 - \mu^0)\delta W$$

Data for calculating these three components are given in Tables 5 and 6.¹⁴

The calculation may be illustrated as follows for Sector 23, textiles:

Element	Initial Value	Proportional Growth	Actual Growth	Deviation	Breakdown of δX
Production (X)	2.09	12.54	32.34	19.80	19.80
Imports (M)	2.05	12.30	5.54	-6.76	13.22
Import ratio (μ)	.495	.495	.146	.349	
Total supply (Z)	4.14	24.84	37.88	13.04	
Final Demand ($D+E$)	1.90	11.43	16.31	4.88	2.46
Intermediate Demand (W)	2.24	13.41	21.57	8.16	4.12

¹⁴ The calculation is as follows:

1. Import substitution is measured from the values of X^0 and M^0 in Table 6 by means of equations (16) and (17), using the normal value-added ratios in columns (1) and (2).

The difference between the actual production at income level \$600 (32.34) and the six-fold expansion of the initial production level (12.54) is shown as the deviation from proportionality (19.80). The breakdown of this total deviation into its component causes is:

- | | | | | | |
|------------------------------------|------|---------|---|-------|---------------|
| (1) Import substitution | .349 | (37.88) | = | 13.22 | (67 per cent) |
| (2) Growth in final demand: | .505 | (4.88) | = | 2.46 | (12 per cent) |
| (3) Growth in intermediate demand: | .505 | (8.16) | = | 4.12 | (21 per cent) |

Similar results for all sectors are shown in Table 7. These are summed to give an estimate of the relative importance of the three factors for all sectors of industry.

This analysis shows that the effect of income growth on final demand accounts directly for only 22 per cent of industrialization. To this should be added the intermediate demand deriving from the growth in final demand, which increases the pure demand effects to 32 per cent of the total deviation from proportionality.

The increased share of domestic production in total supply, defined here as import substitution, is more important than the pure demand effects, since it accounts for 50 per cent of industrialization. This total may be subdivided into three components, using a breakdown of the last term in equation (15):

$$(15a) \quad (\mu^0 - \mu^1)Z^1 = (\mu^0 - \mu^1)Z^r + (\mu^0 - \mu^1)(\delta D + \delta E) + (\mu^0 - \mu^1)(\delta W)$$

(1)	(2)	(3)
(111)	(43)	(37)
		(31)

The first term represents pure import substitution, that which would have taken place if there were only proportionate growth in demand. The other two elements result from the deviation from proportionality in final and intermediate demands. I have classed them as supply effects, however, because without a change in comparative costs and hence in import proportions they would be zero.

There remains a residual of 18 per cent in group (3), attributable to changes in prices and errors of estimation, which cannot be analyzed without further information. There is probably some net substitution of manufactured goods for other goods and services—e.g., for handi-

2. Initial final demands ($D^0 + E^0$) are calculated in column (6) of Table 6 by applying the average proportion of final to total demand in Japan and Italy (which are near the middle of the income range) to the total supply given in column (5). Final demand at the upper income level is determined from equation (12), using the income elasticities of column (13). Since manufactured exports are quite small in this income range, the same growth elasticity is applied to foreign and domestic demand.

3. Intermediate use is measured as the difference between total supply and final demand. Changes in this residual element include the effects of substitution on final as well as intermediate use and of errors of estimation.

TABLE 6—GROWTH OF DEMAND AND SUPPLY

ISIC No.	Sector ^a	Value Added Ratios at: Y=100 Y=600		X ^o	Demand and Supply at Income \$100					μ ^b	Growth Elasticity of:							
		(1)	(2)		(3)	(4)	(5)	(6)	(7)		M ^o	Z ^o	W ^o	Production (θ) (9)	Imports (10) (10)	Total Supply (11) (11)	Import Subst. (θ _m) (12)	Final Demand (13) (13)
A. Investment and Related Products																		
36-37	Machinery	.54	.52	.15	2.28	2.43	1.77	.66	.94					2.83	.96	1.55	1.28	1.60
38	Transport equipment	.65	.43	.28	1.48	1.76	1.41	.35	.84					2.55	.79	1.63	.92	(1.60)
34-35	Metals	.49	.44	.69	.96	1.65	.38	1.27	.58					2.21	1.19	1.85	.36	1.60
33	Nonmetallic minerals	.54	.56	.73	.28	1.01	.73	.28	.28					1.59	.85	1.47	.12	1.45
	Subtotal			1.85	5.00	6.85	4.29	2.56	.73					2.24	.97	1.64	1.27	1.59
B. Other Intermediate Goods																		
27	Paper	.50	.38	.08	.43	.51	.11	.40	.84					2.84	1.12	1.93	.91	1.60
32	Petroleum products	.64	.35	.02	.88	.90	.20	.70	.98					2.22	1.01	1.08	1.21	.80
30	Rubber	.44	.49	.14	.24	.38	.20	.18	.63					1.95	.58	1.46	.49	1.60
31	Chemicals	.41	.38	1.23	1.18	2.41	.72	1.69	.49					1.66	.96	1.46	.20	.70
23	Textiles	.48	.41	2.09	2.05	4.14	1.90	2.24	.50					1.53	.56	1.23	.30	1.20
	Subtotal			3.56	4.78	8.34	3.13	5.21	.57					1.72	.83	1.38	.34	1.34
C. Consumer Goods																		
25-26	Food products	.48	.41	.74	.24	.98	.62	.36	.25					1.84	1.32	1.75	.09	(1.75)
24	Clothing	.46	.39	1.10	.18	1.28	1.15	.13	.14					1.77	.87	1.71	.06	1.20
28	Printing	.57	.61	.56	.03	.59	.42	.17	.05					1.66	1.44	1.65	.01	1.15
29	Leather	.30	.35	.30	.15	.45	.27	.18	.33					1.57	1.14	1.45	.12	1.20
20-22	Food, beverage, tobacco	.31	.30	13.50	1.36	14.86	12.63	2.23	.09					1.16	1.00	1.15	.01	(1.15)
	Subtotal			16.20	1.96	18.16	15.09	3.07	.11					1.32	1.07	1.29	.03	(1.27)
	Total			21.61	11.74	33.35	22.51	10.84	.35					1.55	.94	1.40	.15	1.36

^a In each category sectors are ranked in order of the production elasticity (δ).

Sources:

Column (1) Based on 10 countries having incomes below \$150.

Column (2) Based on countries having incomes between \$150 and \$750.

Column (3) $X^o = 100/Y^o$ from Table 2; x^o from col. (1).

Column (4) From Table 4.

Column (5) $(3)+(4)$.

Column (6) Based on average ratios to total supply in Italy and Japan from [5], Table XV.

Column (7) Y^o/Y^o .Column (8) M^o/Z^o .

Column (9) From Table 2, corrected for change in value added ratio.

Column (10) From Table 4.

Column (13) Based on budget studies in Italy, Ireland, Holland, Finland, Austria, and Japan. For sectors 20-22, 25-26, and 38 and Group C total, the supply elasticity was used, since the final demand estimates were not consistent with these results.

craft products and personal services—which is not reflected in the income elasticities derived from budget studies. There is also some substitution of manufactures—e.g., fuels and fertilizer—for labor inputs in production.

The breakdown by product categories throws further light on the operation of supply and demand factors. In groups A and B, imports provide 64 per cent of the total supply of commodities at an income level of \$100. In all sectors except nonmetallic minerals, economies of scale

TABLE 7—THE CAUSES OF INDUSTRIALIZATION^b

ISIC No.	Sector*	Deviations				Effects of:						
		Import Ratio $\mu^0 - \mu^1$	Final Demand $\partial D + \partial E$	Inter-mediate Demand ∂W	Production ∂X	(1) Import Substitution	Per Cent	(2) Final Demand	Per Cent	(3) Inter-mediate Demand and Substitution	Per Cent	
Group A. Investment and Related Products												
36-37	Machinery	.60	20.5	2.6	24.0	22.7	94	1.2	5	.2	1	
38	Transport equipment	.65	17.6	4.4	24.8	21.2	86	2.8	11	.7	3	
34-35	Metals	.40	4.4	30.5	32.4	17.9	55	1.8	6	12.8	39	
33	Nonmetallic minerals	.35	6.2	1.7	8.3	2.5	31	4.5	54	1.2	15	
	Subtotal		48.7	39.2	89.5	64.3	72	10.3	11	14.9	17	
Group B. Other Intermediate Goods												
27	Paper	.65	11.0	2.2	12.5	10.6	85	1.7	13	.3	2	
32	Petroleum	.13	— .4	1.2	.8	.8	98	0	0	(.02)	2	
30	Rubber	.50	2.0	.8	3.5	2.6	73	.7	19	.3	8	
31	Chemicals	.29	1.8	20.3	19.1	9.6	50	— .9	— 5	10.4	55	
23	Textiles	.35	4.9	8.2	19.8	13.3	67	2.5	12	4.1	21	
	Subtotal		19.3	32.7	55.7	36.9	66	4.0	7	15.1	27	
Group C. Consumer Goods												
25-26	Wood products	.13	10.7	6.3	15.8	3.0	19	8.0	51	4.7	30	
24	Clothing	.11	3.0	16.6	19.8	2.9	15	2.6	13	14.3	72	
28	Printing	.02	.8	7.0	7.6	.2	2	.8	11	6.6	87	
29	Leather	.14	.7	2.6	3.0	.9	29	.5	16	1.7	56	
20-22	Food, beverage, tobacco	.02	23.9	4.2	28.1	2.6	9	21.7	77	3.8	14	
	Subtotal		39.1	36.7	74.3	9.6	13	33.6	45	31.1	42	
	Total		107.1	108.6	219.5	110.8	50	47.9	22	61.1	28	
										Induced by final demand		10%
										Residual		18%

^a Sectors are in order of β_1 in each group.

^b Source: Tables 5 and 6.

relative to the size of the market are substantial, as indicated by the scale elasticity; this is doubtless one of the main reasons for the high proportion of imports. In these two groups, the substitution of domestic production for imports is the cause of the high growth rate, accounting for 70 per cent of the total deviation. For consumer goods, on the other hand, the scale coefficient is not significantly different from zero in any sector and import substitution is a minor factor. The income elasticities derivable from budget studies, however, explain only about half of the

observed growth of consumer industries. In wood products, clothing, and leather goods, a shift from handicraft to factory production is probably equally important. This change in relative costs is analogous to the change in comparative advantage that is the main source of growth in the other two groups.

The growth elasticity of total supply (or total demand) varies relatively much less than the growth elasticity of production. For all industrial sectors, the average growth elasticity of supply is 1.40. Only three (food, petroleum and textiles) are less than this, and only two (paper and metals) are above 1.75. Among the three groups, the range is from 1.64 for investment goods to 1.29 for consumer goods. Intermediate demand in general grows more rapidly than final demand because import substitution requires increased production of intermediate goods; this accounts for the difference between 1.50 for the former and 1.36 for the latter.¹⁵

These results contradict the usual assumption that changes in the composition of demand are the main cause of industrial growth. If a country has an increase in income with no change in comparative advantage, the analysis suggests that only about a third of the normal amount of industrialization will take place. Changes in supply conditions, resulting from a change in relative factor costs as income rises, cause a substitution of domestic production for imports and, to a lesser extent, of factory goods for handicraft goods and services. These supply changes are more important in explaining the growth of industry than are the changes in demand.

IV. *Variation in Growth Patterns*

The differences in income level alone explain 70 per cent of the variance in the levels of total industrial output among countries and something over 50 per cent for the average sector of industry. I shall now examine the factors responsible for the remaining variation.

The most satisfactory method of determining the effect of other variables is to include them in the regression analysis. This requires an adequate measure, direct or indirect, of the theoretically relevant variable. Only for market size, for which population provides a satisfactory indicator, has this been possible, although several indirect measures of resource endowments were tested. A serious obstacle to the analysis of the remaining variation in industrial output is the existence of several sources of bias in the data.¹⁶ The effects of other elements—factor pro-

¹⁵ An allowance has been made for substitution phenomena in group C.

¹⁶ The main sources of bias are discussed in the appendix.

portions, income distribution, and national policies—will therefore be indicated in less precise ways.

Effects of market size. Recent theoretical discussions [12, 14, 16] have given considerable emphasis to market size as a determinant of industrial growth. An important by-product of the present study is a quantitative measure of the net scale effect for each sector of industry.

Market size is increased by a rise in either income level or population.¹⁷ For final products, total demand is given by equation (2): $DN = \alpha_0 Y^{\alpha_1} N$. The demand increases proportionately with population but generally more than proportionately with income, since α_1 is greater than 1 for most manufactured goods. If income level is held constant, however, population may be taken as an indicator of the net effect of market size.

When there are economies of scale in production, an increase in market size lowers costs and thus permits the substitution of domestic products for imports. An increase in size also affects output indirectly by increasing the intermediate demand from other industries which experience a substitution of domestic production for imports. This dual effect of size is indicated in the sector growth function, equation (11). The net relation between per capita output and population that has been estimated above therefore reflects economies of scale throughout the economy, both in the sector itself and in its customers. Only when intermediate demand is negligible can the result be imputed entirely to import substitution in the sector in question.

The quantitative effect of size on industrial output is shown in Table 8, which has been computed from the regression equations with the income level held constant at \$300. The table includes only the industries for which the scale coefficient is significant at a 95 per cent confidence level, but the coefficients are also positive in all but one of the remaining six sectors. Industries having significant scale effects produce about 40 per cent of manufacturing output at an income level of \$300 and 57 per cent at \$600.

Although the elasticity of output with respect to size is only .20 for manufacturing as a whole, the relevant range of variation is substantial. An increase in population from 2 to 50 million causes manufacturing output per capita to nearly double and the sectors having significant economies of scale to more than triple. Beyond some point, market size

¹⁷ The use of national population as a measure of the market area must be qualified by the geographical location and trade policy of the country. In Western Europe, both geography and trade liberalization favor an expansion of industrial markets beyond national borders. This is much less true in Latin America, Asia, or Africa, where protection of new industry is the predominant policy and transport facilities are much less developed. The quantitative significance of these factors will be considered below.

should have less effect,¹⁸ but at the income level of \$300 chosen here, economies of scale are probably significant up to a population of 100 million or more in most of these industries (a market equal to about 10 million people at U. S. income levels). In Latin America, where markets for manufactured goods do correspond quite closely to national boundaries, Brazil may be expected to have twice as much industrial output per capita as the countries of Central America because of scale factors alone. The actual differences are even greater, since the large countries of Latin America have predominantly positive residuals from

TABLE 8—NORMAL INCREASE OF MANUFACTURING OUTPUT WITH SIZE OF COUNTRY^a

ISIC No.	Sector	Size Elasticity (β_2)	Normal Output at Population of:			Ratio Column (4) to (2)
			2 mil.	10 mil.	50 mil.	
		(1)	(2)	(3)	(4)	(5)
23	Textiles	.401	1.95	4.90	9.36	4.80
27	Paper	.518	.33	.76	1.76	5.33
28	Printing	.177	1.37	2.06	2.73	1.99
30	Rubber	.438	.20	.53	1.08	5.40
31	Chemicals	.257	1.79	3.16	4.88	2.73
32/b	Petroleum products	.670	.07	.34	1.01	13.73
34-35	Metals	.419	1.37	3.62	7.05	5.15
36-37	Machinery	.315	1.11	1.84	3.06	2.76
38	Transport equipment	.256	1.27	2.28	3.47	2.73
Subtotal			9.46	19.49	34.40	3.64
20-39	All Manufacturing ^b	.199	42.05	57.99	79.92	1.90

^a For all sectors where β_2 is significant. Income level held constant at \$300.

^b Predicted from the aggregate regression equation which omits countries 1, 3, 6.

the regression equations while the Central American countries have predominantly negative residuals (see Table 9 on p. 648).

Effects of income distribution. On theoretical grounds, we should expect variations from the normal income distribution to affect the levels of demand and production for commodities having income elasticities substantially different from unity. The effects of abnormally unequal income distributions are notable in countries such as South Africa, Kenya, and Peru, in which predominantly European communities have much higher per capita incomes than the larger native communities. An indication of the importance of this phenomenon is given by applying equation (12) to each community separately and adding the results.

¹⁸ There are too few very large countries in the sample to demonstrate this fact statistically.

Assuming two communities, the following formula shows the ratio of true normal output, \bar{V} , to that calculated from the average income level:

$$(18) \quad \frac{\bar{V}}{V_e} = w_A \left(\frac{Y_A}{Y} \right)^{\beta_1} + w_B \left(\frac{Y_B}{Y} \right)^{\beta_1}$$

where

V_e is the output calculated from the average income level,

w_A is the fraction of population in community A,

w_B is the fraction of population in community B,

Y_A is the per capita income in community A,

Y_B is the per capita income in community B,

Y is the average per capita income for the whole country.

For commodities having unit or zero income elasticity, income distribution will therefore have no effect.

To illustrate this result, assume an average income of \$125, income levels of \$300 and \$50 for A and B, and 30 per cent of the population in A, which may not be far from the situation in Peru. From equation (18), the ratio of true to calculated values is 1.84 for a sector having a growth elasticity of 2.0 and 1.30 for an elasticity of 1.5.¹⁹

Although I have not been able to estimate the income-distribution effect in quantitative terms, it is probable that it explains the positive deviations in many sectors of manufacturing shown in Table 9 for Rhodesia-Nyasaland, South Africa, Kenya, Brazil, and Peru.

Factor proportions. Although natural resources are now given less importance as determinants of the rate of growth than they once were, their effect on the pattern of growth is undeniable. A common expectation [12, p. 351] is that countries lacking resources will turn to manufacturing at an earlier stage in their development in order to make up for their lack of primary products for export and domestic use. When they are successful, the result is to substitute capital and skills for natural resource inputs.

A quantitative analysis of this process is beset by many difficulties. Attempts to improve the regression analyses by introducing additional variables representing resource endowments have so far met with only limited success because of the inadequacy of the available measures. Since no single measure of resource endowments has proved satisfactory, a more promising approach is to compare countries which on several criteria can be classed as having relatively low or relatively high resource inputs for their income levels. This procedure has been

¹⁹ In this case, a better prediction of output comes from taking the high-income community alone if the growth elasticity is above 1.4.

TABLE 9—DEVIATIONS FROM PREDICTED OUTPUT AND IMPORTS^a
(Logarithms)

Country and Region ^b	Output of Major Sectors		Output of Manufacturing Total (+) (-)			Per Cent Positive	Total Imports	Manufactured Imports (+) (-)		Per Cent Positive
	Primary	Industry	(3)	(4)	(5)			(8)	(9)	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Africa										
2. Kenya			.152	11	3	79	.251			
2. Kenya and Uganda	-.093	-.008					.002	13	0	100
41. Belgian Congo	-.053	.068					.326	11	0	100
7. Rhodesia & Nyasaland	.025	-.023	.567	11	3	79	.408	1	0	100
5. Egypt	.016	.015	-.154	4	11	27	.052	12	2	86
19. Union of South Africa	.030	.006	.241	12	3	80	.246	9	2	82
Average of region	.015	.012	.202			66	.214			94
Asia										
39. Burma	-.138	.019					.029	6	2	75
1. India	.055	-.001	-.467	0	9	0	-.143	6	2	75
3. Pakistan	.106	-.165	-.431	3	7	30	-.223	2	8	20
4. S. Korea	-.077	-.021	.211	13	1	93	-.001			
42. Thailand	.015	-.035					.038			
6. Ceylon	.074	-.117	-.303	3	7	30	.200	6	4	60
8. Iraq	.118	-.010	-.064	8	7	53		9	6	60
44. Philippines	.096	-.138					-.047	10	2	83
14. Japan	-.020	.100	.333	12	2	86	.048	4	10	29
12. Turkey	.194	-.136	-.062	8	7	53	-.201	8	6	57
22. Lebanon	-.239	-.135	-.168	4	9	31	.074	4	8	33
Malaya							.402	14	0	100
28. Israel	-.290	.019	-.013	8	2	50	.048	3	10	23
Average of region	-.009	-.052	-.107			47	.019			56
Latin America										
40. Bolivia	-.109	.185					.069	2	1	67
Haiti							-.200	1	1	50
16. Dominican Republic	.012	-.014					-.065	4	8	33
43. Guatemala	.051	.047	-.100	4	11	27	-.255	1	10	91
10. Honduras	.079	-.110	-.063	5	8	38	-.126	4	10	29
45. Nicaragua							-.097	3	8	27
11. El Salvador			.075	5	10	33	-.160	5	8	38
9. Peru	.018	.043	.097	11	2	65	.055	7	2	78
15. Brazil	.033	-.027	.193	12	1	92	-.031	8	6	57
16. Mexico	-.068	-.006	.010	11	4	73	-.104	7	7	50
Paraguay							-.354			
17. Colombia	.160	-.152	.119	10	2	83	-.059	8	6	57
43. Ecuador	-.007	.016					-.256	0	6	0
Jamaica							.030	0	1	0
20. Chile	-.096	-.108	-.015	6	3	67	-.205	2	8	20
Panama							-.029	6	7	46
Cuba							.109	7	4	64
21. Costa Rica	.118	-.171	-.076	5	10	33	-.202	4	10	29
Uruguay							-.095	2	9	22
26. Argentina	-.025	-.023	.108	11	1	92	-.186			
23. Puerto Rico			-.062	4	8	33				
50. Venezuela	.223	-.098					.087	6	3	67
Average of region	.030	-.032	.026			60	-.099			43
Europe										
48. Greece	.056	.035					-.090	9	5	64
47. Portugal	-.048	.234					-.031	7	7	50
18. Italy	.062	.160	.176	7	1	88	.052	8	6	57
49. Austria	-.132	.272					.006	7	7	50
24. Ireland	.120	-.027	.004	7	5	58	.145	12	2	86
27. Germany	-.137	.156	.228	13	2	87	.059	9	5	64
51. France	.079	-.079					-.049	5	9	36
25. Netherlands	-.198	.176	.027	9	3	75	.352	12	0	100
30. Norway	-.068	.038	.046	10	5	67	.123	9	5	64
29. Finland	.112	.104	.031	12	3	80	-.052	3	6	33
32. Denmark	.059	.038	-.097	7	8	47	.080	10	4	71
Iceland							-.066	5	9	36
31. United Kingdom	-.213	.068	.132	8	7	53	.138	10	4	71
33. Belgium	-.090	.106	-.295	1	9	10	.202	14	0	100
34. Sweden	-.186	.105	-.023	7	8	47	.032	10	4	71
Switzerland								6	5	55
Average of region	-.042	.099	.022			61	.056			63
Other										
35. New Zealand	.167	-.084	-.170	5	10	33	.006	6	6	50
36. Australia	.157	-.019	-.038	4	11	27	-.015	10	4	71
38. Canada	.116	-.056	-.042	4	11	27	.098	8	6	57
37. United States	-.059	-.204	-.147	0	15	0	-.421	2	12	14
Average of region	.095	-.091	-.099			22	-.083			48

^a Sources: Cols. (1) and (2): Deviations computed from Table 3.
Col. (3): Deviations in all manufacturing from Table 2.
Cols. (4) to (6): Distribution of positive and negative deviations from Table 2.
Col. (7): Deviations in all imports from Table 4.

^b Countries are given in ascending order of per capita income within each region. The number preceding each country indicates its place in Table 1.

followed in another study of this body of data [3]. One result was to identify a characteristic pattern of import substitution. The high-resource countries (such as New Zealand, Denmark, and Costa Rica) tend to have relatively low domestic production of machinery, transport equipment, chemicals, textiles, and metals, and to compensate by high imports of these commodities, financed by high primary exports. The low-resource countries (such as Japan, Italy, Germany, and the United Kingdom) do the opposite; they offset low exports and high imports of primary products by high domestic production of these same groups of manufactured goods. Machinery is the sector most sensitive to resource endowment; a large proportion of it can be supplied more economically by imports when a country has a comparative advantage in primary production and exports. For this sector at least, the inclusion of a measure of trade in primary products in the regression equation does give a significant improvement in the results.

Regional differences. The residuals from the regression equations can be used to test the effects of a variety of other factors, such as climate, government policy, or cultural elements, on the levels of production and imports. A simple procedure is to classify countries according to the given characteristic and then to determine whether the variance among the means of the groups is significantly greater than would be expected from the variance within the groups. The present analysis takes up the effect of a regional grouping which reflects variation in several of these factors.

Deviations from the predicted values in all three sets of regressions are summarized in Table 9 for five regions. These are geographical groupings except for the classification of the United States and Canada with Australia and New Zealand. The variance ratio (F-test) was computed for the deviations in primary production, industrial production, and total imports by region. It shows that for primary output the regional difference is not significant, for industrial production it is significant at a 95 per cent confidence level, and for imports it is probably significant also.²⁰ The following reasons for these regional effects may be suggested.

Industrial output in Europe has an average positive deviation of .10 in logarithms, equivalent to 25 per cent above the predicted values. Asia and the four "other" countries (New Zealand, Australia, Canada, and the United States) have average negative deviations equivalent to 13 per cent and 23 per cent respectively. For the two high-income groups the difference is probably attributable mainly to the difference

²⁰ The chi-square test for deviations in imports is significant at a 99 per cent level (grouping Asia with Africa and "Other" with Europe), but an F-test of the deviation in regional means is only significant at 80 per cent.

in per capita resource endowments and to the earlier start of industrial growth in Europe. The negative deviations in industry in Asia are concentrated in a few sectors, since almost half of the sector observations have positive deviations. Colonial policies and cultural factors may be suggested as possible explanations of the small lag in industrial development in Asia, but it may merely be that the normal relationship is not a linear one. The normality of industrial output in the African countries contradicts the latter hypothesis, however.

In imports, Europe and Africa have positive deviations averaging 14 and 64 per cent respectively, while Latin American countries have an average negative deviation of 25 per cent. Protectionism in Latin America and relatively free trade in Europe and Africa provide a partial explanation, but the relatively high level of primary exports from Africa (and correspondingly high imports) also reflect its rich resource endowment relative to a low income level. (Latin America would have been much closer to this pattern thirty years ago.) Among Asian countries, the effects of colonial development of primary exports are only apparent in the high import levels of Ceylon and Malaya, and the regional average is close to normal. There is no normative implication of this result, however, since the regressions only indicate the prevailing conditions.

Although the regional breakdown does indicate the effect of some set of factors other than size and income on levels of industrial production and imports, the deviations are not sufficiently pronounced to reject the basic assumptions of similar wants and production possibilities on which the study is based. The regional associations do suggest that an attempt to identify other systematic influences on production and trade may be fruitful.

V. Implications for Resource Allocation

The association between industrialization and rising income tells us very little about the factors causing the rise in income itself. What the analysis does indicate is the pattern of resource allocation that normally accompanies a rise in income. Growth is likely to be accelerated by anticipating desirable changes in resource use and retarded by institutional arrangements or government policies that inhibit such changes.

Some of the specific conclusions which affect resource allocation policies are the following:

1. When allowance is made for variations in size of country, there is a well-defined growth pattern for individual sectors of the economy. Deviations from this normal pattern are smallest for services, agriculture and most manufactured consumer goods.

2. The greatest variation in output levels is in industries producing machinery, transport equipment, and intermediate goods, where economies of scale are most important. Differences in factor endowments are reflected mainly in the variation in proportions of imports and domestic production in these sectors.

3. Where a country deviates considerably from the normal output pattern, there is some evidence that lagging sectors of industry are likely to grow more rapidly than normal and tend to approach the normal pattern. A tendency of lagging sectors to approach normal levels was found in a preliminary study of six Latin American countries and also in Japan [4] and Israel.

4. Although additional evidence is needed to prove the importance of economies of scale, the association of output with market size in a majority of sectors strongly suggests their significance. For all but the largest underdeveloped countries, the introduction of regional markets would substantially increase the expected level of industrial production.

5. The explanation of industrialization in the twentieth century that emerges from this study is rather different from the nineteenth-century pattern. In Rostow's terminology, leading sectors are likely to be industries in which import substitution becomes profitable as markets expand and capital and skills are acquired. Even in Japan, the most successful of the low-income countries in increasing industrial exports, import substitution accounted for nearly 40 per cent of the rise of industry (from 23 per cent of GNP to 33 per cent between 1914 and 1954) as compared to less than 10 per cent for exports.²¹

6. Development policies are usually guided as much by analogy to other countries as by an explicit analysis of the factors peculiar to a given situation. While the present analysis has focused on the similarities in the pattern of growth, it has also revealed the substantial variation that exists and the need to separate particular from universal factors. An analysis of the part played by comparative advantage and other particular factors in a given country must therefore be added to a knowledge of general growth patterns to arrive at the best allocation of resources.

APPENDIX: SOURCES OF BIAS IN ESTIMATION

Biases in the statistical estimates arise from systematic errors of measurement, from conceptual differences between the statistical measures available

²¹ This result was derived in [4], which uses an analytical framework similar to the present study. In this case, it was possible to calculate the effects of substitution and technological change as a separate element, since the input-output coefficients at the end of the period were known. The low proportion attributable to changes in demand (16 per cent) is due to the fact that per capita income somewhat less than doubled in this period.

and those desired, and from the estimating procedures used. The following sources of bias may be significant:

1. The only verifiable error of measurement is the variation in coverage of the industrial censuses. In India, Pakistan and Ceylon the difference is most pronounced, since establishments employing less than 20 workers are omitted, whereas most of the other censuses include those above four or even less. These three countries have been omitted in two sectors where small plants are important. In the low-income countries, there is also some evidence of a lower coverage of the establishments that are nominally included. The apparent non-linearity of the production-income relations below \$100 per capita may result from this fact.

2. National income serves here as an indicator of both market size and relative factor costs. In neither case is a conversion at the official exchange rate appropriate; it is used only for lack of a feasible alternative. A conversion using purchasing power for consumer goods would give a better indication of market size. Gilbert and Kravis [8] give such rates for four European countries and the United States, and they have also been estimated for a few other countries.

The following formula gives the corrected ratio of observed to calculated values in manufacturing (\bar{V}_m^o/\bar{V}_m^c), allowing for the exchange rate applied to all income (P_y) and for the exchange rate applied to manufactured goods (P_m):

$$(19) \quad \log \left(\frac{\bar{V}_m^o}{\bar{V}_m^c} \right) = [\log V_m^o - \log V_m^c] + [\log P_m - \beta_1 \log P_y]$$

Taking the average of the four European countries in [8] as unity, the value of P_y for the U. S. is about .7 and for Japan [19] is about 1.33. The average wholesale prices of all manufactures are almost equal in Japan and the U. S. at the official exchange rate [19]. The price of manufactured goods must be close to unity in both countries, since their export trade is mainly in manufactured goods. On this assumption, the correction for the exchange rate would be of the following order of magnitude:

	$\log V_m^o - V_m^c$	P_m	P_y	$\log P_m$	$- 1.44 \log P_y$	$\log \left(\frac{\bar{V}_m^o}{\bar{V}_m^c} \right)$
U. S.	-.147	1.0	.7	0	.223	+.086
Japan	+.333	1.0	1.33	0	-.178	+.155

Although comparable corrections have not been made for other countries, it is probable that they would be considerably less than those indicated for Japan and the United States in most cases. This calculation suggests that the lower share of manufacturing in total output in the United States may be the result of differences in relative prices of manufactured goods and services, resulting

from differences in the growth of productivity, rather than from factors on the demand side.

3. Differences in value-added ratios, resulting from protection, monopoly or technological differences, are a third probable source of bias. Their effect is also included in equation (19). Although there does not seem to be a systematic relation between value-added ratios and income level in most sectors, there is some evidence that prices of manufactures are higher in the less developed countries. To the extent that there is a negative correlation between prices and income level, the growth elasticities will be understated.

4. Although the coefficients were estimated for each sector independently of the others, the total of the predicted values of output from Table 3 is close to total national income. The total is about 1 per cent high at an income of \$100 and 2 per cent high at an income of \$1000 and population of 10 million, but for other population sizes the difference may be larger.

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CREDIT CONTROLS AND FINANCIAL INTERMEDIARIES

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Two important views with regard to credit controls have been expressed in recent discussions of monetary policy. One view holds that existing controls of commercial banks are inadequate and recommends that new control techniques be investigated [11, pp. 588-606]; the other points to structural changes in the economy which have undermined the effectiveness of traditional credit controls and recommends that bank-type controls be considered for nonbank financial intermediaries [7, pp. 536-38] [13, p. 879] [3, pp. 81-82] [2, p. 223]. Both agree that monetary policy is insufficiently effective; both lead to policy recommendations for additional controls. It is the purpose of this paper to examine (with special reference to credit restriction) some of the proposals for additional bank controls and for extending bank-type controls over nonbank financial intermediaries.

I. Federal Reserve Control and Liquidity

At present, the only control over banks is the authority of the Federal Reserve to set reserve requirements for member banks. This authority is not in fact nearly so critical for control of commercial banks as is generally assumed. Legal reserve requirements are not the only and not even the most important means by which the central bank can influence commercial bank policy. Banks would hold liquidity reserves even without legal compulsion; and the Federal Reserve can have a powerful impact on the banking system without recourse to its authority over reserve requirements and even without direct dealings with banks. In the absence of legal reserve requirements, the Federal Reserve could tighten credit by open-market operations even if it were to refrain from making sales of securities to commercial banks. Open-market operations to mop up excess liquidity of the public could affect the banks because the latter are the repository for an important part of the public's liquidity. However, bank deposits are not the only source of funds for security purchases by the public. People could pay for the securities by liquidating near-liquid assets which are held in the form of the indirect debt of some other financial institutions. Although the

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public may tap nonbank sources of funds to buy the securities, the banks could not avoid the impact of the open-market operations. This is because assets from other sources must be converted into money before being used to pay for securities.

The main problem for credit policy is that all the instruments of credit policy together do not ensure sufficient control of bank liquidity.¹ The major way in which the banks can offset the pressure on their reserves is by tapping idle funds and thereby increasing velocity. To enhance the effectiveness of credit policy, additional controls have been considered: on bank assets, on bank lending, and on bank holdings of securities.

The proposal to control the volume of bank credit by basing reserves on bank assets rather than on bank deposits is not substantially different in effect from the present system of basing reserves on deposits. Moreover, it would not touch the problem of bank-induced increases in velocity; for banks would still be in a position to offset pressure on their reserves by tapping idle funds. The effectiveness of asset reserve plans which are designed to induce banks to keep their most liquid assets by providing for differential reserves against different types of assets would depend on the comparative profitability of different portfolio combinations. During a period of economic expansion, such plans might delay but would not be likely to prevent velocity-increasing portfolio shifts in banks.

The proposals to control bank lending may also be inadequate. Although most bank-induced increases in velocity are brought about by an expansion of bank loans based on security liquidations, bank lending per se is not at the heart of the control problem. If bank lending were controlled by law, bankers might increase their purchases of new securities. They might also develop loan substitutes which use new credit forms. For example, they might develop a kind of private-placement mechanism as a substitute for large business loans. Under such circumstances, central bank control of bank lending might not be enough to control bank-induced increases in velocity. Moreover, the activation of idle funds could take place, despite rigid control of loan volume and in the absence of suitable loan substitutes, if banks were to develop facilities for increasing the marketability of loans. Loan marketability (or shiftability) exists when the present holder of a loan can transfer it to another holder. At present, a bank can increase velocity when it acti-

¹ A study of clearing house banks in New York City revealed very wide fluctuations in liquidity ratios when liquid assets were defined as excess reserve balances at Federal Reserve Banks, one-year or shorter-term government securities, call loans to brokers and dealers, bankers acceptances, and commercial paper. Between 1947 and 1952, these ratios ranged from less than 20 per cent to more than 45 per cent of adjusted demand deposits [8, pp. 42-44].

vates idle funds by selling government securities to nonbank investors and uses the funds to make loans to customers. If there existed a market for loans similar to the market for securities, bankers could sell their loans rather than their securities. A degree of marketability for loans already exists for government-guaranteed mortgages because there is a secondary market for those mortgages.

¶The proposal for a variable secondary reserve composed of government securities of any maturities also might not assure adequate control for a variety of reasons. First, banks could shift from one to another maturity of U. S. government security. This shift among different government securities could be just as inflationary as a shift from securities to loans. Second, the authority to vary secondary reserves would no doubt be granted within fixed limits and, in a particular situation, these limits might not be adequate. Third, the plan could not prevent the unstabilizing consequences from liquidation of the uncontrolled part of the security portfolio. Fourth, a variable secondary reserve would not affect the bank's ability to tap idle funds by selling existing loans to nonbank holders in order to make new loans or to buy new securities and thereby to increase velocity. Finally, the authority of the Federal Reserve to impose a variable secondary reserve would probably cover member banks only. This could undermine the effectiveness of the plan not alone because of its limited coverage but also because the Federal Reserve authorities might be inhibited from exercising their powers fully lest member banks leave the Reserve system.)

The common weakness which is shared by the foregoing proposals, and which would limit their effectiveness, is the fact that they do not fundamentally impair the excessive liquidity of the banking system. As an alternative to more controls, it may be more efficient to strike at the sources of bank liquidity by altering the environment in which banks operate, i.e., by a structural change instead of by additional controls on the existing structure. In principle, both loans and securities are potential sources of liquidity for banks. At present, however, the superior shiftability of securities makes them more important than loans as a source of bank liquidity. The largest component in the security portfolios of commercial banks consists of the securities of the federal government. Bank holdings of government securities for short-term liquidity reserves are not troublesome for credit policy. The potentially troublesome government securities are those purchased with funds which are temporarily surplus on the customer loan market and which are held for income purposes pending a revival of loan demand. As a rough approximation, let us assume that the former category consists of Treasury bills and certificates while the latter category consists of longer-term securities. This division could be the basis of a possible

structural change in lieu of additional controls. For example, instead of a variable secondary reserve requirement, the same purpose might more effectively be served by requiring that, with the exception of short-term liquidity reserves, all other bank-eligible government securities be non-marketable.² This would preserve for banks most of the advantages of government security holdings while preventing their potentially destabilizing effects. The volume of reserves could still be altered by open-market operations and the potential money supply could thereby be kept under control. In addition, bank-induced increases in velocity would be significantly reduced.

An important source of bank liquidity at the present time, government security liquidation, would be precluded except at the discretion of the monetary authority operating through the discount window. This structural change would be more effective than imposition of a variable secondary reserve requirement because it would be the equivalent of a 100 per cent secondary reserve of government securities held (other than those constituting short-term liquidity reserves), effective for non-member as well as member banks of the Federal Reserve, without the disturbing effects on the banks of arbitrary changes in the percentage of required secondary reserves, without the possibility of banks tapping idle funds and activating them in the interests of some new government issue, and fully effective without compulsion on banks to buy and hold government securities.

It should be stressed that this structural change could not stabilize the velocity of money. Banks could still tap idle funds by liquidating corporate or municipal securities and, to a very small extent, by liquidating loans. In addition, the public could tap idle deposits by new primary security issues.³ However, this change could substantially reduce the degree of uncertainty which exists for the monetary authority about the impact of credit policy upon the liquidity position of banks. The main disadvantage for individual banks would be the greater inflexibility in portfolio management.

II. *Reserve Requirements and Nonmonetary Intermediaries*

This section examines the proposals to extend bank-type controls over nonbank financial intermediaries. A reserve requirement could be

² The absence of legal compulsion to hold these securities would help to raise their yields above the levels that might prevail in a completely captive market and perhaps above rates on marketables depending on the availability of suitable alternative outlets for surplus funds. The present volume of government securities in bank portfolios suggests that suitable alternative outlets for funds could not readily be discovered. For maximum effectiveness, the supply of Treasury bills would also have to be limited.

³ This amount of slack may even be desirable. Lawrence Ritter has argued that: "Within limits, monetary policy can make a positive contribution toward . . . [controlled expansion], in part because of—rather than in spite of—fluctuations in velocity" [9, p. 129].

most readily imposed on the intermediaries whose liabilities are nearest to money, the financial intermediaries in the savings deposit industry. The most important ones are commercial banks—with respect to time deposits—(TD), savings and loan associations (SLA), and mutual savings banks (MSB). Although the savings deposit industry operates de facto on a fractional reserve basis, only commercial bank time deposits are subject to legal reserve requirements imposed by the Federal Reserve. In analyzing the effects of extending the authority of the central bank to set reserve requirements, it will be assumed that any legal reserves which might be required for the deposit intermediaries would be held as deposits in commercial banks.

Direct effect of legal reserves on savings deposits. The deposit intermediaries,⁴ like the commercial banks, can raise the velocity of money (defined as demand deposits and currency). In order to be effective, therefore, credit policy which is aimed at the reserves of the deposit intermediaries must ultimately influence their velocity-affecting activities. However, it is possible for credit policy to affect velocity without affecting the volume of intermediary deposits. Thus, if intermediary reserve requirements were raised, velocity would be affected but the volume of intermediary deposits would be unchanged.⁵ It is also possible for an intermediary to change the amount of credit it grants (i.e., to increase the turnover of its credit) without making any change in the volume of its outstanding credit. For example, the volume of outstanding credit of a deposit intermediary would not be affected though the velocity of money would be increased, if the intermediary were to tap idle demand deposits by selling a loan to a nonbank holder and using the proceeds to lend to a new borrower. In a similar way, by a sufficient increase in the turnover of its loans, it would also be possible for a deposit intermediary to raise velocity while the volume of its outstanding credit actually fell. In view of these possibilities, in the discussion which follows, the actual variation in the volume of credit corresponding to different assumptions about changes in reserves may only approximate the variations in credit-granting activities.

⁴In this paper, hereafter, "deposit intermediaries" or "intermediaries" refers to SLA and MSB but does not include TD.

⁵Donald Shelby found only a small effect on liquid assets of the public (defined as the public's holdings of demand and time deposits and the shares of savings and loan associations and mutual savings banks) from assumed wide variations of reserve requirements on intermediary deposits. He concluded that "... if there has been any serious erosion of the powers of the Federal Reserve, it cannot be attributed to its inability to control directly the reserve ratios of intermediaries" [10, p. 539]. It should be noted, however, that variations in intermediary credit outstanding, and therefore also in velocity, as a result of variations in assumed reserve requirements for intermediaries are consistent with no changes whatever in liquid assets (as defined above). Moreover, as shown in the text, velocity can also vary without any change occurring in the outstanding volume of credit of the intermediaries.

A given increase in reserve requirements (assuming no excess reserves) would force asset liquidation of earning assets in both banks and deposit intermediaries. Higher reserve requirements on time deposits would not lead to an automatic reduction in time deposits but (*ceteris paribus*) it would lead to a reduction in demand deposits and to a corresponding reduction in bank credit. Thus, asset liquidation by banks restores reserve levels by reducing the volume of both assets and liabilities by a multiple of any increment which is impounded in legal reserves. Higher reserve requirements on other savings deposits (MSB and SLA) would not directly reduce the volume of those deposits as long as the public's propensity to hold those deposits remained unchanged.⁶ In deposit intermediaries, asset liquidation restores reserve levels by altering the composition of assets and by leaving the liability structure unchanged. Hence, the volume of outstanding credit in those institutions would decline, but only by an amount equal to the dollar increase in reserve requirements.

Although a given increase in legal reserve requirements would, in the absence of excess reserves, have a strong impact on the credit volume of commercial banks, it would have a comparatively weak impact on the credit volume of deposit intermediaries. This important difference in the effects of a given change in reserve requirements is not based on different assumptions about the level of reserves which might be required for bank deposits (including demand deposits) and for intermediary deposits. The difference exists because, following an increase in reserve requirements, intermediaries could increase the volume of intermediary reserves whereas the banking system could not increase the volume of bank reserves.⁷

In spite of these differences, the monetary authority could manipulate legal reserve requirements for intermediaries so as to bring about a contraction in the volume of intermediary credit comparable to the multiple contraction of credit in commercial banks. However, in order to achieve this similar effect, reserve requirements in the intermediaries would have to be increased substantially more than any increase in the legal reserve requirements for commercial banks. Under those circumstances, it may be misleading to treat legal reserve requirements for intermediaries as if they were simply an extension of a bank-type control. It is likely that the difference in degree of change required would be widely regarded as tantamount to a different kind of control.

⁶ This differs from the situation with demand deposits. Even if the public's propensity to hold demand deposits were unchanged after higher reserve requirements on demand deposits, the banks would be unable to sustain that volume of demand deposits, assuming no excess reserves.

⁷ Except by borrowing from the central bank: and that borrowing is subject to control.

Indirect and secondary effects. Could credit policy reach the deposit intermediaries indirectly by the application of pressure on the required reserves for commercial bank demand deposits? The answer would depend on whether (and if so, how much) the pressure on commercial banks could alter the volume of intermediary deposits. This would depend on the public's propensity to hold different kinds of deposits. Higher reserve requirements for demand deposits would initially alter the composition of the liquid asset holdings of the public by forcing a reduction in the volume of demand deposits. If the public wished to maintain the same distribution in its liquid asset holdings as before the higher reserve requirements on demand deposits, the pressure on demand deposits could be transmitted to the intermediaries and could force a contraction of their credit as well. On the other hand, if the public were disposed to maintain the level of its savings deposits, despite a decline in demand deposits, the pressure on demand deposits would not be transmitted to the deposit intermediaries.

The possibility that raising reserve requirements on intermediaries might have a secondary effect on the intermediaries could be analyzed in a similar way. Higher reserve requirements for intermediaries would not directly affect the volume of total deposits, demand and intermediary. However, the higher reserve requirements would reduce the volume of demand deposits available to the public because the intermediaries would impound additional demand deposits to hold as reserves. This change in the composition of the liquid asset holdings of the public could lead to a secondary effect on the intermediaries under certain assumptions about the public's propensity to hold different kinds of deposits. These propensities and their consequences for credit policy are examined in greater detail in the following section.

III. *Reactions to an Excess Demand for Money*

A restrictive credit policy on the part of the Federal Reserve would bring about an excess demand for money. A well-known textbook has explained the effects of an excess demand for money in this way: "When the demand for money balances is in excess of the supply of money, the community attempts to satisfy its excess demand for money by decreasing its expenditures for output, by borrowing more to add to its money balances, or by decreasing the supply of loan funds" [4, p. 272]. For our purposes, however, this description is insufficiently detailed because it does not look inside the aggregate of the "community." Everyone is not equally affected by, nor reacts in the same way to, the excess demand for money. This is more than a matter of degree: some may alter the composition of their assets in an effort to overcome the

shortage of money (availability effect); others may experience no shortage of money but may alter the composition of their assets because interest rates have changed (rate effect).

The reactions to an excess demand for money may also reflect the particular manner in which the tight money policy is carried out, because different methods of achieving the same initial quantitative credit impact could have different availability and rate effects. For credit policy, it would be important to know the specific reactions to an excess demand for money, because these reactions serve to communicate the impact of a restrictive credit policy to different financial institutions and to influence which particular institutions will be affected. Certain reactions to an excess demand for money could even have the result of converting a restrictive pressure into an expansionary effect for particular intermediaries. It is the purpose of this section to examine the most important reactions to an excess demand for money, to identify some of the major considerations which influence the public's choices among the alternatives, and to show the implications of different reactions for credit policy. The various ways in which the public can react to a tight credit policy are discussed in terms of the variables which are of special interest for credit control, viz., demand deposits, time deposits, SLA and MSB deposits, and securities.⁸

Interest Rate Effect

Shifts away from money. One possible rate effect of an excess demand for money could be the result of unequal movements of different interest rates. For example, concern has been expressed that higher interest rates associated with a tight money period, far from inhibiting credit extensions of the (deposit) intermediaries, may even stimulate their growth by raising the opportunity cost of holding cash balances [3, p. 50]. This is a possible reaction, but it needs to be restated to take account of differences among individuals. Unequal interest rate movements are not felt equally by all individuals, and, for some individuals, the rise in intermediary deposit rates may not signal a corresponding rise in the opportunity cost of holding cash balances. The increase in interest rates that could be earned on intermediary deposits may not induce bank borrowers to shift away from demand deposits because banks commonly require them to maintain larger compensatory balances (or banks enforce normal balance requirements more strictly) when credit becomes tight. Similarly, higher intermediary deposit rates may not induce some depositors to shift away from money. Periods of rising interest rates accentuated by a tight money policy are often associated with inflation. When inflationary rises in bank costs

⁸ The possible reduction of expenditure for output is ignored in this analysis.

lead to higher service charges on demand deposit accounts, the net advantage of shifting from demand deposits to other assets is reduced for some depositors.

A significant shift away from money to intermediary deposits may be held down for another reason. The fluctuation of intermediary deposit rates is a less sensitive (and, therefore, for some depositors, a less compelling) measure of the changing opportunity cost of holding demand deposits than the fluctuation of rates on other liquid assets, such as U. S. Treasury bills. Table I compares the fluctuations of Treasury bill rates, time deposit rates, and intermediary deposit rates from 1951-1957. It is clear that, during a period of generally rising interest rates, movements of the intermediary deposit rates were generally quite sluggish compared with movements of the Treasury bill rate.

TABLE 1—INTEREST RATES ON TREASURY BILLS AND VARIOUS SAVINGS DEPOSITS, 1951-1957

Year	Treasury Bills (per cent)	Time Deposits (per cent)	SLA Share Accounts (per cent)	MSB Deposits (per cent)
1951	1.55	1.1	2.7	2.1
1952	1.77	1.1	2.8	2.4
1953	1.93	1.1	2.9	2.5
1954	.95	1.3	2.9	2.6
1955	1.75	1.4	3.0	2.7
1956	2.66	1.6	3.1	2.8
1957	3.27	1.8	3.3	3.0

Sources: Treasury bill rates are reported in *Federal Reserve Bulletins*; other rates are given in [14, p. 22]. The savings deposit rates are effective interest rates, i.e., ratios of interest paid to the volume of savings deposits.

A shift from demand deposits to time deposits is another possible rate effect of an excess demand for money after a restrictive credit policy raises the opportunity cost of holding demand deposits as measured by the rate of return on time deposits. The decision about this shift would be influenced by the same considerations which have been described in connection with the shift from demand to intermediary deposits. In addition, a shift to time deposits could be inhibited by an effective ceiling rate on time deposits. However, for several years prior to 1957, the ceiling seems to have exerted no undue pressure on the time deposit rate in member banks.

A restrictive credit policy would be weakened for different reasons if it were to induce a shift away from demand deposits either to intermediary deposits or to time deposits. A shift from demand deposits to time deposits would per se transmit no pressure to the deposit intermedi-

aries. Moreover, the shift would offset Federal Reserve pressure on commercial banks because time deposits carry lower reserve requirements than demand deposits. Similarly, a shift from demand deposits to intermediary deposits would per se put no pressure on the commercial banks and it would offset a restrictive credit policy by the increase in intermediary credit. Although bank credit could expand when demand deposits are shifted to time deposits, because reserve requirements differ for time and demand deposits, the expansion of intermediary credit when demand deposits are shifted to intermediary deposits would not depend on lower reserve requirements for intermediary deposits than for demand deposits. The intermediaries could expand their credit, following a shift from demand to intermediary deposits, regardless of the level of fractional reserves which they might be required to hold. This net increase in credit could occur because the shift of deposits would not force the banks to reduce the volume of bank credit while it would enable the intermediaries to increase the volume of intermediary credit.

Shifts among nonmonetary assets. It has also been suggested that an excess demand for money might cause a shift from time deposits to intermediary deposits (as well as from demand deposits to intermediary deposits) because: "During periods of tight money, the rise in interest rates on primary securities enables nonbank financial intermediaries to raise the rates that they pay on their own obligations relative to the controlled rates paid by commercial banks on deposits" [6, p. 105]. The strength of this reaction depends upon the degree of responsiveness of deposit placement, as between different types of deposit institutions, to changing rate differentials among different deposit intermediaries, as well as the extent to which a tight money policy actually widens the interest-rate differences.

Deposit placement as between institutions is a function of many variables [1, pp. 1-22] and the interest rate is only one of these.⁹ Moreover, a tight money policy may not always widen interest-rate differences among the deposit institutions. For example, except during the recession of 1953-1954, the general level of interest rates moved upwards between 1950 and 1957. During those years of rising primary security rates, interest rates paid by the uncontrolled intermediary changed relative to those paid by the controlled intermediary—but not in the expected direction. Table 2 shows that the ratio of SLA interest-dividend rates to time deposit interest rates actually fell from 278 per cent in 1950 to 157 per cent in 1957. During the same period, the ratio

⁹ For small depositors, comparatively small rate changes are hardly worth bothering about because the disutility of shifting the account may exceed the utility of the negligible absolute interest difference.

of savings bank rates to time deposit rates fell from 222 per cent in 1950 to 143 per cent in 1957.¹⁰

There are many reasons why the higher primary security rates associated with a tight money policy may not widen interest rate differences very much among savings deposit institutions. On the one hand, rates in the controlled intermediary may rise when the ceiling rate is not seriously limiting. This appears to have been the case until 1957 when the ceiling rate was raised. In addition, rates in the uncontrolled intermediary may not rise very much as compared with the increase in the general level of primary security rates.¹¹ This is because interest-dividends are paid from earnings and not even gross earnings rates are tied directly to primary security rates. Most of the assets in the intermedi-

TABLE 2—DEPOSIT INTERMEDIARY RATES AND TIME DEPOSIT RATES, 1950-1957

Year	SLA Rate/Time Deposit Rate (per cent)	MSB Rate/Time Deposit Rate (per cent)
1950	278	222
1951	236	191
1952	245	218
1953	255	227
1954	223	200
1955	207	193
1956	188	175
1957	157	143

Source: Calculated from [14, p. 22]. The ratios are based on effective interest rates.

aries' portfolios would carry the old rate levels; only new credit would reflect new rate levels. Moreover, the new credit may not reflect the full increase in the general level of primary security rates because the deposit intermediaries deal with only a few kinds of primary securities. Their most important asset is usually mortgages; and mortgage rates, even on conventional mortgages, are less flexible than other primary security rates, such as those on government securities. Furthermore, the higher interest rates charged to borrowers may not be promptly reflected in higher dividend rates because, among other reasons, dividend rates are changed at discrete intervals. Finally, the dividend rates

¹⁰ During this period when time deposit rates were rising relative to intermediary deposit rates, the ratio of SLA deposits to time deposits was rising and the corresponding ratio for savings banks also rose somewhat. Specifically, the former rose from 40 per cent in 1950 to 78 per cent in 1957; the latter rose from 57 per cent in 1950 to 59 per cent in 1957 (reaching a high of 64 per cent in 1956). These ratios are calculated from [14, p. 13].

¹¹ For example, during the years 1950 through 1958, SLA (effective) dividend rates ranged between 2.5 per cent and 3.5 per cent; MSB interest rates moved between 2.0 per cent and 3.2 per cent. During this same period, Treasury bill rates fluctuated between .04 per cent and 3.23 per cent [14, p. 22] (*Fed. Res. Bulls.*).

which might be paid by deposit intermediaries may not be predictable solely from a knowledge of the changes in primary security rates because dividends depend on costs, on reserve ratios, and on deposit growth in the intermediaries as well as on their earnings from higher interest rates on primary securities.

Table 1 showed that rate movements both on time deposits and on intermediary deposits are generally smaller than the rate movements on another liquid asset, Treasury bills. These uneven rate movements suggest another possible rate effect of an excess demand for money, the shift from savings deposits to securities. This is an important alternative for savers and the slowdown in the accumulation of time deposits at commercial banks during the fourth quarter of 1958 has been attributed by one financial newsletter partly to the shift into Treasury bills of sizable deposits by corporate and institutional investors [15, p. 1]. The same source concluded that: ". . . the temptation to relate commercial bank losses in time deposits to savings and loan gains and to attribute such shifts to the rate differential between these institutions is probably an oversimplification of the events which are taking place" [15, p. 1].

During several years of generally rising interest rates on primary securities, interest rates paid by an uncontrolled intermediary (SLA) have not risen relatively to the rates paid by a controlled intermediary (commercial bank time deposits). This fact does not preclude the possibility that, with effective ceiling rates on time deposits, interest rate differences may widen in the expected direction during some future tight-money period and bring about a shift from time deposits to intermediary deposits. With respect to the implications for credit control, if an excess demand for money were to induce a shift among non-monetary assets from time deposits to intermediary deposits, the level of demand deposits would increase because intermediary reserves are held mostly in commercial banks. The net increase in demand deposits would be smaller than the amount shifted from time deposits to intermediary deposits (assuming the banks to be fully loaned) because reserve requirements are different for demand and time deposits. Hence, the volume of bank credit would decline. However, under present effective reserves held by banks and by nonbank intermediaries, the decline in bank credit could be more than compensated by the increase in intermediary credit.

When an excess demand for money raises the interest rates on liquid primary securities more than those on savings deposits, the public may decide to convert some of its savings deposits into securities. A shift either from time deposits or from intermediary deposits to securities would be the equivalent of a shift from time deposits or intermediary

deposits to demand deposits when the system is fully loaned. The credit control implications of this shift are discussed below in connection with the availability effect of an excess demand for money.

Availability Effect

The possibility that a tight money policy might induce a direct shift from savings (both time and intermediary) deposits to demand deposits is generally ignored in the literature. This neglect is probably because changes in the substitutability of savings and demand deposits are usually related to interest rate changes.¹² The shift is entirely plausible, however, when viewed as an availability effect of a tight money policy. Those directly affected by the shortage of money, either because they are denied credit from the usual sources or because the credit is very costly owing to higher borrowing rates, can restore money balances in this way. The credit control implications of a shift away from savings deposits to demand deposits will be different depending on which kind of savings deposit is transferred. When commercial bank time deposits are shifted to demand deposits, the volume of demand deposits would increase but the higher reserve requirements for demand deposits would force a contraction of outstanding bank credit. Hence, the net increase in demand deposits would be less than the amount which is shifted from time deposits. Moreover, this shift would have no direct effect on the volume of intermediary credit outstanding. By contrast, when the availability effect of a restrictive credit policy takes the form of a shift from intermediary deposits to demand deposits, the volume of commercial bank credit would not be affected but the volume of intermediary credit would decline.

To conclude this catalogue of possible reactions, the availability effect of an excess demand for money may take the form of a shift from securities to demand deposits. This well-known reaction to a tight money policy would not alter the quantity of money (though it would probably raise velocity) nor would it directly affect the intermediary credit volume. However, the shift would probably raise interest rates on primary securities and bring about some of the effects described earlier in this section under Interest Rate Effect.

IV. Credit Control and Deposit Intermediaries

In order to be effective [a policy of credit restriction must impair the liquidity of the intermediaries. The liquidity of commercial banks can be impaired in two ways: (a) by reducing their ability to tap idle funds

¹² The more usual expectation is for shifts from time deposits to demand deposits as a result of a fall in interest rates, i.e., during easy money periods. Cf. the implicit statement to this effect in [12, p. 545].

by means of asset liquidation, and (b) by reducing the amount of their liquidity reserves available for credit extension. The possibilities of impairing the liquidity of nonbank savings deposit institutions can be analyzed along the same lines.

✓ *Impaired asset shiftability.* Commercial bank liquidity could be impaired by permitting banks to hold only nonmarketable government securities, except for secondary reserves. Would a similar regulation for the savings deposit intermediaries have a comparable effect on the liquidity of the intermediaries? The answer depends on whether the sale of government securities is potentially an important source of funds for the intermediaries. Between 1951 and June 1959, the SLA government security account increased from \$1.6 billion to \$4.4 billion [5, p. 1169]; hence, the sale of government securities was not a net source of funds for SLA during this period. During the same period, although government securities were a source of funds for loans or other investments by MSB, the liquidation of those securities was not a very important source of funds. While the MSB government securities declined from \$9.8 billion to \$7.3 billion, the corporate and other security accounts increased from \$2.6 billion to \$5.6 billion and the mortgage account increased spectacularly from \$9.7 billion to \$23.8 billion [5, p. 1168].

For many years, the intermediaries have been able to dispose of their investable funds between mortgages and corporate securities, and they have not sought government securities as a repository for temporarily surplus funds [12, pp. 550-51]. Moreover, mortgages are normally the most important asset for the deposit intermediaries—and mortgages are the sole loan asset for which there is something of a secondary market. Accordingly, in the deposit intermediaries, loan shiftability is an important alternative to security shiftability as a source of funds. Under these circumstances, it is unlikely that credit policy towards the intermediaries would be made very effective by requiring the intermediaries to hold only nonnegotiable government securities.

✓ *Reduced liquidity reserves.* An impairment of intermediary liquidity by reducing the volume of intermediaries' reserves available for credit extension could proceed along three lines. The first is direct control in the form of legal reserve requirements for the deposit intermediaries. For reasons explained earlier, the direct effect of a given change in reserve requirements on the credit volume of an intermediary is much smaller than the corresponding effect on commercial banks. The intermediaries would experience a strong secondary impact from higher reserve requirements for the deposit intermediaries if the public could be induced to shift from intermediary deposit accounts either into demand deposits or into securities. However, these shifts would not be

more likely to occur because reserve requirements were imposed on the intermediaries than they would be if an excess demand for money were brought about without controls on the intermediaries.

A second possible way to reduce intermediary reserves or at least to retard their growth would be to equalize the competition between commercial banks and deposit intermediaries. One widely suggested proposal would subject all savings deposit intermediaries to uniform ceiling rates on interest-dividends paid to savers. The purpose of this control would be to limit the shift from demand and time deposits to intermediary deposits and to encourage the shift from intermediary deposits to securities when there is an excess demand for money. By itself, this policy is unlikely to be very effective. The competitive importance of rate differences as well as the extent of the rate differences among savings deposit institutions can be overemphasized. In addition, the rate difference between time deposits and other intermediary deposits is not fully explained by the ceiling rate on time deposits. In part, this is because the ceiling rate has not always been effective. Aside from this fact, however, there are differences in the economic circumstances between commercial banks and the deposit intermediaries which would make rate differences both possible and also, in the absence of rate regulation, likely [1, pp. 1-22]. Accordingly, a ceiling rate set at the same level as the ceiling on time deposit rates would simply permit more intensive product competition by those institutions which now pay more than the ceiling rate. Product competition could then substitute for rate competition and could undermine the attempt to extend the impact of an excess demand for money to the intermediaries. The alternative, which is to set the uniform rate ceiling at the highest level of rates paid by any of the savings deposit institutions, would be ineffective because it would leave the existing rate competition unchanged.

⌚ A third possible way to reduce the liquidity reserves of the intermediaries would be to impair their liquidity indirectly instead of introducing new controls. In this approach, pressure could be applied either directly or indirectly on the public's liquidity. In either case, the monetary authorities would concentrate on the reactions of the public to an excess demand for money. If the public were to react to pressure on its liquidity either by transferring intermediary deposits to demand deposits or by converting intermediary deposits to securities (necessarily passing through the stage of demand deposits), the intermediaries would lose liquidity reserves. In this indirect way, the intermediaries could be obliged to conform to the requirements of a restrictive credit policy. }

The indirect approach to the control of deposit intermediaries could

take a variety of forms. For example, as compared with direct regulation of the intermediaries, a restrictive credit policy might put greater pressure on intermediary liquidity if the commercial banks were permitted to hold only nonmarketable government securities (except for short-term liquidity reserves). As long as government securities continue to provide a source of funds for bank lending, the commercial banks will often be able to relieve the pressure upon the public's liquidity when there is an excess demand for money. If government securities held by banks were nonmarketable, the banking system could not use the Treasury's securities to frustrate central bank credit policy and the public would have to look elsewhere for relief. These circumstances increase the probability that the availability effect of an excess demand for money would take the form of a shift from near-money to money. Any shift of near-money away from savings accounts at the deposit intermediaries would reduce the liquidity of the intermediaries and force them to contract their credit volume.

Another example of an indirect way to reduce intermediary liquidity is suggested by the Treasury's experience in 1959 with the \$2.2 billion issue of 5 per cent notes with a maturity of four years and ten months, the so-called "Magic Fives" [16, p. 4]. In that operation, the Treasury was able to attract a large volume of deposits from savings deposit institutions with an appropriate rate magnet on marketable government securities.¹³ That experience suggests an important way for the Treasury to support monetary policy; it also suggests an important supplement to the Federal Reserve's own credit policy. Federal Reserve open-market sales to the public put pressure on commercial banks by absorbing the public's holdings of demand deposits. [The open-market sales could also reduce intermediary liquidity if the public were to pay for the securities by liquidating their intermediary deposit accounts. To a very great extent, the intermediary deposit accounts are held by small savers who are not accustomed to buying marketable government securities. The probability that open-market sales would tap intermediary deposits could be increased in various ways, such as greater efforts at public education designed to encourage small investors to think of marketable government securities as possible substitutes for intermediary deposit accounts. It would also be necessary to reorganize the government securities market which is not at present geared to handle small transactions. It may be useful, too, to experiment with new techniques to supplement open-market sales of securities, such as public subscription sales with securities supplied from Federal Reserve portfolios. Institutional modifications along these lines would raise

¹³ It would undoubtedly require more experimentation to discover how often and under what circumstances this kind of operation could be repeated successfully.

highly technical problems about the internal organization of the government securities market and would deserve careful study by experts in that area.

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*. THE RADCLIFFE REPORT AND EVIDENCE

A Review Article

By JOHN G. GURLEY*

A drama critic recently wrote of an evanescent Broadway play: "It was paved with good intentions, and, like most pavements, it was trodden underfoot." While for myself, I hesitate to call this an epitaph to the Radcliffe *Report*, it describes pretty well the initial responses of the *Report's* horde of critics, some of whom judged it the most monumental flop of the 1959 season, and most of whom felt that it had nice stage settings and all that, but was terribly weak in the theme.

These judgments are understandable and probably not grossly unfair. Nevertheless, I believe that they are based on the worst the *Report* has to offer and not on the best. At its worst, the *Report* is full of exaggerations, errors, confusions, conflicting statements, and careless writing. These no doubt represent, as the critics suggest, the "real" *Report*. But, if one attributes the exaggerations to a desire to escape the conventional, the conflicting and hazy statements to the task of achieving unanimity among diverse committee members, the confusions and errors to deadlines—if, in short, one excuses the weaknesses—and chooses the very best from its pages, the *Report* has much to recommend it.

In this idealized form, the *Report* presents a pioneering analysis of Britain's financial system, in which the monetary system and money are considered as only part of a complex, but integrated, structure of financial institutions, assets, and markets, and in which monetary policy, debt management, and fiscal policy are treated as coordinating techniques of a general financial policy aimed at regulating spending through this financial structure. In the underlying theme of the *Report*, all issuers of financial assets are relevant to financial policy; the private sectors issuing their debts and equities; the Treasury issuing various forms of government securities; the monetary system creating money and other claims; and nonbank intermediaries creating liquid claims. The idealized *Report* sees the level and structure of interest rates, which are the immediate targets of financial policy, determined partly by the whole range of financial assets—the level by the relation of liquid assets, including money, to holdings of financial and physical assets, and the structure to the composition of financial assets and demands for these components, with expectations playing their role in both cases. It sees money as only one asset among many, banks as only one type of institution among many, and the con-

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trol of money as only one aspect of an over-all financial policy. This is the *Report* at its best.

The Committee on the Working of the Monetary System, known as the Radcliffe Committee, was appointed by the Chancellor of the Exchequer in May 1957 "to inquire into the working of [Britain's] monetary and credit system, and to make recommendations." The Committee was composed of the chairman, Lord Radcliffe, two businessmen, two bankers, two trade union leaders, and two academic economists, the latter being Professors A. K. Cairncross and R. S. Sayers. During the fifty-nine days of hearings, scattered from July 1957 to April 1959, evidence was obtained from the Treasury, the Bank of England, clearing banks and other financial institutions, dozens of trade associations, business leaders, and many economists. In addition, the Committee received memoranda from many of these witnesses and from others, including the National Institute of Economic and Social Research, the Central Statistical Office, and, inevitably, a few monetary cranks.

The unanimous *Report* of the Committee was published in August 1959. This was followed in March 1960 by four volumes of evidence.¹ The first of these, *Minutes of Evidence*, is a huge, 980-page, double-column tome of the hearings. The other three volumes—*Memoranda 1, 2 and 3*—contain 142 papers submitted by various groups and individuals. As a rough guess, the *Report* and four volumes of evidence are packed with upwards of 3½ million words, a fact that I must note because it is possible that I have inadvertently slid over a hundred thousand or so here or there, and by so doing have done less than full justice either to the Committee or to some of its contributors.

I begin with a brief outline of the Committee's principal views on monetary theory and policy. I shall then discuss these views, in about the same order, supplementing the discussion with material from the oral and written evidence.² Since this procedure misses three areas that occupied much of the Committee's time—international finance, the status of the Bank of England, and financial statistics—I touch upon these briefly at the end.

I. *The Report's Monetary Theory and Policy: An Outline*

The following appear to qualify as the Committee's dominant views on monetary theory and policy:

1. The money supply has been largely uncontrolled during the postwar period; neither the banks' cash ratio nor their liquidity ratio has placed an effective upper limit on monetary growth.

2. But the money supply is of no great concern. First, it is incidental to the level of interest rates. These rates, though affected by liquidity, are determined largely by the expectations and confidence of the public, which in

¹The five volumes of the Committee on the Working of the Monetary System are: (1) *Report* (London 1959), pp. viii, 375, 15s; (2) *Minutes of Evidence* (London 1960), pp. ix, 980, £7; (3) *Memoranda 1* (London 1960), pp. vi, 308, £2 10s; (4) *Memoranda 2* (London 1960), pp. vi, 227, £2; (5) *Memoranda 3* (London 1960), pp. vi, 251, £2.

²Unless otherwise indicated, footnote references have the following meanings: R, 000 refers to the *Report* and its paragraph number; MinE, 000 refers to *Minutes of Evidence* and the number of the question-answer; M-1, 000 refers to the first volume of *Memoranda* and its page number; and similarly for M-2, 000 and M-3, 000.

turn are determined by what the monetary authorities say. Second, there is virtually no direct relationship between the money supply and spending.

3. Interest rates, in and of themselves, have had little or no direct effect on spending decisions.

4. While spending is not directly affected by interest rates, it is affected by liquidity, which is composed of the money supply and the money people can get hold of. The private sector's liquidity is increased by the lending of commercial banks and other financial intermediaries, because such lending increases the supply of loanable funds ("the money people can get hold of"), and the growth of liquidity stimulates spending. The important thing about financial institutions is not the liquid liabilities (monetary or otherwise) they create but the lending they do—the assets they purchase.

5. The lending of all financial institutions can be indirectly controlled through changes in the level and structure of interest rates. A rise in interest rates will slow down their lending by imposing capital losses on their security holdings. Thus, while a rise in interest rates has little direct effect on spending, it depresses spending indirectly by reducing the lending of financial institutions and so the public's liquidity.

6. This means that the "centre-piece" of monetary action is the level and structure of interest rates, and not the money supply. The structure of rates can best be influenced by debt management policy working on the composition of the national debt. Since, however, it is not a good thing to have highly fluctuating interest rates, and since the lending of financial institutions in ordinary times should not be directly controlled, more emphasis should be placed on fiscal policy as a short-run stabilizer, leaving monetary policy to set the tone of longer-run developments.

7. But, during emergency situations, when runaway inflation threatens, monetary policy should be used vigorously. In these cases, lending should be restricted directly, by controls on commercial-bank lending, perhaps by the extension of such controls to other intermediaries, by consumer credit regulations, and by restrictions on issues of long-term securities.

II. Control of the Money Supply

A. Definition of Money

The Committee regards the money supply as consisting of notes³ (but evidently not coins!) outside of banks plus net deposits, the latter being liabilities of London clearing banks and of Scottish and Northern Irish banks.⁴ However, nowhere in the *Report*—and, indeed, nowhere in the *Minutes of Evidence* or the three volumes of *Memoranda*—is there an extended series of the money supply in accordance with this definition.⁵ Bank deposits com-

³ Most of them issued by the Bank of England but some by Scottish and Northern Irish banks. R, 347, 349.

⁴ R, 388, 478.

⁵ Partial series, which are either inconsistent with one another, refer to different things, or are for different dates, are scattered around like Easter eggs. The *Report* shows total deposits of London clearing banks (R, 134) and of Scottish banks (R, 149), but only for 1958; net personal deposits in London clearing banks for four postwar years (R, 478);

prise not only current accounts, which earn no interest and are repayable on demand, but also deposit accounts, which are similar to our time deposits, not subject to check, and hence, strictly speaking, not used as money by the public.⁶ The Committee recognizes that deposit accounts are not quite the same as current accounts, but nevertheless they are counted as money. The reason seems to be that they are easily exchangeable for money.⁷ The inclusion of these accounts raises the question of why other highly liquid claims, such as deposits in acceptance houses, in hire-purchase companies, in Trustee Savings Banks, and so on, are excluded.⁸ While the Committee does lay down an explicit definition of money, at other times it goes out of its way to "fuzz up" the concept, by placing the supply of money in quotation marks, followed by "however that is defined," "whatever that may be made to mean," and similar derogations.⁹

B. Cash and Liquidity Ratios of Banks

The Bank of England normally attempts to control the volume of bank deposits in the traditional way—by controlling the supply of certain bank assets which are held, by convention and not by statute, in fairly constant proportions to deposit liabilities. The conventional cash ratio is 8 per cent, with cash consisting of vault cash and deposits at the Bank of England. Deposit expansion is also limited potentially by a conventional 30 per cent liquidity ratio, where liquid assets for this purpose include cash, Treasury bills, call loans, and commercial bills.¹⁰ There has been a spirited debate in recent years concerning which ratio, if either, is the effective one.

To reduce bank deposits, the Bank of England can decrease the banks' cash and thus their liquid assets by selling securities in the market. To regain liquidity, the banks can sell short-dated bonds, which do not count as liquid assets, to the discount houses, the latter financing the bonds by borrowing at call from the banks, with call loans counting as liquid assets. Or the banks may sell bonds and buy Treasury bills or commercial bills. It has been easy enough for banks to get liquid assets, but none of these operations replenishes their cash. For this purpose the banks must borrow from (or sell something to) the central bank. In the United States, borrowing is done directly; in Britain it is done indirectly through the discount houses. These houses hold

and a chart of the ratio of the money supply to national income for 1946-58 (R, 478). Elsewhere, data are presented for currency with the public and net deposits of clearing banks (M-3, 69), note circulation and clearing bank deposits (M-3, 184), in chart form the ratio of the money supply (currency with the public and net deposits of clearing banks) to GNP (M-3, 102), note issues and deposits of all reporting banks and acceptance houses, with intergroup items not omitted (M-2, 202-03), and in chart form the ratio of net bank deposits to national income (MinE, 10433).

⁶ R, 128-31.

⁷ R, 131. The Scottish banks appear to treat their deposit accounts somewhat differently. See, R, 153.

⁸ For example, see MinE, 8033-35 and 8132 for evidence that deposits in Trustee Savings Banks are more and more being used as current accounts.

⁹ R, 125, 504; also 395, 523.

¹⁰ R, 143-48, 351; M-1, 9-10

Treasury bills, short-dated government bonds, and commercial bills, and they borrow at call from London clearing banks and other banks. In addition, they and not the clearing banks are permitted to borrow from the Bank of England. When the clearing banks lose cash, therefore, they may call some loans from the discount houses. Under pressure, these houses may then either sell Treasury bills to or borrow from the Bank of England, and in these ways restore to the clearing banks the cash initially lost.¹¹ But, as the Bank explained to the Committee:

It should be noted that the creation of a cash shortage is not without effect. For, particularly, if it is such as to compel borrowing from the Bank of England, which is relatively expensive to the Discount Houses, they will try to reduce such borrowing so far as possible by raising their bids for money [that is, selling bills or short bonds], thus causing short-term interest rates to rise.¹²

This was further explained, in response to a question from the Committee, as follows:

If we wanted to raise interest rates, then we would give less or possibly no help in the ordinary way [by purchasing bills from the discount houses], and we would say: "If you want cash you must come to the Discount Office for it." And moreover we could if need be so arrange things that the market needed a great deal of cash. . . .¹³

This is the familiar process by which open-market sales of a central bank reduce the "cash" of commercial banks and force them to reduce earning assets and the money supply, raising market rates of interest. Commercial banks can get more cash from the central bank any time they want to, but they presumably have a demand schedule for cash that is negatively related to its price. Hence, if the central bank raises the price, the banks will demand less cash, and so less earning assets and less monetary liabilities.

The Committee seems to have misunderstood this evidence, taking it to mean that, since banks could get more cash at any time, the cash ratio was therefore ineffective in limiting the money supply. This confusion appeared several months later when the Committee was again questioning spokesmen for the Bank. The latter stated that during times of inflationary pressure it would be better for the government to finance a deficit by selling Treasury bills to the market than by borrowing from the central bank (by Ways and Means advances), inasmuch as, though either measure could increase the liquid assets of banks, the latter would raise their cash base. This prompted the following exchange:¹⁴

¹¹ R, 175, 355-58; M-1, 9-10; MinE, 183-89, 1568-96.

¹² M-1, 10.

¹³ MinE, 98. For a similar statement, see MinE, 421, which the Committee quotes in R, 360.

¹⁴ In this and subsequent exchanges quoted here, the first part of each paragraph is a question from some member of the Committee, and the second part, after the dash, is the answer by one of the witnesses. I have not thought it necessary most of the time to identify the interrogators and respondents.

I understood their policy of increasing deposit liabilities would be determined by their total of liquid assets, not the amount of cash? — The two things come out to the same, in the sense that the total of liquid assets in either case is affected in the same way; but the composition of their liquid assets would be different. With the Bank of England making Ways and Means Advances, their cash would be inflated. It would be inflated to a point beyond the 8 per cent, they require, and to that extent they would be looking round for earning assets. . . .

Is not this a different doctrine from what we have been told hitherto? — I do not think so. . . .

We are now back on the question of the whole control of the volume of bank deposits; we have been taught hitherto that it is the liquid assets ratio that matters?—Surely we have said there are two aspects of this control. . . .¹⁵

Passing over this and other evidence, the Committee in its *Report* states that the cash ratio was ineffective.¹⁶ Nevertheless, at other times, the Committee seems to recognize that this is incorrect, for it is stated, in substance, that the cash ratio would be ineffective only if the Bank of England set out to stabilize the Bill rate (which it generally has not done); otherwise, it would be effective.¹⁷ But it is the former view that carries the day. In the Committee's eyes, the more effective ratio is the liquidity ratio, though it is stressed that banks have found it comparatively easy to evade this, through the mechanism described above, and that it has been thirty years since bank lending has been restrained by liquidity considerations.¹⁸ In the end, one gathers that the Committee believes that the money supply has been largely uncontrolled.

These views are not easily reconciled with the fact that interest rates have increased sharply during the last decade in Britain, unless of course it is held that the money supply has nothing to do with the level of interest rates, a proposition I turn to in a moment. The banks have been subject, it is true, to requests to hold the line on or to reduce their advances (loans and overdrafts), but these restrictions have been short-lived, and in any case the banks could always purchase other earning assets and so increase their deposit liabilities.¹⁹ In view of all the evidence, it seems more reasonable to conclude

¹⁶ MinE, 2262-70. It should be noted, though, that at times even the Bank argued that the cash ratio was largely ineffective because it had to act as a lender of last resort to the discount market. (M-1, 38). However, it was brought out in testimony that this meant only that the Bank, as lender of last resort, would give the market, *at a price*, all the cash it wanted; it did not mean that the Bank, as lender of last resort, relinquished all control over interest rates (MinE, 94-101).

¹⁷ R, 120, 376, 430, 583. In fact, in direct opposition to the above evidence, the *Report* says: "But now that the credit-creating capacity of the banks is limited by the 30 per cent liquid assets' convention, an increase in cash precisely balanced by a decrease in the Treasury Bill issue has become irrelevant to the credit-creating power of the banks" (R, 167).

¹⁸ R, 376. See also R, 121, 355, 357.

¹⁹ R, 120, 167, 175, 505-7. For further discussion of the weaknesses of the liquidity ratio, see MinE, 183-89, 1568-96, 1654-58, 1909-13, with supplementary notes to 1909 on pp. 952-54, and 3738-65.

²⁰ R, 411, 417, 422; MinE, 2677 ff., 3265-72, 3523-3645; and M-1, 40. However, if the

that there has been more or less effective control of the money supply, at least since 1951, and that this control has probably operated primarily through the cash base of the banks, as described by Bank officials.

III. *Interest Rate Determination*

At one point in its *Report*, the Committee presents an admirable account of the determination of the level of interest rates. After extolling the role of liquidity in economic analysis, the statement continues:

We would nevertheless emphasize that the amount of money . . . is of considerable significance. The other classes of liquid assets . . . are inferior, in convenience to the holders, and this inferiority has to be compensated by the payment of interest. If there is less money to go round, in relation to the other assets (both physical and financial), it will be held only by people willing to make a greater sacrifice in order to hold it: that is to say, rates of interest will rise. But they will not, unaided, rise by much, because in a highly developed financial system . . . there are many highly liquid assets which are close substitutes for money, as good to hold and only inferior when the actual moment for a payment arrives . . . (i.e., the more efficient the financial structure, the more can the velocity of circulation be stretched without serious inconvenience being caused).²⁰

Unfortunately, this statement is in sharp conflict with what appears to be the Committee's principal view of this subject. While it can be reasonably argued that the *Report* does not in fact present a theory of interest rate determination—or, perhaps better, that it obliquely presents several—it is fair to say, I think, that the Committee looks upon “expectations and confidence” of the public as the chief determinant of rate levels—though lip service is paid now and then to the role of liquidity. And these expectations are greatly affected by what the authorities say, by the public's interpretation of their mood. That is, expectations are molded by the “faces” made by the authorities; presumably, a squinty-eyed look might raise interest rates, a vapid stare maintain them, and an ebullient expression lower them. When this theory takes over, the authorities never change interest rates by operating on the money supply or on liquidity generally; rather: “The authorities must seek . . . to influence the general liquidity situation by operating on rates of interest.”²¹ It is even stated that the money supply is incidental to interest rate policy.²² It is for this reason that the Committee can properly hold the view that the money supply has been largely uncontrolled in the face of sharp changes in levels of interest rates; for after all money has little or nothing to do with interest rates.²³

control over bank advances forces the banks to purchase other earning assets that are more liquid than those they would have purchased, a given money supply is likely to be associated with a higher interest rate structure, other things the same.

²⁰ R, 392 and footnote.

²¹ R, 504; also 385, 397 ff.

²² R, 397. This is also suggested in R, 394.

²³ In other words, while there may be a liquidity-preference schedule, drawn against

The theory that interest rates are determined by words and faces is indicated repeatedly in the *Minutes of Evidence*.²⁴ As an example, consider this exchange with Winfield Riefler, then assistant to the chairman of the Board of Governors of the Federal Reserve System:

Then is not much the most important thing you do not the buying and selling of Bills, not the raising or lowering of [discount] rates, but what you say?—No.

Why do you have to operate in the market as well as telling the market what you think about things?—Our operations in the market actually determine the funds available.²⁵

This evidently left the Committee incredulous. Though there are some indications that it was not willing to accept wholeheartedly the dominance of expectations in interest rate determination,²⁶ the truth is that this factor, by elimination, becomes the only solid explanation of rate movements in the *Report*, a theory which suggests that bond markets in Britain are peopled by a lot of nervous wrecks. The Committee was also influenced in this view by R. F. Kahn,²⁷ to whom I shall return later.

The Committee landed in this position despite the fact that F. W. Paish and others presented data to show the very close postwar relationship between the ratio of money to national income and bond rates,²⁸ and despite Paish's excellent testimony about this relationship. Here is a sample of it:

During the period up to 1956 during which there was this tremendous drop in the ratio of bank deposits to national income, was there not considerable liquidity in business? — If there is so much liquidity, why are firms willing to pay very high rates of interest for raising long-term loans on the London market?

They may expect prices to rise? — It is liquidity in relation to what they want to do. I would say that the long-term rate of interest is the inverse of liquidity.

Would this mean that you would feel that if you knew the ratio of bank deposits to net national money income you could predict what the rate of interest would be? — So long as the conditions remain, I would say almost exactly.

The point you are putting to us is that there is an inverse relationship

interest rates, the schedule is subject to such wide shifts, as expectations change, that interest rates are in fact little influenced by the amount of liquidity in the system.

²⁴ I admit that it is a tricky business to impute views to the Committee from questions asked and statements made by it during examination of witnesses. But I have done this only when such evidence seems consistent with the final views of the Committee as stated in its *Report*.

²⁵ MinE, 9760-61; also 10220, 10236. My emphasis.

²⁶ See, for example, R, 563, as modified by 565. Note also the Bank's stress on confidence as the prime determinant of interest rates in MinE, 2398-99, qualified slightly in supplementary notes of the Bank, pp. 955-56.

²⁷ R, 395. For Kahn's opinions on this, see M-3, 144.

²⁸ MinE, 10433; M-3, 102, 184-85.

between the liquidity of the system and the rate of interest, liquidity being defined not just in the terms of the money supply, but to include near-money? — I would put it in terms of the money supply. One would expect the amount of near-money to affect the shape of the curve. . . .

The issue is whether the relationship is sufficiently close and the lags sufficiently limited to allow of operational application? — I would say that in the short run one can get it down to pure expectations if one can persuade people that long term rates are going to fall and that they will have a heavy capital appreciation. There could be very marked temporary shifts on those expectations; but if the authorities wanted to stabilize the long term rate round about $4\frac{1}{2}$ per cent., they would have to allow the ratio of bank deposits to national income to rise to more nearly 40 per cent than 35 per cent.²⁹

And in response to an additional question about the effects of near-money, he answered:

I would say that whereas they could get a given rate of interest with a 40 per cent. ratio of bank deposits to national income if there was not very much near-money in the system, they might need 35 per cent. to get the same rate if there was a lot of near-money in the system. They would have to set off the increased liquidity due to large holdings of near-money by having less real money, in order to get the same effect on total liquidity.³⁰

Excellent memoranda and testimony were also submitted on this question by many others. Altogether, there seems to be an impressive body of evidence to support the view that the money-income ratio, modified by the presence of other liquid assets, and within the context of "real" variables, was the principal determinant of the level of interest rates in Britain during the postwar period.³¹ But, for some reason, the Committee chose to ignore this evidence.³²

IV. *The Direct Effects of Interest Rates on Spending*

The Committee adopts a "three-gears" view of interest rates. At any time, people believe that interest rates are either in low, middle, or high gear. Any play of rates within a given gear is not likely to have much, if any, direct effect on spending. If people "are to be shaken into some change of course, the gear must be changed."³³ A rise in the Bank Rate from $4\frac{1}{2}$ per cent to 7 per cent, for example, would generally be regarded as a shift from middle to high gear.

Nevertheless, though such upward shifts have occurred in postwar Britain, and so might have produced downward pressure on spending through the "interest rate effect," the Committee states that it found very little evidence that

²⁹ MinE, 10431-32, 10434, 10436, 10438.

³⁰ MinE, 10441. See also MinE, 10443-53 for further discussion of these points.

³¹ See M-3, 66, 179, 183-85, 85, 113, 146-48; and MinE, 10425-512. Outstanding memoranda on this subject were produced by James V. Morgan and J. C. R. Dow.

³² For one flagrant example, see R, 570, where the Committee presents three reasons for the rise in bond rates without once mentioning the money supply or national income.

³³ R, 442-43.

this has in fact happened.³⁴ According to the Committee, it found no evidence that higher interest rates, in and of themselves, reduced consumption; there was practically no indication that interest rates were important to large firms with respect to investment in either inventories or fixed capital; expenditures of the nationalized industries were also largely impervious to changes in interest rates; the same was true for local authorities' expenditures; and spokesmen for the smaller firms treated the interest rate effect with general skepticism.³⁵ "It has become clear that, as the system works at present, changes in rates of interest only very exceptionally have direct effects on the level of demand. . . ."³⁶ If we accept these statements, they would seem to dispose once and for all of the subject, as it relates to postwar Britain up to 1959.

But, from other material presented to the Committee by witnesses and others,³⁷ it is doubtful that the statements can be accepted in their present extreme form. When this evidence is compared with the conclusions of the Committee, it is hard to escape the feeling that its final appraisal should have been more guarded.

There is, first of all, the survey made by the Federation of British Industries of its manufacturing members with respect to the impact of higher interest rates in early 1955 on their investment decisions. Among the questions, was: "Was the rise in Bank Rate from 3 to 4½% during January and February, 1955 a *major* factor in taking your business decisions?" (Their italics.) Almost 12 per cent (179) of the firms answering (1526) said that it was. Of these 179 firms, 40 per cent were in the engineering, shipbuilding, electrical, and textile industries. The rest were found among more than a dozen other industries. These firms reported deferment or reduction of investment projects, or reduction or deferment of inventory purchases, or other action.³⁸

The Committee, in commenting on this survey in its *Report*, states:

The response rate . . . was relatively low, and it may well be that answers came for the most part from those who had some positive reaction to report . . . in discussions with us representatives of the Federation were not confident that these figures could be regarded as firm enough to be the basis for general conclusions.³⁹

The first part of this statement suggests that those who did not answer the survey may have had a lower ratio of yes-to-no answers than those who did. This may well be: but it should be noted that the questionnaire contained dozens of questions on other subjects, so that the nonrespondents could have had many other reasons for not participating.

As to the second part of the statement, I can find nothing in the *Minutes*

³⁴ R, 386.

³⁵ R, 450-51, 489, 495.

³⁶ R, 487. Sayers kept pressing the view on witnesses that an increase in interest rates will raise actual investment in the short run. His theory was that higher rates will lower consumption and so leave more room for investment, given aggregate demand. No witness accepted this, and it does not show up in the *Report*. See, e.g., MinE, 4189-92 5656-61.

³⁷ Some of this material appears in the *Report*, R, 452-53.

³⁸ M-2, 118-22.

³⁹ R, 453.

of *Evidence* to support it. What I do find is a continual "hounding" of the witnesses, a barrage of counterarguments and suggestions from the Committee, until finally the witnesses wilt under extreme pressure and state that "it might not be so." First of all, the Committee made it quite clear to the witnesses that it considered the figure of 179 out of 1595 "astonishingly high" and "most surprising," especially in view of the fact that the firms at the time probably anticipated inflation. The witnesses, after some hesitation, finally agreed that the figure might indeed be surprising. The Committee then asked them whether such expectations of inflation might not swamp the effect of higher rates [yes], whether other measures that accompany the higher rates might not be responsible for reduced spending [very likely], whether the interest rate can be isolated from other influences [yes], and whether the interest rate is really a factor taken into account before investment is made [not a principal factor]. Having prodded the witnesses into saying that interest rates are relatively unimportant, the Committee next asked them how, in view of that, the interest rate came to be a major factor with 179 firms. The answer and the Committee's response to it follow:

[First witness:] I am becoming nervous about how well the question was understood by these 179 people. [Second witness:] I do not think the 179 merely took account of the rise in interest rates alone; they took it in the context of the other measures. It is not explicit in the answers. [First witness:] We could go back to these 179 firms, and cross question them about this, and perhaps get more information.

It is rather vital to our discussions. We have had so many people tell us over so many years that, at any rate in the manufacturing industries, the rate of interest hardly affects these decisions at all, and one has come to think that whatever else Bank Rate changes can do they cannot do anything much about investment in the manufacturing industries. — [Answer not relevant.]

And, finally, after the witnesses agreed that the factor of uncertainty in investment projects diminished the importance of interest rates—still another observation having nothing directly to do with the survey results—we find:

That is exactly what the university lecturer says when he is lecturing. He is describing the position correctly when he says that? — In my limited experience, yes.

You will see why, after having had years of that, one finds these figures so surprising? — It is a complex of factors, and this expectation of inflation has, I am sure, been a very prominent consideration in that complex.

My experience as a partner in an issuing house is relevant [to what has just been said]. I cannot remember a case where a company has come to my firm, and asked what the cost of money would be. — [Answer not relevant.]

Is there not another aspect to this? A great deal of capital expenditure so classified is in fact a total of a very large number of small decisions taken throughout the period; in relation to those decisions all sorts of practical considerations of the market such as have been mentioned are

really far more relevant and major considerations than the cost of money?

— Yes.⁴⁰

Well, the theory of the university lecturer won out over the facts. "In discussions with us," the Committee said—and I come back to this without further comment—"representatives of the Federation were not confident that these figures could be regarded as firm enough to be the basis for general conclusions."

Another survey in March 1958 was taken by The Association of British Chambers of Commerce, inquiring about the effects of the credit squeeze after September 1957 on a wide range of companies. 16,000 questionnaires were sent out, but there were only 3404 usable responses. Of those stating that they had, since September 1957, experienced reductions in their turnover or in investment programs, only 4 per cent said they were due to the higher cost of borrowing, though 20 per cent attributed them to that plus tight money and restrictions of bank credit. And 30 per cent of those firms said that they had taken steps to reduce or pay off their bank borrowing because of increased costs.⁴¹ After noting this, the Committee throws doubt on the results in this way:

... but it may well be that the dramatic rise in cost, which certainly attracted much attention, was being blamed for reductions many of which were in fact dictated much more by expectations of a decline in the level of activity than by the rise in the rate of interest.⁴²

Its concluding remarks about these two surveys are also interesting:

The results of these questionnaires add up to substantial evidence that a proportion, big enough to be relevant to policy, of business firms were vaguely discomfited by the changes in monetary conditions in 1955-57, and especially in September 1957; but we have not found sufficient evidence to justify a conclusion that in the conditions of the 1950s the rise in interest rates would by itself have directly provoked a worthwhile curtailment of demand.⁴³

Aside from what is "worthwhile," the last part of this statement is necessarily true because the surveys did not seek to determine the amount of reduction in demand due to higher interest rates but only the number of firms reporting such a reduction.

A third survey was carried out, in October 1957, by the Birmingham Chamber of Commerce; questionnaires were sent to 3,400 member firms, and 610 responded. 185 firms reported that they had postponed or cancelled plans after 1955 for new factory or office buildings, extensions to existing buildings, replacement of machinery or equipment, or orders for new machinery or equipment. Of these, almost 40 per cent attributed such reductions to the increase in interest rates on borrowed capital.⁴⁴

⁴⁰ This is part of the full exchange found in MinE, 5566-5617.

⁴¹ M-2, 88-96; R, 453; MinE, 11119 (and footnote).

⁴² R, 453.

⁴³ R, 453.

⁴⁴ M-2, 87-88.

During the hearings with the representatives of the Association of British Chambers of Commerce, this was brushed aside rapidly, in the following way:

In the Birmingham inquiry about 40 per cent. of those who postponed or cancelled plans attributed this to an increase in interest rates. That is a very high proportion. Did it not surprise you? — I have no explanation of the results. It may be something that was peculiar to Birmingham. I wonder whether this may have had something to do with the recession in the motor industry.

If we leave the Birmingham inquiry aside for the moment, and concentrate on your own much more elaborate inquiry. . . .⁴⁵

As it turned out, the "moment" proved to be of infinite duration.

That is by no means all of the evidence submitted to the Committee that ran contrary to its general conclusions. Not all of the following evidence is clear-cut; in fact, much of it is at best sketchy, but all of it stood up under sharp questioning.

For example, representatives of the British Engineers' Association stated that higher interest rates increased their costs and so hurt their business;⁴⁶ those of the Country Landowners' Association testified that higher interest rates had cut back farm improvement projects substantially;⁴⁷ The Scottish Landowners' Federation had essentially the same story, claiming that capital expenditures in agriculture and forestry had been reduced by higher interest rates and would almost certainly be stimulated if credit became easier;⁴⁸ for this reason capital expenditures were also reduced by wholesalers, and their demand for inventories was curtailed;⁴⁹ it was the opinion of representatives of The Association of Investment Trusts that higher rates had cut back business spending all along the line;⁵⁰ a furniture dealer said that the rise in rates in September 1957 caused him to reduce capital expenditures, and that an increase in the Bank Rate "does come very much into our planning for the present and for the future";⁵¹ a dealer in wine and spirits offered similar testimony;⁵² witnesses for the Association of Municipal Corporations knew of several instances, involving water projects, housing programs, and so on, where expenditures were cut back because of higher interest rates;⁵³ a small amount of local authority's expenditures was said to be affected by a change in rates;⁵⁴ there is some evidence, though not much, that rubber merchants reduced inventories because of higher rates;⁵⁵ a survey carried out by the Council of Scottish Chambers of Commerce found that one-third of those firms replying stated that they had taken steps after September 1957 to reduce bank loans because of the higher cost;⁵⁶ the Committee heard that the high cost of money was a factor in reducing inventories of automobile dealers;⁵⁷ it also heard that retail chemists reduced or postponed capital expenditures and inventory purchases partly because of higher borrowing costs, though this was qualified in response to questioning;⁵⁸ a representative of the North of Scot-

⁴⁵ MinE, 11169-70. ⁴⁶ MinE, 6266-69.

⁴⁷ MinE, 6427-29. ⁴⁸ MinE, 6490-93, 6499. ⁴⁹ MinE, 6734-47. ⁵⁰ MinE, 7568-79.

⁵¹ MinE, 8149, 8155-67, 8171-75, 8178. ⁵² MinE, 8188, 8206. ⁵³ MinE, 8263-66, 8276. But see 8279 where this view seems to be modified. ⁵⁴ MinE, 8544. ⁵⁵ MinE, 8583-85.

⁵⁶ MinE, 8918. ⁵⁷ MinE, 11360-62, 11414. ⁵⁸ MinE, 11605, 11618-28.

land Hydro-Electric Board stated that higher rates undoubtedly slowed down their program for distributing electricity;⁵⁹ the building of an hotel was deferred partly because of higher borrowing costs;⁶⁰ evidence was put forth that some wholesale tobacconists, timber people, and others reduced inventories because rates went up;⁶¹ two bankers stated that the higher rates after September 1957 caused some businesses to reduce overdrafts;⁶² the same was reported by other bankers, who further claimed that merchants, and to a lesser extent, industrial concerns reduced inventories and that some capital expenditures were curtailed for interest-rate considerations, though this view was modified under questioning;⁶³ London clearing bankers, too, reported that bank loans probably fell after September 1957 due to higher borrowing rates.⁶⁴ The Committee also had the opinions of several economists that a change in interest rates was effective in altering spending throughout the economy;⁶⁵ and the Bank of England concurred with this.⁶⁶

It is far from certain, of course, what all this adds up to, especially since an equally impressive list could be produced for the other side. But, as a minimum, it would seem that the Committee's extreme conclusions are presumptuous in view of the evidence it heard.

V. Liquidity and Financial Institutions

As I have noted, the Committee all but eliminates the money supply as a factor in the determination of interest rate levels; it believes that changes in these rates have had little direct effect on spending; and it does not think that there is any direct, close connection between the money supply and spending. But, while money is shoved out of the house through the front door, for all to see, it does make its reappearance surreptitiously through the back as a part of general liquidity: and the most important source of liquidity is the large group of financial institutions.⁶⁷

This is the reason the Committee devotes much space in its *Report* not only to the monetary system but also to the large number of nonbank financial intermediaries. The more important of these intermediaries are the discount houses, hire-purchase companies, insurance companies, superannuation and pension funds, Post Office Savings Bank, building societies, and investment trusts. At the end of 1958 the assets of this group exceeded those of the monetary system by about 60 per cent.⁶⁸

It is no simple matter to discover in the *Report* by what process the intermediaries alter liquidity, for the Committee shifts around from one point of view to another and never does get down to definitions. But its principal view seems to be that the public's liquidity is composed not only of the

⁵⁹ MinE, 11726.

⁶⁰ MinE, 12287. ⁶¹ MinE, 12992-96. ⁶² MinE, 12952-55.

⁶³ MinE, 4976-86. ⁶⁴ MinE, 3617, 3646. ⁶⁵ M-3, 178-82, 182-88, 95, 213. ⁶⁶ M-1, 35-38.

⁶⁷ R, 125, 389.

⁶⁸ The monetary system is composed of the Bank of England, the London clearing banks, and the Scottish and Northern Irish banks. The assets of the separate financial institutions for several postwar years are given in the memorandum of the Central Statistical Office (M-1, 130-41), but the most recent data are in the *Report*, Table 20, and referred to in R, 313.

money supply but of the amount of money it can get hold of; at one point, the matter is put in even vaguer terms when it is stated that liquidity in the broad sense depends on "the amount of money people *think* they can get hold of."⁶⁹ Since people can get hold of money from financial institutions, liquidity is increased when such additional borrowing sources become available: the greater the number of potential lenders, especially institutional lenders, the greater is the public's potential liquidity, because it is then easier to raise funds.⁷⁰

Put somewhat differently, the notion is that the proliferation and growth of financial intermediaries increase the demand for "bonds," which "makes money more available" and stimulates spending for current output, even though the banking system is tightly controlled. With one possible exception, nowhere in the *Report* is there a statement as explicit as that, and nowhere does the Committee attempt to explain the process just described.⁷¹ The reader is simply left with the thought that if a new intermediary comes along, or if an old one grows, the aggregate demand for "bonds" is somehow increased—there is an increase in the supply of loanable funds, and more money is made available to potential spenders.

The Committee fails to note that the extent to which this is true depends on whether the growth of intermediaries reduces the public's demand for money—and so money becomes vital to the analysis. This may be illustrated as follows. Assume that there are three financial assets—bonds, money, and savings deposits—which are liabilities of the public, the monetary system, and nonbank intermediaries, respectively. Assume, further, for simplification, that the assets of all financial institutions consist only of bonds. Then, in the usual definition, the supply of loanable funds is defined:

$$(1) \begin{array}{l} \text{Supply of Loanable} \\ \text{Funds} \end{array} = \left\{ \begin{array}{l} \text{Planned saving by public} \\ + \text{Increase in stock of money} \\ - \text{Increase in demand for money (hoarding)}. \end{array} \right.$$

To simplify further, assume that saving and investment are done by different groups and that savers do not repay debts. Then planned saving is equal to the public's increase in demand for bonds, money, and savings deposits; the public's increase in demand for savings deposits is equal to nonbank intermediaries' increase in demand for bonds; and the increase in the stock of money is equal to the monetary system's increase in demand for bonds. Hence,

$$(2) \begin{array}{l} \text{Supply of Loanable} \\ \text{Funds} \end{array} = \left\{ \begin{array}{l} \text{Increase in demand for bonds by the public,} \\ \text{nonbank intermediaries, and the monetary sys-} \\ \text{tem.} \end{array} \right.$$

It may be seen, then, that an increase in demand for bonds by nonbank intermediaries will increase the supply of loanable funds only if it is not accompanied by an equivalent or greater decrease in demand for bonds by the public and the monetary system. Suppose the public increases its demand for

⁶⁹ R, 390. My italics.

⁷⁰ R, 316, 389, 390.

⁷¹ The possible exception is R, 392.

savings deposits, enabling nonbank intermediaries to increase their demand for bonds. Given the public's propensity to save, this means that there is a reduction in the public's demand for either money or bonds. If bonds, then the reduction in demand for bonds by the public is exactly offset by the increase in demand for bonds by nonbank intermediaries, and so the supply of loanable funds remains the same. On the other hand, if the shift is away from money, then the public does not reduce its demand for bonds, and neither does the monetary system reduce its demand for bonds (because a decrease in demand for bank liabilities will not alter the monetary system's outstanding liabilities and so will not change its bond assets). Hence, in this case, there is a net increase in the supply of loanable funds.

It is possible, therefore, that the purchase of bonds (lending) by nonbank intermediaries will not increase the supply of loanable funds. If it does not, then the public's increase in demand for money and savings deposits is matched exactly by the increase in the stock of these assets. There are more "liquid assets" in the economy, but there is also an equivalent increase in demand for them. The amount of funds made available to the public, at given interest rates and other terms of lending, is exactly the same as before; the growth of nonbank intermediaries has not changed the over-all situation.

It follows that the extent to which the growth of nonbank intermediaries will increase the supply of loanable funds (or "liquidity" in the Committee's terms, i.e., the excess stock of liquid assets) depends on the degree of substitutability between savings deposits and money, so that an answer to this question requires an analysis of the types of claims issued by the intermediaries. By concentrating on the asset side of the intermediaries' balance sheet, and neglecting the liability side, the Committee fails to come to grips with the problem. Its failure to compare what is being withdrawn from the market with what is being issued to the market—or, put another way, its failure to consider the demand for liquid assets as well as the supply of them—is at the heart of the difficulty. This one-sided view is reflected in many conclusions in the *Report*: that nonbank intermediaries are important only because they lend; that banks are important not because they create money but because they make loans; that it is not the money supply that should be controlled but bank advances; and so on.⁷²

I cannot say that the Committee is wrong in stating that nonbank intermediaries have increased liquidity and the supply of loanable funds, and have

⁷² On this last point, the basic difficulty with the Committee's approach stands out most clearly. It is stated that "regulation of the banks is required not because they are 'creators of money' but because they are the biggest lenders at the shortest (most liquid) end of the range of credit markets." (R, 504). That banks lend at the "most liquid end of the range of credit markets" is an argument against controlling them. The more the assets purchased by a financial institution resemble the liabilities it creates, the less need is there to control that institution. An institution, for instance, that purchased money and created money would not have to be controlled. (In another context, the Committee comes to this conclusion with respect to the note issues of Scottish banks which are backed by Bank of England notes.) Nor would one that purchased bonds and issued what the market considered to be identical bonds need to be controlled. Financial intermediaries become potentially more dangerous to the stability of the economy the more illiquid their assets are relative to their liabilities, given the rate and pattern of their growth.

done this in such a way as to exert a destabilizing influence on spending. These views may well be correct as applied to postwar Britain. The point is that the analysis leading to these conclusions is faulty, which not only leaves the reader unconvinced of them but leads the Committee down some wrong policy paths.

The Committee raises the question of whether the destabilizing activities of nonbank intermediaries should be controlled as the banks' are. On this it states:

If we are right in believing that the level of total demand is influenced by the lending behaviour [the asset side again!] of an indefinitely wide range of financial institutions, and not just by the supply of money, it might be supposed that we should substitute for the traditional control of the supply of money a complex of controls of that wide range of financial institutions. Such a prospect would be unwelcome except as a last resort, not mainly because of its administrative burdens, but because the further growth of new financial institutions would allow the situation continually to slip from under the grip of the authorities.⁷³

The *Report* later adds that:

Any severely restrictive control of [bank] operations is certain, over a period of time, to be defeated by the development of rival institutions; during the interim, the community will have suffered loss by interference with the most efficient channels of lending. We therefore begin with some presumption against discriminatory control of banks, at any rate in ordinary times.⁷⁴

In principle, then, there should be no discriminatory controls on banks in ordinary times; but only as a last resort should controls be imposed on other financial institutions; and whether they are imposed solely on banks or on a wider range of institutions they are bound to be undermined in the long run by the development of rival institutions. Leaving this somewhat ambiguous principle, which, not surprisingly, was endorsed unanimously by the Committee, it is stated that when you get right down to it the banks must be controlled.⁷⁵ However:

If, in the light of future experience, it should appear desirable to reinforce the authorities' power by raising the minimum liquidity ratios of the banks appreciably above their present levels, the practicability of imposing comparable restraints on other groups of financial institutions should be considered.⁷⁶

The question of controlling other financial institutions was raised with several witnesses, and other individuals expressed their opinions (or those of their organizations) in memoranda to the Committee. The most extensive discussion of this was carried on with M. H. de Kock, Governor of the South African Reserve Bank. Here is how a small part of it went:

⁷³ R, 394.

⁷⁴ R, 504.

⁷⁵ R, 505.

⁷⁶ R, 509. See also R, 510-11, 527.

Does it follow from [your] argument . . . that there is a danger that when you operate on the quantity of money the banks as particular financial institutions may be penalized, and that, if you are going to operate a credit squeeze, it should if possible operate widely over the whole field of finance and not narrowly on one set of institutions? — Yes. Years ago, when commercial banks were the main financial intermediaries, the authorities could by contracting bank credit achieve all that they wanted to achieve. In the thirties we considered that that was all that was necessary. Since that time the development of these other financial institutions has been far more active than that of the banks, and they are encroaching more and more on the field of the banks in all sorts of little ways; and today the banks are hampered in their attempts to follow the requests of the central bank.⁷⁷

Roughly similar views were expressed by T. Balogh and by representatives of the Trades Union Congress.⁷⁸ There were of course many dissenters, among whom was Winfield Riefler, who was torn between two opposing views: that nonbank intermediaries, when borrowing funds from the public, have no effect on monetary equilibrium; and that such intermediaries, when creating money substitutes, do upset monetary equilibrium.⁷⁹ He was not asked to reconcile these views. Others, including M. W. Holtrop, president of The Netherlands Bank, were on the same side. Holtrop stated that, inasmuch as nonbank intermediaries simply redistributed the community's savings, there was no reason to believe that they would create a monetary disturbance, but that his bank, nevertheless, collected data on their activities—why, he did not say.⁸⁰

VI. *Debt Management and Monetary Policy*

The Committee's views up to this point seem to leave us in a box. The money supply has little to do with the determination of interest rates; changes in these rates have had little direct effect on spending; spending is instead influenced by liquidity, which is increased when financial institutions lend; but only as a last resort should lending by nonbank intermediaries be directly controlled, and only in emergency situations (as we shall see) should bank lending be so controlled. How is spending, then, to be regulated effectively by monetary techniques?

The answer is that the authorities can *indirectly* control the lending of financial intermediaries, and, hence, the liquidity and spending of the public, by changing the level and structure of interest rates. A rise in interest rates, for example, will slow down lending by these intermediaries to the private sector by imposing capital losses on their security holdings. Or, as Sayers put

⁷⁷ MinE, 9377. For de Kock's full views on this, see M-1, 289-90.

⁷⁸ M-3, 37; MinE, 10172-80.

⁷⁹ This testimony is in MinE, 9822-27. Riefler also dealt with this problem in his memorandum, in which he stated that nonbank intermediaries play a neutral role in the saving-investment process and that control of the money supply has pervasive influences throughout the whole financial structure. See M-1, 301.

⁸⁰ MinE, 11812-18. For similar, though milder, statements, see the memorandum of The Bank of Australia, M-1, 249, and the British Treasury's statements, MinE, 1607-11.

it during the hearings, "you insert the gelatine of uncertainty into their liquidity."⁸¹ Moreover, if the national debt is lengthened at the same time, the liquid-asset base of commercial banks will be limited and thus their lending depressed.⁸²

This brings debt management to the head of the class, because it is the principal means of affecting the level and structure of interest rates, and the rate structure should be considered "the centre-piece of monetary action." It is through this mechanism that regulation is exerted on the lending ability of banks and other financial intermediaries and on the public's ability to spend.⁸³ The authorities, in managing the debt, should not concentrate exclusively on short rates but should give a lead to the market on long rates as well; it is this structure of rates they should keep their eyes on and not "some notion" of the money supply.⁸⁴ In principle, during inflationary periods, the debt should be lengthened and interest rates raised; during deflation, the opposite policies should be followed.⁸⁵

Thus the national debt, and not the money supply, becomes the focal point in the economy for the control of interest rates, and through these rates, for the control of institutional lending and thus the economy's liquidity and spending. In this set-up, banks are important because they are key lenders:

It is the level of bank advances rather than the level of bank deposits that is the object of this special interest; the behaviour of bank deposits is of interest only because it has some bearing, along with other influences, on the behaviour of other lenders.⁸⁶

In looking upon the national debt as the focal point of financial control, the Committee claims that it is following the evidence submitted to it by Kahn.⁸⁷ But, when one looks at this evidence, it is apparent that the Committee has gone somewhat further than Kahn did. Kahn stated his main thesis on this point in the following way:

Within wide limits it is possible to achieve any desired structure of interest rates *by a suitable combination* of monetary policy with management of the National Debt. The Exchequer, by issuing short-dated securities in the place of long-dated, or *vice versa*, can secure the desired shift in relative rates of interest *against the background of a monetary policy which operates on rates in general*.⁸⁸

Now Kahn further added that the authorities, in conducting a monetary policy, should not control the money supply "*as an end in itself*" but only because the money supply, other things given, determines the general level of interest rates.⁸⁹ He underlined this by saying:

⁸¹ MinE, 9366.

⁸² R, 374, 393.

⁸³ R, 395, 514, 603.

⁸⁴ R, 499, 395.

⁸⁵ R, 562.

⁸⁶ R, 395.

⁸⁷ R, 393.

⁸⁸ M-3, 145. My emphases.

⁸⁹ M-3, 145. His emphasis.

It is the lower level of interest rates, not the larger quantity of money, which exercises an expansionist influence . . . it is immaterial what changes in the quantity of money have to occur as part of the process of securing a particular desired behaviour of rates of interest.⁹⁰

Kahn is clearly advocating a monetary policy that controls the money supply for the purpose of setting the general level of rates, and a debt policy that controls the composition of debt for the purpose of setting the rate structure. But, presumably because of Kahn's oft-repeated assertion that the money supply is not important as an end in itself, the Committee seems to have been misled into believing that he was de-emphasizing the link between money and interest rates, and thus removing money from any central role in policy decisions, that he was putting all of his eggs into the debt-management basket, and that he was advocating the structure of rates as the "centre-piece" of monetary action. In any case, the Committee in its *Report* and in its questioning of witnesses seems obsessed with the idea of debt managers "doing something" about the structure of rates, while control of the money supply as a means of influencing the level of rates, or the price level, receives only passing mention, and then usually for the purpose of ridiculing the notion.

Presumably, then, debt management should operate in this way: the managers set the general level of interest rates by forcibly playing on the expectations and confidence of the public; they then go to work on the structure of rates by manipulating the composition of debt. In pursuit of this policy, debt managers may be called upon at times to change the level and structure of interest rates markedly. Also, in view of the record of the 1950's, rates would have to fluctuate very widely to affect spending directly. Are such fluctuations desirable? In answering this, the Committee states that if the wide fluctuations could be confined to the short-end of the rate structure the case for this policy would be fairly strong. But, since movements in short-term rates cannot much influence spending, either directly or indirectly, it is the long-term rates that must move. And there are three reasons why this policy should be rejected. First, while long-term rates can be raised quite high, the reverse policy of lowering them sharply during recessions would require a flood of liquidity into the economy that would play havoc with attempts at stability when the economic climate reversed itself;⁹¹ and this liquidity would at the same time inspire a speculative swing against sterling. Second, widely fluctuating long-term rates would gravely weaken the foundations of financial institutions, by involving them, when rates are rising sharply, in capital losses on large blocks of securities.⁹² Third, changes in long-term rates, no matter how marked, probably have little direct impact on spending in the short run.

⁹⁰ M-3, 144. See also Kahn's testimony, MinE, 10983-87.

⁹¹ This statement recognizes the connection between liquidity (lending) and the level of interest rates, and it is therefore in opposition to what I have called the dominant view of the *Report* on this matter.

⁹² R, 487-91. N. Kaldor, in his memorandum, advanced an additional argument against highly fluctuating rates: namely, that the average rate over time would then be higher, due to risk considerations, which would push the economy more toward consumption and away from investment, and so slow down its growth rate. He also felt that saving, under these conditions, would be allocated less efficiently to investment alternatives. M-3, 148.

Inasmuch as short-run movements in interest rates, in the Committee's opinion, can get at spending only through imposing capital losses on financial institutions' security holdings, and inasmuch as this policy is rejected partly because it would "gravely weaken" the foundations of these institutions, where do we go from here? Well, the Committee states, interest rates do have a direct effect on spending in the long run, and they do influence institutional lending in the long run, so they should be used carefully and slowly in a way to set the general long-term tone of the economy.⁹³

Our conclusions on rate of interest policy are therefore that, while there can be no reliance on this weapon as a major short-term stabilizer of demand, the authorities should think of rates of interest—and particularly long rates—as relevant to the domestic economic situation. The authorities should not aim at complete stability of interest rates, but should take a view as to what the long-term economic situation demands and be prepared by all the means in their power to influence markets in the required direction.⁹⁴

To fill the policy gap in the short run, the Committee would like to see more use made of fiscal policy, as a short-run stabilizer (after some of its present defects are corrected), leaving monetary policy to act on the longer-term situation.⁹⁵ Thus, the standard policy prescription is turned on its head; it now reads: Use fiscal policy to iron out short-run fluctuations and monetary policy to guide the economy for the longer period.⁹⁶

So much for poor old monetary policy in ordinary times. However, during an emergency, when the danger is that of "headlong inflation," monetary measures should be used vigorously. This does not mean restriction of the money supply—for that is not important—but rather striking "more directly and rapidly at the liquidity of spenders."⁹⁷ Now, since most of us are immediately inclined to identify the money supply as a major part of liquidity, these pronouncements seem contradictory, until we remember that, for the Committee, liquidity often refers only to lending (or borrowing). So it comes as no surprise that the monetary measures the Committee has in mind in such an emergency are direct controls of capital issues, bank advances, and consumer credit—and perhaps the control of lending by nonbank intermediaries.⁹⁸

The role envisaged for monetary policy, in ordinary times, as modest as it may be, nevertheless requires that the authorities take positive action from time to time to influence the level and structure of interest rates. The Committee notes that during most of the 1950's the monetary authorities concentrated on short-term rates and were unwilling to admit "there was much

⁹³ R, 492-97.

⁹⁴ R, 498.

⁹⁵ R, 516-17. See also the Committee's discussion of this with the Treasury MinE, 13311-22.

⁹⁶ Along these lines, for what is probably the most extreme statement ever made on the relative merits of fiscal and monetary policies, see the memorandum by I. M. D. Little, R. R. Neild, and C. R. Ross, M-3, 159-67.

⁹⁷ R, 524.

⁹⁸ R, 520-29.

scope for the exercise of official influence over long-rates"; it recommends that the authorities give a more positive lead in the long end of the market.⁹⁹

This seemingly innocent recommendation was actually the upshot of a running debate between representatives of the Bank of England and the Committee that was one of the hottest of the hearings, rivaling even those of recent years in this country between the Federal Reserve and its Nemesis in Congress. The Bank authorities defended, with the greatest of moral fervor, the doctrine that they do not, and most certainly should not, influence in any direct way the level of long-term rates; and, moreover, at times they argued that they should not exert such influence, except on a very temporary basis, in any indirect way, such as by operating at the short-end of the market. They simply "follow the market," permitting the natural forces of supply and demand to determine the level of rates. Spokesmen of the Bank were never ready to concede that operations at the short-end were meant to influence the entire rate structure, though they did think that at times their operations probably did have some impact on long-term rates, undesirable and annoying as that may have been.¹⁰⁰

At one stage, Bank officials were asked why, in view of strong inflationary pressures, they did not act more energetically in getting long-term rates higher. The answer and subsequent exchange follow:

I want it to be quite clear that we do not set out to intervene in a trend.

Even though it was reasonably plain that the reason for the action was to establish a long-term rate of interest that was needed to the general economic health of the country? — Yes, even doing one's best to make it plain that "it hurts me more than it hurts you."¹⁰¹

Bank officials felt that their operations had not in fact pushed up long rates, that these rates had increased because of the public's fear of inflation,¹⁰² and that it was really the impersonal forces of supply and demand that had "a pretty big influence" on long-term rates.¹⁰³ Under questioning, however, it was admitted that the Bank could affect both supply and demand, but the officials made it clear that under no conditions would they wish to do this in any direct way, because such a policy would lead the public to cry "stinking fish" at government securities and would damage Her Majesty's Government's credit. But the officials were hard put to defend this position:

When you say that "it would greatly damage the Government's credit," what effect have you in mind there? — Put briefly, that if we have just issued a new stock at, let us say, 100, and we then proceed actively to sell it down ourselves to 95, we have largely by our own direct actions on that security forced a book loss of five points on the people who took the security at 100.

⁹⁹ R, 428.

¹⁰⁰ This is noted in R, 428, 552, 583. The Committee states that "we were sometimes assured that the bill rate has practically no connection with long rates."

¹⁰¹ MinE, 1884, 1887. See also MinE, 1792, 1796, 1804, 1845.

¹⁰² MinE, 1849-51.

¹⁰³ MinE, 1870.

It operates in a similar way in your sales of Treasury Bills from day to day; the Bank Rate will very definitely affect the Treasury Bill rate. You impose a loss on all holders of Treasury Bills? — I accept that, but that is the recognized play of the money market.

Why is the one transaction objectionable and the other not? — The Treasury Bill and short bond market is a technical market, where these things are clearly understood . . . [The institutional market is different]; if, shortly after you bought [a security] at 100, you found someone else buying it at 92 or 93 your confidence would be very seriously reduced. We cannot afford to impair people's confidence. . . .¹⁰⁴

VII. *The Bank's Status, Sterling, and Statistics*

In the foregoing, I have neglected several areas that received much attention in the *Minutes of Evidence* and that were dealt with at some length in the *Report*. There is space only to skip across the high points of three of these areas.

A. *The Status of the Bank of England*

The Committee rejects the theory that the Bank of England should be completely independent of political influence.¹⁰⁵ It believes that the function of the Bank "is to act as a highly skilled executant in the monetary field of the current economic policy of the central Government," and that "the policies to be pursued by the central bank must be from first to last in harmony with those avowed and defended by Ministers of the Crown responsible to Parliament."¹⁰⁶ The Bank should generate advice, views, and proposals of its own, but the will of the government should be paramount.¹⁰⁷

The Committee's opinions on the status of a central bank were not shared by Riefler, whose comments on this subject prompted the committee to ask:

This means, leaving Congress on one side, that, as between the Administration and the Federal Reserve on an issue of policy, the initiative and the last word rest with the Federal Reserve? Suppose that the Administration want a certain economic climate to be created, they can merely discuss their desires with the Federal Reserve? — The only question that comes up relates to the reading of the business and credit situation. Sometimes there are differences of view. The question then is whose judgment is going to prevail. Our position is that obviously the people who are more specialized in reading the business and credit situation have to make the judgment. It would make no sense to have us try to make a judgment on what the business and credit demand is, and then

¹⁰⁴ MinE, 1849, 1855-60. For a fuller discussion of these questions, see MinE, 1762-63, 1792-1805, 1841-98. The Committee's views are in R, 551, 575-76. The Treasury's position was much the same as the Bank's; see MinE, 2387-98, 2953-72, 2799-2995.

Subsequently, the Bank did use open-market operations to influence long-term rates. For a justification of this and a slight modification of its initial position, see MinE, 11919, 12000-01, 12008-14, 13416, 13453. This change is discussed by the Committee in R, 341, 428, 553.

¹⁰⁵ R, 768.

¹⁰⁶ R, 767, 769.

¹⁰⁷ R, 761-62. See also MinE, 256-58.

have somebody else super-imposing another judgment. It ought to be made by whoever is most capable of making it.¹⁰⁸

Riefler returned to the point later:

If the System is in this way by statute independent of the executive, and therefore from direct political pressure, and also independent of private interest, is the argument ever heard that, by being so insulated, they are in an ivory tower and out of touch with what is going on in the length and breadth of the United States? — Yes, Mr. Elliott Bell does say that in *Businessweek* [sic]; anyone wishing to criticize the system is very likely to say that. I do not think it is taken very seriously. The system, through the directors of the Banks and the branches, and through its contacts, is intimately bound into the structure of the economy. In a sense it is the most important recorder of the state of the economy of the country; often the same people who are opposed to some action taken and who raise the cry of the "ivory tower" rush to us to corroborate any judgment they have on economic questions.¹⁰⁹

The *Report* recommends the formation of a Standing Committee which would be advisory in character; it proposes that changes in the Bank Rate be made in the name of the Chancellor; and that part-time directors of the Bank be retained, a subject that had been discussed at length before the Parker Tribunal.¹¹⁰

B. *International Aspects*

The Committee says nothing in this portion of the *Report* that will upset foreign confidence, steps gingerly around many touchy issues, and generally approves of official postwar policy toward sterling.

It has little confidence in the ability of changes in short-term rates to cor-

¹⁰⁸ MinE, 9407.

¹⁰⁹ MinE, 9454. Compare Riefler's statement with that of the Governor of the Bank of England, who said in part: "I have no doubt that in modern conditions it is proper that Government should have the final word on policy and that the central bank should not be free to pursue a completely independent line." MinE, 12813.

¹¹⁰ R, 771, 773, 778-87; MinE, 262-63, 269. A similar body has been suggested here many times in recent years, but the proposal has been attacked partly because it would jeopardize the independence of the Federal Reserve. Four years ago, before the Joint Economic Committee, Elliott Bell submitted a remedy:

If . . . it is felt that the Federal Reserve Board is so sensitive that contact with the President would corrupt it, then I suggest there might usefully be formed a National Economic Council without regular representation by the Federal Reserve Board. In this event, the Fed might be invited to send an observer with the express understanding that he could sit near an open door ready to fly to the sanctuary of Constitution Avenue if he felt the danger at any point of political contamination.

See *Hearings before the Subcommittee on Economic Stabilization of the Joint Economic Committee*, on December 10-11, 1956, p. 7.

The Committee received and heard an unusually large amount of evidence concerning the status of the Bank. For the Bank's views, see M-1, 5-9; MinE, 249-86, 752-60, 12813-900. For opinions of part-time directors of the Bank, see M-1, 44-45; MinE, 12066-188. Riefler's statements are found in MinE, 9395-9407, 9422-36, 9452-54. The views of former Chancellors and others are found in M-3, 47-48, 70-71, 207-11, 248-49; MinE, 11250-99, 12301-640.

rect balance-of-payments difficulties. "We have had little evidence of actual movements of funds in response to changes in short-term rates . . .," though it adds that there is some indication that a rise in long-term rates has induced purchases of long-term securities in London. However, "the fact that changes in rates have had only a limited effect on the movements of funds in a period when sterling was weak does not imply that they will be correspondingly ineffective if sterling is strong." Rather than relying on movements of interest rates, it perhaps would be better at times to support the forward rate of exchange, though many weaknesses of this proposal are noted.¹¹¹ The Committee favors a fixed parity for sterling, rejecting freely fluctuating exchange rates, which were advocated by James E. Meade, and devaluation as a policy, "though as a way of escape it cannot be excluded."¹¹²

Despite the recent improvement in reserves, the Committee states that they are still far from adequate; but it is noted that the U.K.'s problem in this respect is really part of a world-wide "shortage" of international reserves relative to levels of trade.¹¹³ This raises the question of whether there should be a substantial increase in the world price of gold, a proposal that is rejected by the Committee as not "immediately necessary [nor] the most helpful approach to the problem of international liquidity." Moreover, an increase in the price of gold would alter the existing distribution of reserves "very much in favor of the countries that are most amply provided."¹¹⁴

It is felt that this problem can best be attacked by utilizing existing international machinery, such as the International Monetary Fund, but the Committee believes that in its present form the IMF has many defects. Several suggestions are offered to remedy these defects, including relaxing the requirements on drawing rights, but the Committee seems more enthusiastic in suggesting that the IMF might be turned into an international central bank, with its own unit of account, along the lines originally suggested by Keynes.¹¹⁵

The Committee approves the steps taken toward convertibility and non-discrimination in trade. It is hopeful about the international position of the United Kingdom, and believes that the dollar problem "is likely to be more intermittent and less intractable than is sometimes supposed, and that it has already changed in character, and is likely to continue to do so."¹¹⁶ It is reported that the Treasury is looking ahead to a larger current account surplus in the early 1960's.¹¹⁷ This is all to the good, because it is necessary "to maintain a balance of payments sufficiently favorable to leave a margin for loans and other investments. This margin must be correspondingly widened to allow of grants in aid of colonial development." Since, however, it is better for the United Kingdom to use its surplus more to build up reserves than long-term

¹¹¹ R, 695-702, 703-07. See the Treasury's spirited objection to the support of the forward rate in M-1, 121-22.

¹¹² R, 716, 719-22, 728.

¹¹³ R, 670-71; MinE, 2531.

¹¹⁴ R, 672-74.

¹¹⁵ R, 678.

¹¹⁶ R, 684.

¹¹⁷ R, 630, 734.

assets, other sterling countries should be encouraged to borrow more in other areas of the world.¹¹⁸ But the United Kingdom should remain a major source of capital to the Commonwealth even if it has to borrow abroad for this purpose.¹¹⁹

C. *Financial Statistics*

Although many trade associations, the Bank of England, the Treasury, and others submitted economic data to the Committee that had not previously been published,¹²⁰ the Committee was frequently handicapped by a lack of information on matters into which it was inquiring. The Committee discussed this problem in its *Report* at some length and carefully pointed out the means of correcting it.¹²¹ As anyone who is at all familiar with the paucity of British economic data might guess, the *Report* called for more reporting and publishing of statistical series in almost every field of conceivable interest to the authorities and the informed public. This required a certain amount of courage on the Committee's part, considering what it went through in its examination of some witnesses. To give the reader some idea of this, I cannot do better than follow through the trials and tribulations of the Committee in its attempt to gain one small piece of information—that on the distribution of investments held by commercial banks. The Committee, early in the game, confronted the Bank of England:

What information does the Bank of England now receive from the clearing banks about their investments? — I should like to take that one away, Mr. Chairman; you see, the banks are our customers.¹²²

Rebuffed there, the Committee turned next to the Treasury, in this way:

Then is it the case that the holdings of the clearing banks, in the categories, say, nought to five year bonds, five to ten, ten to fifteen, or whatever the broad categories may be, are not known in aggregate either to the Bank of England or to the Treasury? — There is no regular return to my knowledge; but it is not true that we do not know.¹²³

Since the Treasury knew but was not telling, the Committee raised the question with the London clearing bankers:

It is suggested that the aggregate figure of the distribution of the clearing banks' investments may be an important thing from the point of view

¹¹⁸ MinE, 2492-2507, and p. 956.

¹¹⁹ R, 741-47. The Bank's views on the international aspects in M-1, 13-17, 34-35; MinE, 833-947, 948-71; the Treasury's in M-1, 105 112-22; MinE, 2483-2615, 3211-22, 9695-9734. Economists addressed themselves to these issues in M-3, 71-76, 132-36, 243.

¹²⁰ The Bank presented a memorandum on the current sources of banking statistics (M-1, 66-70); the Central Statistical Office prepared data on sources of financial and economic statistics relating to the monetary system and on assets of financial institutions (M-1, 129-62); The National Institute of Economic and Social Research presented a comprehensive memorandum on financial and economic statistics (M-3, 3-27); and the many data presented by trade associations are found throughout the evidence.

¹²¹ The recommendations are found principally in Ch. 10 of the *Report*; but see also R, 366-67, 580, 582, 629.

¹²² MinE, 167.

¹²³ MinE, 2841.

of an informed study by the public. The question is: could that have any adverse effect upon the interests of the banks, as banks? — [Mr. Robarts] I should like to consult Mr. Tuke on this point. [Mr. Tuke] I have been brought up in an atmosphere of not disclosing our affairs or, so far as possible, any of these things; and (though perhaps I have no right to say this) I should want to be convinced that it really was in the interests of the country that these things should be disclosed. If I was told that there was, a sufficient reason, other than what I would call curiosity to know about these things, then it might outweigh our disinclination to disclose figures. But we have this natural disinclination.

A great deal of our discussion this morning had to do with the liquidity of the banks. Would you not agree that in that connection it was of interest to know something about this? — Mr. Robarts has told you the sort of way in which we try to divide these investments up in pretty well equal tenths. I would support that. That is our policy, always, in practice, with a bias at the lower end; there is more under five years than over.

Would there be any more significance in knowing the proportion of your investments under five years than in knowing the money at call and short notice, which you do disclose? — If we give you the figure of stocks under five years, someone will come along and ask for more information; they will want to know the figure for stocks under two years.¹²⁴

Undaunted, the Committee later tried out the Scottish bankers on this delicate issue:

Do you not think that it would be useful if figures for the distribution of your investments by maturities were available to the public? — This question rather takes me by surprise. If . . . it appeared in a certain month that the Scottish banks were going longer in investments, what reaction would the public have to that announcement?

Let us for the moment leave the public out. Suppose that you were a monetary authority and concerned to operate a monetary policy on behalf of the Government; would not the availability of the figures on this question be important to you? — It might quite well be important, and the Scottish banks would probably be willing to give this information, if it were known that it was not for public dissemination.¹²⁵

Finally, about a year later, the Committee raised the question again with the London clearing bankers, with the following disastrous results:

Subheading (ii) also relates to the division of your figures of holdings of gilt-edged securities between under five year bonds and over five year bonds. Would you see objection to disclosure of that to the public at some interval? — I would see the very greatest objection.

You observed this morning that I was in some doubt whether any substantial change had taken place since before the war in this distribution. I imagine that, if we are in doubt, the general public would be still more in doubt? — The general public would not think anything about it.

¹²⁴ MinE, 3862, 3864-65.

¹²⁵ MinE, 5042-43.

I was referring to the informed public. There have been discussions of this in the financial Press? — Not altogether well informed, if I may say so.

Would it not be better if they were well informed? — We are not at all keen on the idea of publishing them to all and sundry.

What is the reason for this? — Everything can be picked up by certain financial journalists who will fasten on anything which could in any way be used to the detriment of the banks' wellbeing.

You are giving more rope to the people who make uninformed comments. Is that what you really want? — *We do not want the public to discuss our affairs.*¹²⁶

That should be enough to indicate the nature of the barriers that faced the Committee, but I cannot refrain from reporting one more exchange. The Committee suggested to the clearing bankers that it would be of interest to have quarterly figures analyzing bank deposits as to holder. The bankers' answer to this was:

Professor Cairncross used the word "interest"; if I may say so without disrespect, if it is merely a question of interesting academic economists I am not very anxious to spend a great deal of money on that.

Do you not think that academic economists have contributed to an understanding of monetary policy? — They all quarrel amongst themselves, so that one does not know what to believe.¹²⁷

Finally, for anyone interested in further pursuit of this matter of statistics, he could do no better than follow the Committee in its quest for the marketable security holdings of the Exchange Equalization Account in 1939, a real thriller with a surprise ending.¹²⁸

VIII. Concluding Remarks

An English visitor here, when asked what he thought of the Radcliffe *Report*, replied that it was "woolly." He undoubtedly meant that it was confused and hazy. But there is a colloquial meaning of the word, which is "attended with unusual excitement," as a woolly melodrama. The *Report* is certainly woolly in both senses.

It is exciting because the Committee's undertaking was hazardous from the very beginning, being nothing less than the development of a general theory of finance that would explain the impact of financial variables on the postwar British economy. And it must be said, considering the string of celebrated witnesses that paraded before it, that the Committee received surprisingly little help in its main task. In view of all this, not to mention time limitations, the *Report* it turned out, for all its deficiencies, is remarkable. The macrocosmic view the *Report* gives of the world of finance will leave many monetary theorists and policymakers uneasy in their self-imposed exile to a small corner

¹²⁶ MinE, 13247-52. My emphasis.

¹²⁷ MinE, 13234-35.

¹²⁸ MinE, 2848-55, 3223.

of this world. If it does nothing more than open up this larger world to them, it will have served a worthy purpose.

But at the same time, in its analysis of this wide, wide world there seems to be confusion everywhere—in the role of the money supply, in the concept of liquidity, in the analysis of nonbank intermediaries, in the discussion of interest rate determination, in the exalted role assigned to debt managers, and so on. Even so, though this is the *Report* at its worst, it can still be judged an honorable failure. “Honorable” because, as Zarathustra reminded the dying rope-dancer, there is nothing contemptible in making danger one’s calling, nor in perishing in that calling. Nothing contemptible; but still, in the way it all ended, something distinctly sad.

COMMUNICATIONS

The Public Debt: A Burden on Future Generations?

"Personally, I do not feel that any amount can be properly called a surplus as long as the nation is in debt. I prefer to think of such an item as a reduction on our children's inherited mortgage."

—President Eisenhower, *State of the Union Message*, January 7, 1960.

Two things are certain. The first is that, whatever else this quotation from President Eisenhower's State of the Union Message may imply, the President appears convinced that the costs of debt-financed public projects can be passed on to future generations. The second is that the popular economics textbooks of our day are nearly unanimous in their rejection of this "naive" view of the public debt. The purpose of this brief note is to suggest that in this instance it is the President who is—in at least one highly important sense—right.¹

The basic question at issue seems simple indeed: Can the "real burden" of a public project financed by a privately held internal debt be shifted from one generation to another? The usual economics textbook answer to this question is that the burden can *not* be shifted to future generations because government spending must drain real resources from the community at the time the government project is undertaken (assuming full employment of resources) regardless of whether the project is financed by borrowing, taxes, or money creation. As Samuelson puts it: "To fight a war now, we must hurl present-day munitions at the enemy; not dollar bills, and not future goods and services."²

What is wrong with this by-now-standard argument? Absolutely nothing, if the real burden of the debt is defined as the total amount of private consump-

¹ J. M. Buchanan [1] is one of the few contemporary economists to argue in favor of the proposition that the real burden of a public debt *can* be shifted to future generations. It was Buchanan's stimulating book that started the train of thought that has resulted in the argument contained in this paper. The reason for the present paper is that while Buchanan has arrived at essentially the same conclusion, he has apparently not succeeded in convincing very many people that he is right—at any rate, he has not convinced several reviewers of his book (see, for example, the reviews by Rolph [5], Lerner [4], and Hansen [2]). Perhaps the reason these reviewers have not accepted Buchanan's conclusion on this point is that Buchanan: (1) does not always define "real burden" in a sufficiently clear manner; (2) defines "generation" in such a manner that the same person can be considered a member of many different generations [1, pp. 33-34]; and (3) relies on what Rolph [5, p. 184] has called a "proof by indirection." We have tried to avoid these pitfalls.

² [6, p. 351]. Among the widely used elementary texts, C. L. Harriss' book [3, pp. 689-97] seems to come the closest to accepting the line of argument presented here. However, Harriss' exposition is badly impaired by an unclear distinction between "real costs" and "money costs." All writers seem to agree that the so-called "transfer payments" necessitated by a public debt involve real burdens in the sense that taxes used to meet interest payments may impair incentives to work and save. Neither this aspect of the debt problem nor the relationship between the public debt and economic stabilization are discussed in this paper.

tion goods given up by the community *at the moment of time the borrowed funds are spent*. Under this definition of real burden, the cost of the public project simply must be borne by the generations alive at the time the borrowing occurs.

There is, however, another definition of real burden which permits, under certain circumstances, present generations to shift the burden to future generations. And this definition, we submit, is a more accurate representation of the everyday notion of burden and is a more sensible concept for deciding if the real cost of a certain project can or can not be postponed to future generations. Let us define the real burden of a public debt to a generation as the total consumption of private goods foregone *during the lifetime* of that generation as a consequence of government borrowing and attendant public spending. (For the moment, we are not taking into account the benefit that may result from the public expenditure, and so we are talking about a "gross burden.") Our preference for the lifetime of a generation as the unit of account is based on the proposition that people can and do forego consumption at a moment of time in order to be able to consume more later, and that to use the amount of consumption foregone at any one moment of time as some sort of index of the over-all sacrifices made by a generation is misleading.

Let us now consider the following situation. Assume a full-employment economy. Assume further that there is within the society an identifiable "generation" of people, all of whom are, let us say, 21 years old. Suppose that at a given moment of time the government sells bonds to the private sector of the economy in order to finance public project X^3 and that all of these bonds are voluntarily purchased by the group of 21-year-olds, whom we shall refer to as Generation I.

To determine the allocation of the burden of public project X between generations, consider a point of time 44 years later when all members of Generation I are 65 years old and the rest of the community is made up of a Generation II, whose members are all 21 years old. Suppose that at this moment of time all the members of Generation I who own the still outstanding government bonds sell these securities to members of Generation II and use the proceeds for the purchase of consumer goods during retirement.

In this case it is clear that the lifetime consumption of the members of Generation I has not been reduced even though the total subtraction from the production of private goods due to the carrying out of public project X took place during their lifetime. The reason is simply that the saving represented by Generation I's original purchase of the bonds has been matched by the dissaving resulting from the later sale of the bonds to Generation II and the subsequent spending of the proceeds. Conclusion: Generation I has not assumed any of the burden entailed in financing public project X by the issuance of government bonds. (For the time being, we ignore the interest charges on the debt.)

Let us now examine the situation of Generation II. If the government makes

³ At this juncture, the precise characteristics of the government project are best left unspecified. The relevance of the particular type of government project undertaken will be considered shortly.

no effort to retire the debt during the lifetime of Generation II, and if Generation II sells its bonds to Generation III, then Generation II also escapes the burden of paying for the public project, and so on. To make a potentially long story short, suppose, however, that during the lifetime of Generation II the government decides to retire the debt by levying a general tax in excess of current government spending and using the surplus to buy up the bonds that are now held by members of Generation II. The inevitable outcome of this decision is a reduction in the lifetime consumption of Generation II. The taxpayers of Generation II forego consumption in order to retire the debt and yet the bondholders of Generation II do not experience any net lifetime increase in their claims on consumption goods since they are simply reimbursed for the consumption foregone at the time when they (Generation II) bought the bonds from Generation I. Conclusion: the burden of public project *X* rests squarely on Generation II, and not on Generation I.

The skeleton of our argument is now complete: While the resources consumed by a debt-financed public project must entail a contemporaneous reduction in private consumption, the issuance of government bonds permits the generations alive at the time the public project is undertaken to be compensated in the future for their initial sacrifice. Generation I merely makes a loan of its reduced consumption, and the real reduction of consumption is borne by the generation(s) alive at the time this loan is extinguished. Consequently, even though the real private consumption of the community as a whole need not be altered by the growth of the public debt, it is still possible for the distribution of the community's private consumption *between generations* to depend on whether or not public projects are debt-financed.

One other form in which the general argument that no burden can be passed on to future generations often appears is the "we owe it to ourselves" or "assets equal liabilities" version:⁴ No burden can be passed on because corresponding to every asset in the form of a government bond outstanding there is an equal liability in the form of liability for taxes to meet the interest charges and to repay the principal of the debt. Since there are always these two offsetting sides to the debt instrument, the argument proceeds, future generations can not be handed any burden since when our taxpayer-children inherit the tax liability our bond-holder children will acquire an equal asset.

The difficulty with this argument is simply that the asset and liability sides of the public debt are "passed on" in significantly different ways. In so far as the government bonds are acquired by Generation II by purchase rather than bequest, the recipients of the bonds have only received a *quid pro quo*. On the other hand, the members of Generation II who are handed the tax liability are not reimbursed for accepting this liability. From the vantage point of Generation I, the bondholders in this generation received claims on consumption in exchange for the asset (bonds) which they sold to Generation II; at the same time, the members of Generation I as liability-holders passed on their tax liability to Generation II by the simple expedient of dying, and thus did not have to give up consumption goods to get rid of the liability.

⁴For a fuller exposition of this line of argument, with references to the literature, see [1, pp. 4-14, and *passim*].

Consequently, unless Generation II can in turn pass its assets on to Generation III by sale while at the same time passing on its liability without making a compensating payment, the burden of the debt will be borne by Generation II.

We come now to the question: How about interest payments? We shall argue that the interest payments on the debt represent some burden on each and every generation that must pay taxes to make such payments.

To show why this is so it is necessary to reconsider the meaning of our definition of real burden—namely, “the total consumption of private goods foregone during the lifetime of a generation.” Thus far we have implicitly assigned all amounts of consumption enjoyed during the generation’s lifetime equal weights in arriving at total lifetime consumption, and have disregarded entirely the stage in a generation’s lifetime at which various amounts of consumption were enjoyed. Consequently, we were able to argue that a generation which gave up a certain amount of consumption early in life to buy bonds and then was able, by selling the bonds later in life, to enjoy the same amount of consumption during retirement years had avoided all of the burden involved in the debt financing of project *X*. The difficulty with this treatment is obvious. So long as people have a positive rate of time preference (that is, prefer present consumption to future consumption), they will feel that they have made a sacrifice if they give up a certain amount of consumption in their youth and then receive back exactly the same amount of consumption in their old age.

If we assume that the interest rate on the government bonds approximates the generation’s rate of time preference, then the interest payments on the national debt serve to compensate the owners of the debt for their willingness to forego consumption early in life, and thus (along with the recapture of the principal late in life) serve to make the discounted value of the lifetime consumption of the bondholders the same as it would have been if project *X* had never been contemplated.

Turning now to the tax side of the interest transaction, it is clear that the tax payments needed to make interest payments represent a real reduction in the lifetime consumption of the people paying the taxes. Furthermore, since any given year’s debt service is paid out of approximately contemporaneous tax payments, the same generation that receives the interest payments will be making a large part of the tax payments. The inescapable conclusion is that while the interest payments (along with the repayment of the principal) do not increase the discounted lifetime consumption of a generation, the tax payments do decrease lifetime consumption. Consequently, the discounted lifetime consumption of the generation is, on balance, reduced by the existence of debt service. This burden represents the real loss of welfare incurred by the generation as a consequence of the fact that it postponed its consumption but did not—because it received in interest payments only what it paid in taxes—receive any compensation for this distortion of its preferred consumption pattern.

The reason that in our discussion of interest payments we have spoken of “a” generation or “the” generation is that this burden (measured now in

terms of the reduction in the discounted value of the generation's lifetime consumption) is borne, of course, by each generation that pays a service charge on the debt. Consequently, even if the principal value of the debt is continually passed on, each generation bears a burden in the form of an uncompensated distortion of its preferred pattern of consumption.

So far we have avoided consideration of the type of government project financed by the initial borrowing. Actually, whether the government funds are spent wisely or foolishly is largely irrelevant to the question at issue here; for we are concerned solely with the allocation of the real *cost* of debt-financed government spending between generations and not with the allocation of the benefits of the government spending over time. Consequently, our conclusion that it is possible to shift at least a part of the cost to future generations does not imply that the absolute well-being of the future generations has been worsened by the combined borrowing and spending operation. If the borrowed funds were spent on a project whose benefit stream extends far into the future (for example, on fighting a "war-to-end-wars"), then the generation that assumes the main burden of the debt may still be much better off than if the debt had never been incurred. Our point is simply that the use of borrowing—as opposed to taxation or money creation—has improved the lot (measured in terms of lifetime consumption) of the first generation relative to the lot of succeeding generations.⁵

There is one final qualification to our argument. We have constructed a somewhat simplified case by assuming that all the bonds held by Generation I are sold to Generation II and that the proceeds are used entirely to increase consumption during the remaining years of Generation I's life. If, for example, all the members of Generation I were to will their bonds to Generation II, all real sacrifice of consumption would be borne by Generation I. Nevertheless, in spite of this simplification, the argument undoubtedly contains a large measure of relevance for the real situation. Purchasers of bonds, during a war for example, lose current consumption and receive marketable securities which surely are at least in part intended for conversion into spending on consumables in later years. The resulting claims upon consumer goods are realized at a time when they draw against the productivity of new members of the community who would otherwise enjoy a higher level of consumption. The existence of the marketable bonds undoubtedly makes possible at least some transfer of real income between generations.

Our conclusion that the real cost of debt-financed government spending can (at least in part) be transferred to future generations does not, of course, establish any *prima facie* case against deficit financing or in favor of the

⁵ There is a second, closely related reason for not tying the argument of this paper to a particular government expenditure, whether it be the construction of public schools or the giving of an enormous fireworks display. If, at the time public debt is issued, the government is spending money for many activities and financing these activities by taxes (and perhaps by money creation) as well as by borrowing, then it is hard to see how one can impute any specific project to any specific method of finance. The fact that we cannot solve this version of the imputation problem is irrelevant to the basic proposition that the cost of debt-financed government projects can be passed on to future generations, and thus cannot be used to disprove this proposition.

prompt retirement of the national debt. For one thing, to the extent that public projects undertaken today aid future generations, it may be fairer to let these future generations help pay the cost of these projects than to put the entire burden on present generations. Furthermore it is obvious that many considerations other than the location of the debt burden—such as the employment situation, the needs of the country for collective consumption, and the effect of taxes on incentives—are relevant in determining budget policy. However, at the present moment, there seems to be less danger that economists will forget the importance of these other considerations than that they will deny the possibility that the public debt can be used to shift a part of the real cost of public projects on to later generations.

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Capital Formation in Underdeveloped Countries

In the rapidly growing volume of literature on the problems and prospects for economic development, considerable attention is being devoted to the determinants of net capital formation or, more particularly, to the obstacles to and limitations upon capital formation in underdeveloped countries. This preoccupation is, without question, well deserved. One need not subscribe to a monocausal theory of development to argue that an increase in the percentage of annual output devoted to investment is an urgent and indispensable prerequisite to a long-term rise in real per capita incomes. Indeed, current attempts at what is now being called, rather pretentiously, "development programming," often consist exclusively of measures designed to raise the over-all rate of capital formation and to exert some centralized guidance over the allocation of investment resources.¹

A central question in a theory of development, then, is: Why are rates of

¹ "A programme of economic development is the expression of a simple idea, namely, the desirability of increasing and judiciously regulating capital investment, so that a stronger impetus and greater order may be given to the growth of the country" [11, p. 3].

capital formation as low as they apparently are in most underdeveloped countries? Economists can hardly be accused of having neglected this question. Indeed, it will be suggested that we now have too many explanations.

Explanations of capital deficiency are commonly organized in terms of two separate sets of forces: factors accounting for low saving propensities, on the one hand, and those responsible for weakness in the inducement to invest, on the other. However, such an approach leads to a preoccupation with certain phenomena and preference patterns which are commonly treated as independent causal agents, although there appear to be compelling reasons for regarding them as the superficial consequences of more fundamental factors.

It is proposed to show that certain preference and behavior patterns which are commonly observed in underdeveloped countries are in fact part of an elaborate adaptive mechanism geared to an economic environment characterized by a generally low marginal efficiency of capital and by a limited spectrum of profit opportunities. It will in fact be suggested that an adequate explanation of the general weakness of investment incentives and of the peculiar distribution of profit prospects in different sectors of underdeveloped economies would, *by itself*, account for many of the other apparent causes of low rates of capital formation. It will be demonstrated that the failure to "shake out the implications" of weak investment incentives has led not only to a double-counting of "causes" of low rates of investment but also to serious analytical errors and highly questionable judgments.

I. *Low Aggregate Savings*

The most obvious explanation for low rates of capital formation is the present poverty of underdeveloped countries. Their capacity to undertake productive investment appears to be sharply limited by the very low levels of per capita income combined with the frequently extravagant expenditure patterns of the rich and an interest, among virtually all income classes, in the acquisition of certain kinds of durable consumer goods.

What is not so apparent, however, is that individuals in underdeveloped countries may regard the acquisition of certain types of consumer goods as a form of personal saving and investment. This may well be the case for a variety of so-called luxury goods, especially such items as jewelry and precious ornaments which may constitute excellent stores of value in an environment characterized by political insecurity or inflationary pressures. Even where such conditions do not prevail, the absence of well-developed and reputable financial institutions may provide strong inducements for the acquisition of jewelry, gold or even foreign assets as relatively secure forms of personal saving. To the extent that this is so, our present measures of capital formation may significantly understate the capacity for saving and productive investment in underdeveloped economies. What appear to be low rates of saving in underdeveloped countries may be attributable, in part, to the fact that durable consumer goods are acquired and held not so much as the result of a real taste preference as because these goods are regarded as a form of personal saving in a backward economic environment [5, pp. 175-76].

A closely related and much more serious difficulty of a theoretical nature

arises over the common practice of explaining low rates of productive investment in terms of the failure of underdeveloped economies to generate a sufficient flow of domestic savings. The low rate of saving is in turn attributed to practices and preferences which squander the "potential economic surplus"—purchases of jewelry and gold, acquisition of foreign currencies and assets, lavish consumption expenditure patterns of the upper income classes, etc. In a certain sense such actions do help to explain the low rates of saving and productive investment, but only in the same sense as the statement "I prefer to stay home and watch television tonight" explains why I do *not* attend the local high school concert. It seems necessary to insist that statements of preference call for an examination and evaluation of the nature and comparative attractiveness of available alternatives. Precisely what is involved here is the unattractiveness of the alternatives, i.e., the unprofitable nature and/or high risk factor attaching to productive investment in underdeveloped countries. Low rates of saving and the absence of attractive investment opportunities are not independent of one another and cannot be treated as entirely separate and unrelated causes of capital deficiency. Rather, the actions referred to above may, to a large extent, be attributed to the "undeveloped" nature of the investment mechanism and to the complex of forces which weaken the inducement to invest [3, Ch. 2].

The problem discussed here cannot be dismissed as of a "Which came first—the chicken or egg?" nature—not, at least, without discarding much of Keynesian economics. That is to say, the same forces which account for the weak inducement to undertake productive investment may also account, in large measure, for the unproductive disposition of potential savings; and policies which have the effect of raising the marginal efficiency of capital schedule and therefore increasing the inducement to invest may reasonably be expected to induce, at the same time, an increased flow of (*ex ante* as well as *ex post*) domestic saving.

Implicit in much of the current discussion, however, is the classical assumption that the real limitations upon investment lie in the limited willingness or capacity of the public to save and that, therefore, an increased propensity to save is all that is needed to generate an increased flow of productive investment activity. Such analysis, however, often not only ignores the current weakness in the inducement to invest, but may also overlook the fact that an increased propensity to save, by its adverse effect upon the marginal efficiency of capital schedule, may weaken the inducement to invest even further [4, pp. 210-13].

Moreover, the priority which is usually attached to the need to increase savings as a precondition for raising the rate of capital formation really involves the concealed premise that an increased rate of capital formation ought not to be achieved at the expense of domestic inflation. Obviously, all measures or policies which result in a transfer of resources from the production of consumer goods to the production of investment goods necessarily generate an equivalent amount of *ex post* savings. This is not to ignore the many cogent reasons for avoiding the inflationary route, but merely to sug-

gest that the currently fashionable way of stating the problem may be seriously misleading.

II. *Methods of Holding Assets*

The preoccupation with the low level of domestic saving in underdeveloped areas as an independent determinant of low rates of capital formation creates a host of difficulties in attempting to move from the micro- to the macro-economic level of analysis. The general problem involved here is the macro-economic consequences of certain kinds of asset preferences on the part of individual savers. It is important that we distinguish between (a) factors which affect the aggregate volume of savings, and (b) the *forms* in which an individual chooses to hold his assets.

It is frequently held, e.g., that when an individual chooses to hold a net increase in his assets in the form of liquid balances (i.e., "hoarding") that this does not result in genuine saving in an aggregative sense, i.e., in the release of resources for nonconsumption purposes.

This argument appears to result partly from the failure to distinguish between the hoarding of cash balances and the "hoarding" of physical assets—jewelry, gold, or other durable commodities—the acquisition of which may involve a drain upon scarce domestic resources (or the supplies of foreign currencies, if imported).²

The hoarding of cash balances, even in the absence of appropriate financial institutions, will nevertheless result in the release of resources which might otherwise have been used for consumption purposes and therefore creates an added potential for capital formation.³ Obviously, such hoarding does not ensure that resources will be devoted to investment purposes. The deflationary consequences and the unfavorable impact upon interest rates and the inducement to invest may simply result in unemployment and an increase in the volume of unused resources. The point is that hoarding of cash balances in no way absorbs real resources but releases them from consumption uses, just as do other forms of saving. To the extent that savers decide to add to their stocks of liquid assets, the possibilities for raising the rate of capital formation are increased, and may be realized, for example, in a noninflationary fashion by deficit spending on the part of the government. Whether such possibilities *are* realized is not at issue. The essential point is that hoarding of cash balances, in itself, releases rather than absorbs real resources.

Although the hoarding of cash balances does not reduce the volume of sav-

² "Though the potential of capital formation in underdeveloped regions seems considerable when judged by the high share of output retained by the landowner, actual capital formation goes on at low rates. Oriental landlords have been known for generations for their high propensity towards 'unproductive' use of accumulated revenues. Even if the money is not spent in travelling abroad, it is invested largely in hoards of cash, jewellery and gold" [2, p. 38].

³ Of course it is possible that the absence of secure financial institutions discourages the saving habit and that individuals would save more readily if they had convenient access to a bank or other saving institution. Moreover, the absence of adequate financial institutions may also partly account for the habit of acquiring and holding goods as a liquid asset.

ings it may have an important consequence in limiting the availability of financial resources to certain potential borrowers and therefore influencing the composition of the capital formation which takes place. In this respect the hoarding of cash balances is one aspect of the more general problem of the deficient or inappropriate financial institutions of underdeveloped countries. In spite of the extremely serious nature of the latter problem, however, it needs to be kept conceptually distinct from the forces determining the volume, as opposed to the composition, of investment activity. Much confusion may result from the failure to maintain a clear distinction between the determinants of the volume of saving, on the one hand, and the factors influencing the supply and availability of financial resources, on the other.⁴

As has already been pointed out, hoarding frequently takes the form of the acquisition of physical commodities such as jewelry, gold, or other durable assets which are regarded as highly liquid and which possess the added advantage over cash balances that they may constitute useful hedges against inflation or political uncertainty. Such preferences obviously involve (in a closed economy) the use of domestic resources in their production, and may therefore be regarded as absorbing scarce productive agents. However, certain structural features and other peculiarities of underdeveloped economies may play an important role in determining the consequences of such preferences. An increased propensity to hoard commodities whose domestic elasticity of production is very low (as seems to be the case, for example, with gold mining in India) will result almost exclusively in a price effect and virtually no output effect; it will not significantly increase the quantity of the commodities hoarded, nor will it result in an increased use of domestic resources for such purposes. There may, then, be sharply defined limits to the extent to which a preference for hoarding physical goods results in an absorption of domestic resources. If, moreover, there are legal (and adequately enforceable) restrictions upon the importation of a commodity whose domestic elasticity of production is very low, the results of an increased preference for such commodities are formally similar to an increased preference for nonreproducible assets.⁵

III. *Preference for Investment in Land*

The classic example of nonreproducible assets is, of course, land. It appears to be a widely held belief that one of the reasons for the low rates of capital formation in underdeveloped countries is that upper-income groups employ their savings in the purchase of land. The following statement by Meier and Baldwin [7, pp. 307-8] is illustrative:⁶

. . . inequality in the distribution of income does not contribute as much to productive investment as might be expected . . . (because) the group

⁴ See [2, Ch. 10, esp. pp. 197-203] for an example of such confusion.

⁵ Where such commodities may be legally imported (or where they are successfully smuggled into the country in spite of import restrictions) the result is a drain upon domestic resources because of the increased exports ultimately resulting from such transactions. The effect is the same as if people were acquiring foreign currency or other foreign assets.

⁶ Cf. also [6, p. 227] [12, p. 22].

at the top of the income pyramid is composed of land-owners and traders who tend to invest in more land, real estate speculation, capital flights, or inventory accumulation rather than long-term industrial investments or public utilities.

Assuming that the purchase of land was financed out of current incomes, such individual savings must have resulted in the release of real resources for nonconsumption purposes. In so far as a desire for the acquisition of land generates a willingness to abstain from current consumption, it thereby increases the volume of *ex ante* saving, and may, indeed, be one of the most important motives for such saving in underdeveloped countries. If we assume that net saving is positive (i.e., that the savings of some individuals are not offset by the dissavings of others) and that total output does not decline, an equivalent amount of capital formation of some sort must be taking place within the economy.

A "preference for investment in land," whatever it may do to the price of land and the distribution of landholdings, does not, *in itself*, reduce the volume of capital formation by "absorbing" a part of current saving. Nevertheless, the belief that it does so seems to be firmly entrenched in the current literature. It is true, of course, that from the point of view of the individual investor, such a purchase represents a real alternative to the purchase (and creation) of a new capital good. However, the analysis ought not to be terminated, as it typically is, with the purchase of an existing, as opposed to a new, asset. The ultimate impact of the transaction will depend, among other things, upon how the seller chooses to dispose of the revenue received from the sale of his asset. If, as is at least conceivable, the seller of land intends to use the proceeds to acquire a new, reproducible capital good, the ultimate result will be an addition to the community's stock of such goods. It is only where the saving of the buyer is offset by an equivalent amount of dissaving on the part of the seller out of such revenue that real saving (and capital formation) are not increased. Although this is a possible consequence, there seem to be no compelling reasons for regarding it as a necessary consequence of land purchases.

Although a strong preference for land need not, as is commonly asserted, automatically reduce real saving and capital formation, it may have this as well as other important consequences, under certain special circumstances and via a more circuitous mechanism than has yet been specified. For example, in agricultural economies where land is a highly liquid asset, the ease of borrowing, on the part of landowners, by using their land as collateral, may induce a higher level of consumption expenditure and/or the growth of debt for strictly consumption purposes on the part of the owners of land [14, p. 85] [9, p. 69]. In such a case aggregate saving will be reduced, not because of the preference for land *per se*, but rather because such a preference makes possible an upward shift in the consumption expenditures of landowners.⁷

⁷ This, of course, may cut both ways. The high liquidity attaching to land may facilitate borrowing on the part of landowners for investment rather than consumption. All that is suggested here is a set of circumstances under which a strong attachment to land *may* reduce capital formation.

Some such process may, in the past, have played a significant role in reducing aggregate saving in peasant economies. If, to the generally low level of real incomes of small landowners is added the pressures resulting from population growth, occasional crop failure and a variety of other emergency (as well as ceremonial) occasions, the high liquidity uniquely attaching to land may have been the strategic factor making possible the growth of rural indebtedness for strictly consumption purposes.⁸

Moreover, if the rate of return on money-lending to an impoverished peasantry, who borrow on the strength of their small landholdings, is very high (and there is much evidence that this is so) we have an important factor accounting for the low level of investment in industrial enterprises in underdeveloped countries. In addition to such well-known deterrents as high degree of risk, limited markets, and absence of external economies attaching to industrial investment in underdeveloped countries, is the decisive consideration that rates of return for rural lending, even after discounting for risk, are extremely high, partly for institutional reasons and partly because of serious market imperfections.⁹

A further possible consequence of a strong preference for holding land is that it may reduce industrial investment by establishing high interest rates on borrowed funds which act as a deterrent to potential industrial entrepreneurs. A strong preference for land means that owners of wealth (potential lenders) can earn high rates of return by lending money for the purchase of land (i.e., buying mortgages). The combination of a small and inelastic supply of funds with a relatively high demand for their use serves to establish unusually high rates of interest. Therefore potential borrowers for industrial investment find that they must compete, in borrowing funds, with people whose preference for land is such that they are willing to pay extremely high rates of interest for such funds. In this respect a strong preference for land acts as a serious deterrent to industrial investment.

This last case is similar to the one which Keynes seems to have regarded as of major importance [4, p. 241]:

... it is conceivable that there have been occasions in history in which the desire to hold land has played the same role in keeping up the rate of interest at too high a level which money has played in recent times. ... The high rates of interest from mortgages on land, often exceeding the probable net yield from cultivating the land, have been a familiar feature of many agricultural economies ... in earlier social organizations where long-term bonds in the modern sense were non-existent, the competition of a high interest-rate on mortgages may well have had the same effect in retarding the growth of wealth from current investment in newly produced capital-assets, as high interest rates on long-term debts have had in more recent times.

⁸ It is also possible that the bidding up of the price of land may induce owners of land to increase their consumption expenditures or to dissave because of the increase in the market value of their asset.

⁹ Evidence on the profitability of rural lending is available from many sources. Among recent works [10, esp. Vol. II, Ch. 14] and [13] will be found very illuminating.

Thus, statements to the effect that a preference for land on the part of upper-income groups automatically reduces real saving and investment are, at worst, incorrect and, at best, highly elliptic. Where saving and investment are in fact reduced, it is due not to the preference for land by itself, but because of the presence of other important conditions which are often unspecified.

IV. *Inadequacy of Entrepreneurial Talents*

Much attention has been devoted, in recent years, to the apparent shortage of entrepreneurship in underdeveloped countries. It has become fashionable to deplore the absence of Schumpeterian entrepreneurs, and many economists have dwelt at length upon political, sociological and historical explanations for this shortage. Although such noneconomic explanations are not to be despised, it is difficult to avoid the conclusion that these people have been led into a faulty line of analysis by excessive preoccupation with their own definitions. Having defined entrepreneurs as those daring and imaginative individuals who undertake risky long-term industrial investments, the observed absence of such investment in underdeveloped countries is then attributed to the absence of entrepreneurs. A more plausible and purely economic explanation, or at least a more fruitful working hypothesis, is that most underdeveloped countries possess a reasonable number of people with entrepreneurial talents, but that these people behave in a subjectively rational fashion when they adopt short economic horizons and avoid long-term commitments in industry. Many of the activities referred to in this paper in fact help to account for the often-lamented "absence" of entrepreneurship in underdeveloped countries.

To quote from an interesting paper by Henry Aubrey:

The acquisition of real estate is often considered as evidence of sentimental attachment to land or of feudal patterns of unproductive investment. This explanation may be perfectly correct in some instances; in others, however, such "investment" may result from preferences well founded in the expectation of profit or, conversely, of security against a danger of depreciation that might face other forms of asset-holding [1, p. 398].

Rather than infer from the absence of long-term industrial investment the nonexistence of entrepreneurs, it seems more reasonable to infer that potential entrepreneurs, in evaluating alternative opportunities (including the purchase of existing assets), usually conclude that personal income maximization is not to be achieved in long-term industrial activities. In so doing, entrepreneurs are not necessarily irrational or behaving in response to considerations of social status, prestige, family honor, or even necessarily lacking in the "capitalist spirit." Considering the special circumstances of many underdeveloped countries, their decisions may constitute a perfectly rational evaluation of the structure of economic opportunities. The notion that there is an extreme scarcity of entrepreneurship in underdeveloped countries usually involves a failure to distinguish between the aggregate supply of entrepreneurship and its distribution. To employ the terminology recently adapted by Leibenstein from Von Neumann and Morgenstern, potential entrepreneurs are behaving in

un-Schumpeterian but subjectively rational fashion by selecting to play zero-sum rather than positive-sum games. If adherents of the entrepreneurial school of thought reply that such people are not, by *their* definition, entrepreneurs, they must weigh the serious consideration that such businessmen could conform to their definition only by deliberately failing to employ the calculus of profit maximization.

This question of the determinants of investment decisions in underdeveloped countries deserves more attention than it has so far received. It has, unfortunately, often been lost sight of in the current practice of indiscriminately lumping together a highly heterogeneous combination of factors which are presumed to be responsible for low rates of capital formation and unproductive investment patterns in low-income countries. The following statement by Gunnar Myrdal is illustrative:

It is . . . highly characteristic of all the underdeveloped countries that their business classes are bent upon earning quick profits not by promoting long-term real investment and production but by buying and selling, moneylending, and other *easier ways* of making money, which also often escape taxation. Profits tend to be invested in land, or else hoarded or transferred abroad, when they are not dissipated in a costly display of wealth and social status. There is a low propensity to save and to invest productively in new enterprises [8, pp. 202-3; italics added].

Myrdal's statement seems to imply that the investment pattern which he deplores is somehow connected with certain peculiar features of the "business classes" of underdeveloped countries. Yet, by his own assertion, they are merely pursuing the "easier ways of making money." Surely what is significant in this context is the existence of such opportunities and not the fact that businessmen take advantage of them. Is it at all probable that "westernized" entrepreneurs would behave differently when confronted with the same spectrum of alternatives?

V. Conclusions

A theory of economic development (which involves, of course, an analysis of why development does *not* take place as well as why it does) must include, above all, an explanation for the existing structure and distribution of market opportunities in underdeveloped countries. The fundamental question in an *economic* theory of economic development is: Why is the structure of market opportunities in underdeveloped countries of such a nature that it fails to provide the personal incentive for individuals to undertake those activities which appear to be conducive to economic growth? When this question has been adequately answered, many of the other pieces in our puzzle will fall easily into place as dependent, rather than independent, variables. The low amount of *ex post* savings, the widespread preference for the acquisition of assets which fail to enlarge productive capacity, the apparent malallocation of the small volume of resources which are devoted to investment purposes, the "scarcity" of entrepreneurship and the propensity to engage in short-term speculative ventures, all fall, to a substantial degree, within this category. They represent behavior patterns which may reasonably be expected either

to disappear or significantly decline in importance in the face of a drastic outward shift in the marginal efficiency of capital schedule.

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*The author is assistant professor of economics at the University of Pennsylvania. He is indebted to E. Smolensky for valuable discussions and criticisms. R. A. Easterlin, A. Freedman and D. Horlacher also offered helpful suggestions.

Wages and Interest—A Modern Dissection of Marxian Economic Models: Comment

Paul A. Samuelson's article, "Wages and Interest: A Modern Dissection of Marxian Economic Models," which appeared in this *Review* (December 1957), though enlightening and interesting, is open to criticism on the ground that it does not closely enough represent Marx's formulation of the issues treated.

1. *Samuelson's Conception of the Marxian Rate of Profit*. Samuelson writes (pp. 886-87):

Though prices and wages are constant . . . this does not mean that production is timeless or that intermediate products just now produced by labor and machines will exchange one for one against themselves when

"ripened" one period from now—or one for one against finished goods produced today from last period's inputs.

According to Marx, however, intermediate products will exchange one for one against *themselves* when ripened—if they contain the same quantity of labor; and if they so exchange among themselves, they must contain the same quantity of labor.¹ Time, in the sense of a process of "ripening," is nowhere found in Marxian price theory. Samuelson, however, suggests the presence, as a rule, of such a process in Marx's model. He writes (p. 887):

Only under special, and unrealistic, market assumptions can the competitive supply and demand relations be expected to ignore these timing relations. . . .

He develops Marx's rate of profit on the basis of the difference between ripened and unripened products (p. 887):

The fundamental factor relating unripened product today to ripened product one period from now is the market interest rate r (or what Ricardo and Marx would call the rate of profit, a pure percentage per period).

This "ripening" time, though present in Samuelson's Marxian model, does not appear in any guise in the model Marx endeavored to use.

2. *Profits and Wages.* Prefacing his discussion on the incompatibility of falling profits and falling real wages, Samuelson writes (p. 892):

. . . we must tackle directly the question of what accumulation will tend to do to r , basing ourselves on the actual behavior equations of competitive capitalism.

But the question arises: do Samuelson's "actual" behavior equations accurately represent the behavior equations employed by Marx?² The answer seems to be in the negative. The contrast between Marx's and Samuelson's equations stand out in the latter's formulation of a new theorem respecting technological change under perfect competition.

¹ Samuelson uses the following symbols: Y and K , output in consumer and capital goods industries; (a_1, b_1) (a_2, b_2) , labor and capital coefficients of production in capital and consumer goods industries; p_1, p_2 , cost of production of capital and consumer goods; L_1, L_2 , the quantity of labor employed in capital and consumer goods industries; w , the wage-rate; and r , the rate of profit. Equation (4) represents national income expressed in terms of labor; equation (5) represents the cost of production of consumer and capital goods, and equation (6), the explicit solutions for (5) in terms of a_1, b_1, a_2, b_2, r .

$$(4) \quad Y = \frac{1 - b_1}{a_2(1 - b_1) + a_1b_2} L \quad K = \frac{b_2}{a_2(1 - b_1) + a_1b_2} L$$

$$(5) \quad p_1 = (wa_1 + p_1b_1)(1 + r) \quad p_2 = (wa_2 + p_1b_2)(1 + r)$$

$$(6) \quad \frac{p_1}{w} = \frac{a_1(1 + r)}{1 - b_1(1 + r)} \quad \frac{p_2}{w} = \frac{a_2(1 + r)[-b_1(1 + r) + a_1(1 + r)b_2(1 + r)]}{1 - b_1(1 + r)}$$

² Marx considered the distinction he drew between the "labor-power" going into production and the "product of labor" resulting from the labor process as his greatest contribution to value theory. Exchange between the commodity labor-power and the commodity product of labor cannot be considered an exchange between *themselves*. They are fundamentally different commodities, composed of different factors of production. It is this difference that *allows* for surplus-value.

Samuelson states, "A technical improvement must be an improvement or it will not be introduced into a perfect-competition market economy . . ." (p. 894). By improvement he means a change resulting in either an increase in real wages or in the rate of profit. Thus, he suggests that a rational capitalist would not employ a technique that depresses r , or, that in a labor-dominated economy, labor would not select a technique that diminishes (w/p_2) real wages.

Marx's market economy, however, differs from Samuelson's. Marx assumes the presence of temporary monopoloid market structures. These structures are a prerequisite to innovation, the means through which greater profits (Marx's "surplus profits") may be earned. It is this possibility of realizing surplus profits (monopoly profits) that induces capitalists to innovate. These surplus profits, however, do not persist. Competition, in the Marxian model, compels capitalists to adopt available innovations, hence supplies increase and their price falls to a lower equilibrium (cost of production) level. As a result surplus profits are eliminated; only surplus value, attained by the "exploitation of labor-power," remains. Since Marx *assumes* that the employment of labor-power decreases with the introduction of new techniques, the amount of surplus value realized *may* be less with the new than with the older technique. So Marx, in Volume III of *Capital*, attempts to resolve the "paradox" or "riddle" of capitalist production, namely that the capitalist's quest for higher profit rates results in diminished rates of profit. Only by assuming temporary monopoly conditions in the market economy does Marx arrive at this seemingly paradoxical conclusion, and its corollary that capitalists do not revert to the old technique of production, even if it had formerly been more rewarding, because the now prevailing (lowered) price no longer permits the higher profits formerly associated with use of the old technique. The new position, though less favorable than the old, still is the best to be had.

Samuelson "proves" the incompatibility of falling profits and falling real wages through recourse to the new theorem. First, he sets down the reciprocal of his equation (6) to show that, with specified values for a_1 , a_2 and b_1 , b_2 (the labor and capital coefficients), a decline in the rate of profit (r) must produce an increase in real wages (w/p_2):

$$\frac{w}{p_2} = \frac{1 - b_1(1 + r)}{a_2(1 + r)[1 - b_1(1 + r)] + a_1(1 + r)b_2(1 + r)}$$

However, as Samuelson notes, Marx, by introducing technological change, makes necessary a relaxation of the assumption of fixed a_1 , a_2 and b_1 , b_2 . Yet, even with changing coefficients, Samuelson asserts, the incompatibility continues to prevail (p. 894):

Remember that in a perfectly competitive market it really doesn't matter who hires whom: so have labor hire "capital," paying the new market rate of interest $r' < r$; then labor could always use the old technology and paying less than r get better than the old real wage.

In Samuelson's example the old technology is assumed to yield the new rate r' . In the Marxian model, however, it is impossible to obtain r' given the

old technology, since r' is a sequel to recourse to the new technique. If both the new and old techniques are available, and uniform prices are assumed, the old technique is no longer profitable, since the price of the product has fallen with the introduction of the new technique. Therefore, in Samuelson's example, if the old technique is used labor is *not* better off; it may even be in a less favorable position. If one incorporates in the above equation the Marxian assumption that r is a function of the state of applied technology one may select certain values for the variables in the equation which permit falling rates of profit and falling real wages to be compatible. If r were determined by some exogenous factor (e.g., time or "ripening") Samuelson's assumption would be legitimate. Such assumptions, however, lie outside the framework of Marxian theory.

3. *Changing Factor Proportions and Prices.* Samuelson describes what may happen to factor prices and factor shares when their relative supplies (or rates of growth) change, given a situation in which the technical coefficients of production are rigidly fixed. He writes (p. 900):

In this case where capital goods have ceased growing as fast as labor, the rate of profit has risen to become all of the product. So bizarre a result came from the bizarre assumption of fixed coefficients.

On the following page he adds (p. 901):

The Marxian model with fixed coefficients presents some quite pathological features. For if the attempt to accumulate were to cause physical machines K to grow relative to fixed labor L , the machines would become redundant in supply and their rents would fall immediately to zero.

Marx would have objected to such statements of the matter. In the Marxian model, the growth rates of K and L depend upon relative factor prices. If K increases more rapidly than L (Samuelson's example), wages increase concurrently with the decline in the price of capital. The total effect of the price change, however, is not instantaneous or as great as Samuelson assumes. Price changes of L and K , in the Marxian system, induce qualitative changes in innovations, and these ultimately counteract the initial price changes. For example, suppose that, compatibly with the Marxian model, K becomes superfluous. Capitalists would obviously switch to more capital-using techniques of production. This "deepening" of capital would make K more scarce (increasing the price of K) and L relatively more abundant (decreasing the price of L , i.e., wages). Thus Marx explained the more rapid introduction of machinery into the American than into the English economy (*Capital*, Vol. I, pp. 429-30). Elsewhere he writes:

A momentary excess of the surplus-capital over the laboring population controlled by it would have a twofold effect. It would, on the one hand, mitigate the conditions, which decimate the offspring of the laboring class and would facilitate marriages among them, by raising wages. *This would tend to increase the laboring population.* On the other hand, it would employ the methods by which relative surplus-value is created (*introduc-*

tion and improvement of machinery) . . . (*Capital*, Vol. III, p. 256; my italics)

More fundamental to this discussion, however, is the question: Did Marx actually hold to a fixed coefficients of production model? Samuelson answers (pp. 906-7):

Perhaps Karl Marx really had such a technology in mind. Perhaps not . . . he speaks again and again of alternative techniques. While many of these clearly depict technological change in the production function rather than movement within one function, the fact that the old methods are still known along with the new shows that Marx and Ricardo definitely envisage the existence of more than one technique.

Marx was obscure in his discussion of this matter. In Marx's analysis of the rate of profit however, one finds "averaging" of organic compositions of capital in a specific sphere of production—thereby implying the presence of variable coefficients (*Capital*, Vol. III, pp. 182-85). So Samuelson's assumption of fixed coefficients of production is not always warranted.

4. *Concluding Remarks.* Samuelson's exposition attributes assumptions to Marx (e.g., the rate of profit is a function of time; the market economy is characterized by instantaneous price adjustments; foresight is perfect; and coefficients of production are fixed) which differ more or less from those to which Marx seemed to adhere. Samuelson's findings, though quite valid on the premises postulated, are not accurately descriptive of Marx's system.

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Wages and Interest—A Modern Dissection of Marxian Economic Models: Reply

I must assert agreement with the view that my paper on Marxian economic models did not do justice to Marx's own formulations of the issues treated. Nor was it ever intended to undertake such a task, whose extreme difficulty can be illustrated by the following topic.

What did Marx really think would happen to the real wage under capitalism with its alleged falling rate of profit? Some scholars (e.g., Jürgen Kuczynski) believe Marx proved that the real wage would fall and claim by empirical statistical observation to verify this law of immiserization of the proletariat; other scholars (such as Maurice Dobb, perhaps?) seem to argue that, with exceptions, this was Marx's view about competitive capitalism but that it came to be falsified by historical reality primarily because of the growth in political and economic powers of trade unions and the working class; still other scholars (notably Thomas Sowell in the March 1960 issue of this *Review*) argue with considerable persuasiveness that in his major economic writings, Marx did not conclude that the real wage per hour or day would decline under competitive capitalism. I claim no competence or interest in such doctrinal history.

Of the many uses we can make of the past, one—but certainly not the only one—is to reask some of the questions older writers posed and to provide them with answers in terms of modern analytical methods and terminology. The Marx-like or Ricardo-like model I described could be stripped of all proper names and could as well be described thus: a simple model involving labor, unlimited land, producible circulating- and fixed-capital items; and involving competition-consistent technology.

From the present methodological slant, it may not be presumptuous to wonder what would be Marx's comment on my 1957 model and analysis. I suspect with Gottheil that he might disagree often with my reasonings. Although Marx is not here to speak for himself, it is a legitimate question to ask whether my conclusions can stand up to the objections which seem implicit in Marxian reasonings and categories, and I am grateful to Gottheil for having raised specific queries for further consideration.

Query 1. If Marx and I agree it is unthinkable for production not to take time, which is the more appropriate behavior equation of competitive capitalism: mine, in which unripened spring product sells at a discount to finished autumn product (corresponding to a market-determined positive interest or profit rate); or the view quotable from Gottheil, "according to Marx . . . intermediate product will exchange one for one against *themselves* when ripened—if they contain the same quantity of labor . . ."?

That a modern theorist will agree that my position is the one appropriate to competition seems to admit of little doubt.

Query 2. If an improvement in technology becomes known to one or more producers, and even if they realize that after they have used it for some time competitive imitation will deprive them of their temporary "monopoly" profits, is it possible for the new equilibrium to involve a lower interest (or profit) rate *and* a lower real wage rate? My 1957 answer was categorically No: if the innovation is competitively viable, it *must* either raise the real wage, the interest rate, or both. This theorem I hold to be valid whether there are fixed or variable coefficients of production; and I may remind readers that I have no great liking for the usual narrow interpretation of Ricardo and Marx which imputes to them a rigid fixed-coefficient assumption.

Gottheil seems to suggest that if Marx were here today he could *validly* put forward behavior equations other than mine which would negate my theorem. This I bluntly deny. Whether or not factor supplies react to their changing market prices, whether or not technological innovation is induced by factor-price changes so as to counteract initial price changes (as Marx, Hicks, and Fellner have argued), any new equilibrium that is truly characterized by a lower interest rate $r' < r$, must by the same kind of reasoning that makes two plus two equal four, be characterized by a higher real wage $(w/p_2)' > (w/p_2)$; and if in the short or longer run we have the old and the new technologies persisting side by side, then the computed average profit and wage rate cannot both go down.

If Joan Robinson and I are wrong in this contention, we are dead wrong, it not being an issue upon which two contradictory opinions can be legitimately held. And no reasoned defense of the opposing view have I yet seen.

In standing my ground, I hope I am not contradicting Professor Gottheil

or any one else but rather am clarifying the methodological background to the earlier discussion.

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The Pure Theory of International Trade: Comment

In his admirably lucid and comprehensive survey of "The Pure Theory of International Trade" [1] Robert Mundell ends his analysis by a commendable attempt to see how far the results of the simple two-country two-commodity model remain valid in a multicountry multicommodity world. He concludes that in the latter case it is necessary to assume that tax proceeds are spent entirely on domestic goods in order to establish the conclusion that the terms of trade are improved by a uniform import duty, a consumption tax on imports, or a production tax on exports [1, pp. 105-7, esp. n. 30], whereas in the former model this conclusion can be derived on the assumption that tariff proceeds are redistributed and their expenditure divided between the goods in accordance with marginal propensities to consume. The contrast between the results for the two models is curious in view of the well-known Hicksian theorem that a bundle of commodities whose prices change in the same proportion can be treated as a single good; and it turns out that the difference is due to an error in Mundell's treatment of the income effect in his elasticities of demand for imports. When this error is corrected, it appears that there is no difference between the two cases. For brevity, the demonstration is confined to the effect of a uniform import duty.

Mundell shows that, on the assumption that all export goods are gross substitutes (an increase in the price of one country's exports, other prices constant, improves the balance of payments of every other country), a change in one country's policy will definitely improve its terms of trade if it has the effect of worsening the balance of payments (at initial prices) of every other country. The problem is therefore to determine the effect of the policy change on the balance of payments of the typical other country. For a tariff whose proceeds are spent on home goods, Mundell writes this effect as $-\eta_{jo}I_{jo}$, where I_{jo} is the initial quantity imported from country j by country O (which has imposed the tariff). The elasticity η_{jo} is described as "the elasticity of demand for imports (with respect to own price) . . . from country j to country O " [1, p. 106]. If this is understood in the usual sense of the elasticity with respect to the price of j 's good, Mundell's expression for the effect of the tariff is clearly erroneous, since the tariff changes the prices of the other countries' exports to country O , with cross-effects on O 's imports from j which are not represented in the formula. Mundell has since explained (in correspondence) that he meant η_{jo} to stand for the elasticity with respect to the price of country O 's domestic good; the revised definition is assumed in what follows.

Since η_{jo} is positive by the gross-substitutes assumption, country j 's balance of payments is necessarily worsened; hence a tariff whose proceeds are spent on domestic goods necessarily improves the terms of trade of the

tariff-imposing country. But if the tariff proceeds are redistributed and spent like marginal income, the effect (at initial prices) of the tariff on country j 's balance of payments is $-\eta_{jo}I_{jo} + m_{jo}I_o$, where the second term is the product of country O 's marginal propensity to import from country j and the initial quantity of country O 's imports and represents the effect of the expenditure of the tax proceeds. The expenditure of the tariff proceeds introduces a positive term to be weighed against the negative term of the former case.

To determine the net effect on country j 's balance, it is necessary to break the elasticity term down into its substitution and income components. It is here that Mundell made his mistake, for he writes his income term as $m_{jo}I_{jo}$, which is less than $m_{jo}I_o$, and concludes that the sign of the effect on j 's balance is indeterminate. But $m_{jo}I_{jo}$ is the income term for a rise in the price of the j 'th good only, whereas the tariff has raised the price of all imports relative to the domestic good, so that the income term should be $m_{jo}I_o$. This exactly cancels out the effect of the expenditure of redistributed tariff revenue, leaving the effect on country j 's balance as $-\eta'_{jo}I_{jo}$ (the prime indicating a compensated elasticity); this is necessarily negative by the gross substitutes assumption,¹ so that j 's balance (at initial prices) is worsened and the equilibrium terms of trade of the tariff-imposing country necessarily improved. The effect of a tariff whose proceeds are redistributed is therefore the same in the many-country many-good case as in the two-country two-good case.²

In conclusion it may be helpful to make explicit three possibly important points implicit in Mundell's argument. A good may be a "Giffen good" in aggregate demand without being inferior in any individual's consumption [1, p. 75]. The necessity of an inelastic foreign demand if growth is to reduce real income is limited to the complete-specialization model [1, p. 85]. When domestic demand for imports is inelastic a tariff whose expenditure is biased towards imports may turn the imposing country's terms of trade against it [1, p. 86 and *passim*].

HARRY G. JOHNSON*

REFERENCE

1. R. A. MUNDELL, "The Pure Theory of International Trade," *Am. Econ. Rev.*, March 1960, 50, 67-110.

¹ Gross substitution means that the positive income effect of a fall in the price of one good on the quantity of the others demanded is always outweighed by a larger negative income effect.

² In terms of Mundell's formula [1, p. 106, n. 30] the final effect of a tariff whose proceeds are redistributed is:

$$\frac{dP_i}{dt_0} = \sum_{j=1}^n \eta'_{jo} I_{jo} \frac{\Delta_{ji}}{\Delta}$$

where the prime denotes compensated elasticity and the determinant ratios refer to the effects of price changes in multiple markets. The compensated elasticities are positive and the co-factor ratios negative under the gross substitutes assumption, so that the tariff unambiguously improves the terms of trade of the tariff-imposing country.

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Inflation: Correction and Restatement

In a paper on inflation in the March 1960 issue of this *Review*, I attempted to abstract from monetary complications by assuming "... that the monetary authority provides sufficient funds to meet the requirements of the economy at a fixed interest rate" [4, p. 20]. It has been pointed out to me¹ that this assumption invalidates that part of my argument which considers the impact of a wage-push under the assumption of spender real-income consciousness. It is also inconsistent, in spirit at least, with my assumption [4, p. 29] that "It appears strategic, however, to abstract here from the effects of fiscal policy designed to maintain full employment, in attempting to assess the impact of cost-push, since fiscal policy is a tool used to deal with the unemployment ... which results from a wage rate increase."

I attempted to show that under certain conditions a spontaneous wage increase, in contrast to a spontaneous increment to demand, could be identified by the unemployment it causes. A spontaneous wage increase tends to generate unemployment under a number of circumstances, three of which are: (1) reduced spending (in real terms) via the money illusion²; (2) if the monetary authority does not provide sufficient funds to meet the requirements of the economy at a fixed interest rate, spending may be reduced also via (a) real-balance effects on consumption and (b) a reduction in investment as the interest rate rises³; (3) reduction in spending on investment and exports due to rising wage-cost effects.⁴ In my formal model, I let the wage increase generate unemployment through the money illusion and then attempted to ascertain the extent to which the resulting shift in distribution of income between wages and profits affected spending and thereby offset the unemployment effects of the wage increase. I concluded that some unemployment was likely to remain. In considering the significance for my results of assuming real-income consciousness, I concluded [4, p. 36] that the unemployment effects would be "... offset in whole or in part. . . ." In terms of my assumptions, this was an error—"offset completely" would have been the correct statement. Given real-income consciousness, price flexibility, and adjustments in the money supply to keep the interest rate constant, a spontaneous wage increase would set off a chain of events ending in a new equilibrium in which all factor incomes and prices would have increased proportionally leaving the real variables of the system, including the level of employment, unaltered.⁵ Under these circumstances there would be no redistributive effects.⁶

¹ I am very much indebted to Michael Lovell and my colleagues J. Crutchfield, D. Gordon, and A. Zellner for discussions of the contents of this note.

² Defined here as "money price illusion" [3]. "Money income illusion" could lead to increased employment.

³ Assuming no liquidity trap and an investment function not perfectly inelastic with respect to the interest rate.

⁴ These so-called "indirect cost-pull" effects were considered in the paper but not incorporated into the part of the model under discussion.

⁵ Unemployment could of course occur in the transition to the new equilibrium.

⁶ This follows well-known propositions advanced by Walras, Hicks, and Lange (cf.

Had I assumed an incomplete adjustment of the money supply to the rising wage and price level, real-balance effects would have reduced consumption expenditures; in addition, a rise in the interest rate would have curtailed investment spending. Under these circumstances, the shift from money illusion to real-income consciousness would not have completely wiped out the unemployment generated by the wage-push.⁷ Obviously, a wage-push with both money illusion and monetary stringency would induce a higher level of unemployment than with money illusion alone and for this reason would be easier to distinguish from a demand-pull situation (which inevitably leads to reduced unemployment).

Spontaneous wage increases may generate inflation and it is meaningful, in my opinion, to try to distinguish such an inflation from a demand-induced inflation. Changes in the level of employment can only be used to distinguish them if the wage-push has co-conspirators like money illusion and incomplete monetary adjustment—the more and the stronger the co-conspirators, the easier the distinction.⁸

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3. T. C. LIU AND C. G. CHANG, "Rejoinder," *Am. Econ. Rev.*, March 1953, 43, 148-51.
4. F. D. HOLZMAN, "Inflation: Cost-Push and Demand-Pull," *Am. Econ. Rev.*, March 1960, 50, 20-42.
5. K. W. ROTHSCHILD, "Aggregative Wage Theory and Money Illusion," *Jour. Pol. Econ.*, Oct. 1957, 65, 442-45.

[1, Ch. 6] for a summary statement) and has been demonstrated for a model similar to mine by Rothschild [5].

⁷ Bronfenbrenner [2] argues similarly in answer to Rothschild [5].

⁸ But, we might add, the quicker the wage inflation will grind to a halt.

BOOK REVIEWS

General Economics; Methodology

Czechoslovak Economic Papers. Prague: Czechoslovak Academy of Science (Ceskoslovenska Akademie Ved), 1959. Pp. 223.

This volume consists of 17 different articles, varying in length from 10 to 20 pages and dealing with a variety of subjects. As a result, the reviewing of such a nondescript collection is not easy, especially since in the judgment of the reviewer, none of these contributions is outstanding. Some of the articles are mere descriptions, while others contain propaganda, and, presumably, attempt to justify current economic policies of the Czechoslovak rulers. The question may be even raised as to why this collection has been translated into English and sent to the American Economic Association.

To indicate the variety of the subjects discussed, the titles of the articles and their authors follow: "International Division of Labor in the World Socialist System," by Vladimir Kaigl; "The Inter-relation of the Expansion in Gross Social Product and National Income," by Felix Oliva; "Certain Problems Connected with Building the Material Production Basis of Socialism," by Pavel Turčan; "Some Thoughts on the Class-Relations in Our Village," by Michal Faltan; "Certain Problems of the Operation of the Law of Value on the World Socialist Market," by Josef Mervart; "The Problems of Economic Efficiency in Foreign Trade," by Villiam Černiansky; "On the Basic Relations in Technical Progress and Their Influence on the Growth in Productivity of Labor," by Fantišek Kutta; "Problems of Stable or Variable Wholesale Prices," by Antonín Minář; "Economic Problems of Relations Existing Between Demand and Supply in Socialism and of Planning the Composition of Retail Sales," by Miloslav Kahoutek; "Czechoslovak Cost-of-Living Index New Series," by J. Mach; "Some Conclusions from a Statistical Analysis of the Census of Fixed Production Funds in Engineering," by Jiří Skolka; "The Disaggregation of an Absolute Increment," by Jiří Bouška; "The Evolution of Soviet Views on Statistics," by František Egermayer; "The Role of the State in Contemporary Capitalism," by Jaroslav Langr; "Agriculture in the U.S.A.," by George S. Wheeler; "Effect of the Structure of India's Economy on Her Foreign Trade," by Zdeněk Švejnar; "Emigration from Slovakia Between the Years 1870-1940," by Jan Svetoň.

In view of the variety of topics, there is little reason for reviewing all these articles. However, a few of them may merit some comments. Vladimir Kaigl's article, "International Division of Labor in the World Socialist System," contains a criticism of past and present policies of the countries behind the Iron Curtain in their attempts to reach self-sufficiency, and the resulting low volume of trade between them and with the outside world. He pleads for expanded international trade based on the principle of comparative advantage and ob-

serves that this can be attained by determining more exactly costs of production. He is critical of the theory prevailing in these countries, that foreign trade is "a kind of marginal supplement to the national economy used as a first aid in case of sudden developments of disproportions. According to this 'theory,' the purpose of foreign trade is to import what we need from abroad and to export as much as necessary to pay for these imports" (p. 19).

Villiam Černiansky's article on "Problems of the Economic Efficiency of Foreign Trade" is a discussion of ways and means to achieve "the greatest possible savings of social labor by exchange of goods with foreign countries." The point is made that "a simple comparison of domestic prices with the price obtained abroad is not satisfactory as domestic prices are notoriously distorted by too great differences in the mark-up and taxes and by inclusions in the price of components that ought not be included at all" (p. 111).

"The Role of the State in Contemporary Capitalism" by Jaroslav Langr is a typical Communist diatribe against the Western world, with the usual humbug about capitalist evils and inequities. The article concludes that "after World War II capitalism has not entered any higher evolutionary stage modifying in any way the nature of imperialism, as the highest stage of capitalism. After World War II, the tendencies of the state monopoly capitalism had gained momentum primarily through the militarization of the national economy, through nationalization of certain industrial enterprises, through a more intensive use made of State budgets. This evolution did not result, however, in any changes marking the beginning of a new stage of imperialism. The evolution is taking its course in accordance with economic laws and together with the strengthened tendencies of State monopoly capitalism provides further evidence of the disintegration of this social system and the creation of a material basis for a new and higher social order—that of socialism" (p. 288). In the article "Effect of the Structure of India's Economy on Her Foreign Trade," Zdeněk Švejnar concludes that as a result of British colonial policies, India, since achieving independence, is at the mercy of the United Kingdom, the United States and the German Federal Republic regarding her imports and exports. An expansion of India's trade relations with the countries of the Socialist bloc is necessary to accelerate economic expansion and independence according to him.

Most of the authors refer to the sacred writings of Marx, Lenin, and Stalin in support of their assertions.

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Basic Economics. By THOMAS J. HAILSTONES. Cincinnati: South-Western Publishing Co., 1960. Pp. iv, 513.

Basic Economics, written for a one-semester course populated by nonmajors, is devoted exclusively to macroeconomics. Hailstones writes in crisp short sentences, pitching his analysis well below the average elementary text, including many tedious repetitions and numerical illustrations, with an occasional humorous aside. Bearing many similarities to a handbook, emphasizing

definitions, techniques, and statistics, the book will do little to stimulate its audience to do further thinking and reading about economics. An offsetting factor is the questions at the end of each chapter, raising interesting problems relating economics to other disciplines, but requiring analysis beyond that offered in preceding pages. A short book must be selective, and this one has chosen for major attention: business cycles and money and banking.

The first four chapters cover the usual introductory material clearly, but too briefly for the reviewer's taste. Even though Hailstones' objective is to expound macroeconomics, more could have been done to set the scene in terms of scarcity and allocation. The novice is not given a clear picture of what is excluded and how this limits the analysis. And since he aims at people who will not again brush against formal economics, the relation of other scientific thought to the problems encountered and methodology adopted could have been more fully explored.

Part II, perhaps the best in the book, describes the banking system and contains a thorough description of money creation. One might quibble about presenting the transactions analysis before defining money, and question why general credit controls are alleged to restrict money for specific uses (p. 148) with no analysis of rationing by interest rates. An unusual amount of space is devoted to the history of banking and the structure and techniques of the Federal Reserve System, with only a few pages given to an evaluation of the effectiveness of monetary policy. Inflation, regardless of degree, is painted as an unmixed evil.

The following Part is a potpourri of definitions and miscellaneous relationships, called "Production, Employment, and Income," including an overview of the determinants of GNP and a balanced but dated discussion of the Employment Act of 1946. Its chief merits are a good description of the limitations of GNP as a measure of well-being, an interesting discussion of the prediction of the GNP, and a useful short exposition of flow-of-funds accounts. A mass of statistics depicting personal distribution of income is offered without reference to the functional theories listed, but left uncoordinated, in Part I. Lack of a clear statement on the consumption function leads to a confused, and partially inaccurate, definition of the multiplier (pp. 225-30). Nevertheless, the stage is appropriately set for the second half of the book: instability, the malady and its treatment.

The first two chapters of Part IV present an appropriate general discussion of fluctuations, perhaps a bit too simplified, but including a few novel devices to enhance the clarity. The rest of the section describes all of the traditional business cycle theories, but does little about integrating them.

The final Part portrays income-expenditure analysis first in terms of aggregate demand and supply and then in terms of the consumption function and marginal efficiency of capital. The first approach is quite successful but the second suffers from a failure to distinguish between planned and realized investment. This section also includes a fair but conservative evaluation of policies for dealing with price and employment stability, but neglects growth implications.

Hailstones' writing is neither provocative nor muddy; hence the book could

be advantageously used along with "controversial" material. However, any rigorousness in analysis would have to be provided by the instructor.

ARTHUR D. BUTLER

University of Buffalo

Concepts and Cases in Economic Analysis. By AARON W. WARNER and VICTOR R. FUCHS. New York: Harcourt, Brace and Co., 1958. Pp. xv, 288. \$3.00.

Professors Warner and Fuchs of Columbia University have produced a supplementary case book for use in a one- or two-semester introductory course in economics. In content, about one-fourth of the book is devoted to discussion of analytical tools and concepts, with the remaining three-fourths devoted to cases. There are many (188) short cases. Each case is followed by several questions designed to apply the principles outlined in the preceding expository sections. Some cases presuppose a knowledge of such economic institutions as the banking system, the elements of business organization, and trade unions, and of the procedures of national income accounting, not covered in the expository section of the book itself. For this reason it is necessary either to build on a prior course in economic institutions and principles or to use another book of readings or standard text concurrently.

In its organization, this book is heavily weighted in microeconomics. Of its total 284 pages, 196 pages or 69 per cent of the content is devoted to price theory. Only 58 pages or 20 per cent is devoted to aggregative analysis. Of these 58 pages, 15 are devoted to monetary concepts, and the balance to Keynesian concepts. Some major areas, including international trade, labor economics, corporation finance, and distribution theory, are discussed only incidentally or not at all.

Use of this book in an introductory course in economics, like any other case book, will add some variety to an introductory course, perhaps at the expense of breadth and institutional description. This particular case book would also add a great deal of interest to the course. The cases are almost universally interesting and well selected. Many of them are from the world of the *New York Times*, the *Wall Street Journal*, and semipopular magazines. One attempt to draw from the commonplace may have gone too far. A case entitled "Branch Rickey's Baseball Equation" requires for its analysis a more thorough knowledge of baseball than of economic principles.

The questions for thought and discussion following each case are particularly well done. The questions bring out important economic principles illustrated by the case and provide the student with exercise in some analytical tools. For many students study will become more productive as a result of the forced thinking involved in answering the questions. Possibly also the class period will be enlivened by debate.

Along with the above strengths attributed to the book, there are also weaknesses. The most obvious is its rather lopsided organization, with considerably more than half of the book devoted to price and output problems. There are far more cases included in this section than a one-year course could

profitably digest. The area of money and banking in contrast is given very little attention. Only one of the many important analytical problems of the field is considered—the forces determining the general price level are analyzed through the equation of exchange. Only about two pages are devoted to the important area of monetary policy. International economics is given attention in only seven cases, and most of these analyze elasticity of demand for imports. There are no cases concerned with corporation finance, though a few cases are included dealing with break-even charts, refusal pricing, and marketing problems. For these reasons, the book does not adequately meet a need for cases covering the full range of interests included in standard introductory courses. In their preface the authors state that "We have designed the book to be used either as a basic text in economic analysis or as a supplement to be used with other readings." For this purpose, a more balanced treatment would seem in order.

For limited rather than general use this reviewer believes that this case book will make a useful and important contribution to our teaching aids. Its adoption by individual professors will, however, depend upon the degree to which one wishes to emphasize (1) price theory and secondarily Keynesian theory rather than a broader range of topics and (2) economic analysis rather than institutional description.

WALTER J. MEAD

*University of California
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Price and Allocation Theory; Income and Employment Theory; Related Empirical Studies; History of Economic Thought

Ekonomicheskii raschet nailuchshego ispol'zovaniia resursov. (Economic Calculation of the Optimum Use of Resources.) By L. V. KANTOROVICH. Moscow, 1959. Pp. 343.

In a pamphlet published in 1939, Kantorovich gave a general formulation of the linear programming problem, and described a practical method for solving such problems. His method was based on finding what he called "resolving multipliers," which a Western economist would today recognize as the shadow prices of the dual of the linear programming problem. For nearly twenty years this pioneer work was ignored by the Russians, but with the interest in mathematical methods in economics since Stalin's death, they have "discovered" it, and economic thinking in the direction pursued by Kantorovich has been approved and encouraged. The book under review is one of the results. In this book Kantorovich has developed his ideas far beyond the point achieved in the original article. He now recognizes the broader significance of his resolving multipliers and acknowledges their meaning by labeling them "objectively determined values." Much of his book is devoted to the demonstration that the finding of these values is equivalent to finding the optimum plan, that is the plan which gives the largest output. In the course of the exposition he develops the concepts of opportunity costs, the rental value of

superior land, quasirents on capital goods, and scarcity values for current inputs. In Chapter 3 he shows how his methods can clarify some of the issues in planning capital investment. He starts with the proposition that only those capital goods should be created for which the quasirents exceed the cost of production, both figures being based on his "objectively determined values." But there are many more items meeting this criterion than can be created out of current investment resources, and the rule is to create them in descending order of productivity up to the limit of investment resources. He explains the rationale for discounting future quasirents in the process of capitalization and shows how this is to be done in several situations. Finally he lays stress on the point that these objectively determined values are not only an aid in calculation, but will also serve as a set of indexes which, in contrast to existing prices and value indicators, will provide a rational set of incentive indicators and an accurate evaluation of performance, for the guidance and control of local management.

Thus what started out as an algorithm for finding optimum values of certain variables in a problem of production planning has culminated in the rediscovery of much of Western value theory. Of course it is a seriously truncated value theory, which treats the composition of output as already determined and only seeks to maximize output in the proportions assigned. It is still a cost theory of value, but a sophisticated one, involving imputation and opportunity costs.

This is surely a portentous event in the development of economic thought in the Soviet Union. It is a significant step in freeing the Russians from the limitations of Marxist theory, so that Soviet economists can at last provide some clarification of the issues that practical planners must deal with. It marks the way for the transformation of Soviet economists from priests to scientists. Whether these potentialities are realized depends on whether this threat to orthodoxy is allowed to pass. Kantorovich's objectively determined values and Marxist value are clearly not the same thing, and Kantorovich has not been unaware of this problem. He has put probably the best possible face on the matter. He contends that there is really no contradiction, basing his argument mostly on the idea that "socially necessary labor time" under modern conditions of production acquires definite meaning only when one considers alternatives, capacity limitations, and current availabilities, and that all he has done is to work this out concretely. He has taken care not to make the politically dangerous step of formulating his value theory in a way which might imply that composition of output was a problem to be solved. Likewise he avoids the ideological *gaffe* of bringing in utility (a concept inevitably linked with the epithet "notorious" in the Soviet discussions of value theory). His values *are* objectively determined by all the material circumstances of the problem to be solved, and he has forestalled any taint of idealism. But others have not been willing to ignore the discrepancy; and both in the introduction to the book by Nemchinov and in reviews in the economics journals this discrepancy is noted. But the book is published (though in a relatively small edition), and the defense of orthodoxy put up by Nemchinov and the reviewers can only be described as lackadaisical. Any perceptive sophomore can see that

Kantorovich's values are not Marxist values, but that they are indeed "objectively determined," that is "true" values. But apart from its fate as a foray into the theory of value, the book is a testimony to the proposition that the problem of economic theory is to show how to maximize the output from scarce resources, and this portion of its message alone will mean a revolution in Soviet economic thinking.

ROBERT W. CAMPBELL

University of Southern California

Capital, Interest and Profits. By B. S. KEIRSTEAD. New York: John Wiley and Sons, 1959. Pp. 180. \$4.00.

This book is at once pleasing and provocative and a scholarly blending of theory and history, aggregative and partial analysis, and the economics of developed and underdeveloped countries. It presents criticism of "accepted" theories as well as original contributions, all this within 171 pages. The book is divided in two equal parts.

The analysis of the first part concerns the process of capital formation. This requires investment (the making of tools) and saving (the provision of purchasing power). Capital formation is unlikely to get started without a "social shock," which initiates the investment and provides the necessary saving. The process takes time and there is a lag between the start of the investment and the resulting increase of output.

This sketch of capital formation is the background for Keirstead's criticism of the marginal productivity theory of distribution and the time preference theory of interest and for his theory of profit, interest, and return to stock. His theory of profit stresses the uncertainties of innovation as contrasted to risk. The uncertainties arise from two kinds of expectations: general, those arising from forecasting the gross national product, price levels, etc.; and particular, those pertaining to the expectations for the industry or firm. In a static economy with perfect knowledge and competition, profits would not exist.

Keirstead presents the loanable funds theory of interest. The rate which firms have to pay for investable funds must cover the opportunity cost of consumer borrowing including that of government, and in addition a return to cover the costs and risks of the lender. Interest is not a reward for waiting because a large proportion of saving is supplied involuntarily by the rich and by corporations, and through monetary and fiscal policies. Only a small proportion of saving is supplied voluntarily and this supply moves inversely to changes in the rate of interest. Hence the rate reflects a large element of scarcity value under normal circumstances.

The analysis then turns from the general market for capital to that of the firm. Keirstead rejects the marginal productivity theory that the firm equates the marginal cost and marginal revenue of capital. Nevertheless he presents a determinate explanation of the firm's use of capital. On the supply side the firm must pay the opportunity costs of the different sources of funds which it must have to carry out its plans. These are its own internal funds, bank loans and the flotation of new securities. The opportunity cost rises as the firm has

to draw upon additional sources so that the firm is confronted by a "stepped" supply schedule.

On the demand side the firm's schedule is likewise discontinuous. The investment plans of the firm are governed by three factors in addition to the expectation of gain: flexibility, economies of scale, and the time horizon. Investment plans are based upon a variety of hypotheses and should be flexible since expectations may turn out to be incorrect. Economies of scale together with the time horizon of the plans determine the growth of the firm. The demand schedule of the firm slopes downward because, other things being equal, uncertainties increase with the length of the time horizon. Hence the firm's use of capital is determinate.

In the conclusion to Part I Keirstead writes, "What I have tried to do in this brief essay is to supplement criticism which I have made in previous books, of the marginal productivity theory of distribution with some more positive statements" (p. 80).

Part II is devoted to "the elaboration, illustration or application of points made briefly in passing in Part I" (p. ix) and consists of five essays. The first illustrates the characteristics of a "conventional" economy and the conditions that are necessary for the initiation of capital formation with a wealth of historical and anthropological examples. The second is a case study of Newfoundland. The third is an application of sector analysis in theory and practice to the dual problem of inflation and unemployment. The fourth essay is a brief criticism of certain cyclical concepts, periodicity, the multiplier, and the acceleration principle. The last is a brief conclusion to Part II followed by a statement of implications for policy.

This is a provocative little book both for what it says and for what it does not say. Its penetrating criticisms induce reactions both pro and con. Here I wish to offer some comments on what it does not say; for my criticism is that the book is too short. In the preface, again in Part I and at the end of Part II the author states that the book is concerned with the theory of distribution. But Part II deals mainly with problems of capital formation. The average undergraduate is unlikely to integrate the two parts for himself. Of more importance for the advanced reader is my impression that the truly original argument concerning the firm's demand for investment might have been more fully developed. This is new ground and needs cultivation.

Notwithstanding these comments this short book offers rich rewards for a variety of students and because of its unique scope and stimulating style should receive a wide acceptance.

JOSHUA C. HUBBARD

Bryn Mawr College

Studies in the Economics of Welfare Maximization. By P. R. BRAHMANANDA. Bombay: Bombay University Press, 1959. Pp. xiv, 520. Rs 18.

Dr. Brahmananda begins his preface with: "By and large economics has been pursued not merely for the sake of knowledge but also for the sake of the healing and hope that knowledge brings" (p. viii). The whole volume reflects this point of view. The author has read and interpreted the classics with

sympathy and understanding; but his primary interest has been not in theory qua theory, but in the tools that theory has forged which may be used in dealing with problems of economic development.

Part I traces the evolution of thought in the writings of Smith, Malthus, Ricardo, Mill, and Sidgwick on the role of the state, conditions favorable to enterprise, and the factors bearing upon population change and capital formation. Brahmananda notes (and the reviewer approves) that "the present antithesis between growth and allocation is not justified and to understand the process of change and its determinants we have to study in detail the workings of the allocation mechanism: it is through the latter that the changes in the supplies of factors can be influenced" (p. 158).

Part II begins with an exhaustive discussion of Marshall's analysis of the surpluses that arise, both to consumers and to producers, because of the conjuncture in which individuals find themselves when they trade. The author is aware of the difficulties involved in measuring these surpluses and of the current controversy in this area, but he expresses confidence in the possibilities of quantification for purposes of economic policy, formulation almost to the point of making a fetish out of the idea. For example he writes, "It is now necessary to concern oneself with practical problems. It is here that the concept of consumers' surplus is of immense aid" (p. 253). One wonders just how one would get the necessary data to employ the surplus concept in deciding whether to use Indian resources in building a new steel mill rather than a textile plant.

This section continues with an examination of how value judgments, the stock of knowledge, the distribution of income and wealth, indivisibilities, individual versus collective perspective, and the divergence of private from social product bear upon economic welfare.

Part III catalogues a number of involuntary infra-optimal situations and the need for remedial action. Much use is made of the concept of increasing returns (again displaying the strong Marshallian bias that permeates the entire volume) and of the possibilities of external economies. This part concludes with a chapter on public utility pricing.

Brahmananda is at his best when discussing the conditions necessary to promote economic development in India. For example he observes that "Competition is only the husk; the kernel is the strength of the economic motive" (p. 385). And again he writes, "In some countries, such an action [i.e., deliberate state action] requires a change in time-honoured customs, traditions and mores, against which individual units might be striving in vain; it requires support and encouragement for the limited number of leaders, who are making the community conscious of the non-optimal positions; it requires a coordination of the efforts of all those who are striving to raise the tempo of progress; it requires direct State activity or initiative to break the rigid bonds which keep individual units in chains, as it were. In other words, in most undeveloped countries, a well-coordinated and concerted attack, planned from the vantage point of a collective perspective, may be the only method by which the so-called 'natural' trends and courses of activity, which have such tremendous inhibiting effects, can be transformed" (pp. 430-31).

In the reviewer's judgment, students of welfare economics and of economic development will find much of interest in this volume. It does not break new ground in the area of pure theory but it does contain real insight with respect to some of the most crucial economic problems of our times.

WILLIAM B. PALMER

The University of Michigan

La répartition du revenu national. Vol. 3, *Modèles classiques et marxistes*. By JEAN MARCHAL and JACQUES LECAILLON. Paris: Ed. Génin, 1959. Pp. 393. 2,400 fr.

The volume under review is the third volume of a four-volume study of the distribution of the national income by Professor Marchal of the faculty of Law and Economic Sciences of the University of Paris and Professor Lecaillon of the faculty of Law and Economic Sciences of the University of Lille. The first two volumes were gracefully reviewed in *Review* for September 1959 by George Jaszi. In those volumes the authors divided receivers of income on the basis of five major groups and analyzed the characteristics of these varied recipients.

In Volume 3 Marchal and Lecaillon begin the second and last part of their long work, the part which studies the processes by which these groups obtain their respective portions of the national income. Before attempting the construction of a model of distribution viable for the modern world, the two collaborators deem it wise to investigate the different models proposed in the past century and a half in order to ascertain in what measure these models still provide instruments of analysis acceptable for the comprehension of today's realities. Hence the third volume is devoted to a critical discussion of models, classical and Marxist. Volume 4, in preparation, will take up the neo-classical, Keynesian and post-Keynesian models.

By way of introduction, the Physiocrats are held incontestably to have been the first scholars to have raised the problem of distribution in its entirety and clearly to have employed the macroeconomic and sociological approach. The classical model is then scrutinized in three chapters of approximately 35 pages. Adam Smith is hailed as the founder of the English classical school but as far more concerned with problems of production than of distribution. The real architect of the classical theory of distribution emerges in the person of David Ricardo. While the approach of Smith is looked upon as necessarily that of microeconomics, the approach of Ricardo, like that of the Physiocrats, is regarded as embodying the essentials of macroeconomics.

Discussion of the Marxist model, by contrast, runs to approximately 335 pages. In five relatively long chapters the reader is introduced to the functional aspect of the Marxist model, the determination of the variables of the model, the evolution of the variables, and the evolution of the different social classes which share in the national revenue. Chapter 6 closes the discussion on a lofty note with some general reflections upon the Marxist model.

By reason of the many ties that allegedly exist between the classical and the Marxist theories, Marx has often been called the spiritual son of Ricardo. Yet Marx went much farther than his predecessor, believing as he did in a

long-range analysis of distribution which definitely classifies his theory as sociological, an approach barely considered by Smith and only slightly elaborated by Ricardo. With Marx, furthermore, the time is surely coming when at long last the total revenue of society will be lodged in the hands of the masses alone—the proletariat. Complete macroeconomics—a term of course unknown to Marx—will then be the order of the day.

Documentation is abundant and at times fascinating. The style is typically Gallic in its felicity. For readers whose command of the French language is fluent, here indeed is rewarding fare.

The issues raised by Jaszi in his review of the earlier volumes, however, still remain unsolved. Nor can any fair evaluation of the ambitious study undertaken by Marchal and Lecaillon be made until the promised fourth volume is at hand. This reviewer, mindful of the pleasure and profit which a careful perusal of the third volume has accorded him, awaits with anticipation the publication of the final volume in which the neoclassical and contemporary models of distribution are to be dealt with.

JOHN M. FERGUSON

Muhlenberg College

The Wage-Price Issue—A Theoretical Analysis. By WILLIAM G. BOWEN. Princeton: Princeton University Press, 1960. Pp. xv, 447. \$8.50.

This book examines the question whether “the wage- and price-setting institutions of the contemporary American economy impart an upward bias to the price level.” Of the various processes alleged to lead to such an upward bias, the one selected for detailed analysis is the “dilemma model” which provides the framework for much of the current discussion of this question. The model may be summarized in three assertions: (1) in our society strong unions are able to raise wages faster than productivity in general can advance; (2) these increases in unit cost of labor and unit total cost of product encourage producers to raise prices; and (3) monetary authorities are then faced with the dilemma: (a) to increase the supply of money, thus justifying the higher price level and setting the stage for another round of wage and price increases, or (b) to refuse to increase the money supply, thus halting the inflationary trend but causing unemployment by denying the expansion of funds necessary to buy all of the products available at the higher prices. These three assertions are analyzed in great detail in three separate sections which comprise the main body of the book.

The author finds that all three assertions contain serious faults which make the assertions highly suspect as a basis for public policy decisions. He argues that the wage-determination assertion rests upon a serious oversimplification of the complex relationship between the institutional factors of trade-union structure and the collective bargaining process, on the one hand, and the economic determinants of labor productivity, on the other. The reviewer believes that the author succeeds very well in showing that the wage-determination assertion is misleading and also that it is unsatisfactory because it fails to comprehend the basic causes of wage change.

His analysis of the cost-and-price-determination assertions begins with the

impact of wage adjustments on costs and proceeds to the impact of costs on prices. The author points out that the close link which the assertion assumes between unit labor costs and total costs of product is in reality quite loose unless certain possibilities, such as the substitution of other factors for labor, the change in the costs of nonlabor factors, and the alteration of the rate of output, can be eliminated from consideration.

The same kind of conclusion is reached with regard to the impact of unit costs on prices. The strict cost-plus pricing doctrine of the dilemma model does not give an adequate answer, since it is difficult to apply to sectors of the economy such as the administered price sector. Also the general price level depends upon innumerable individual price decisions, and demand considerations cannot be neglected. But the excess demand model does not provide a useful framework either, since it is applicable only to the competitive section of the economy. The author, therefore, attempts a fusion of the two to give a more serviceable approach.

The discussion of the monetary-policy assertion leads to the conclusion that it is incomplete, because it does not explain how monetary policy affects wage, price, and employment decisions. The importance of the monetary environment should not be underestimated, but the purely monetary approach does not answer certain fundamental questions such as the effect of change in velocity and in money supply, and the influence of the factors which determine private decisions to spend. Questions arise concerning the policies which the monetary-fiscal authorities are likely to adopt when the price level is being pushed upward, and what the impact of these policies will be. The inadequacies of the monetary-policy assertion add to the weaknesses of the dilemma model.

In spite of its faults, the author feels that the dilemma model has served a useful purpose in focusing attention on the macroeconomic problem, and that the controversy over the dilemma model has encouraged consideration of the relative desirability of alternative public policies before adopting any specific program. He concludes with the thought that computation of the relative costs of the various alternatives is likely to be arduous and time-consuming, but that to date no other acceptable procedure has been developed.

The author has covered his subject thoroughly and has defined his terms carefully. His analysis of the very difficult problems involved is painstaking and detailed. He is examining highly controversial matters, and disagreement with some aspects of his work is to be expected. But he has tried to account for every possibility, and although the result is wordy in places, the book is well organized with frequent summaries, comments, conclusions, and helpful diagrams to keep the reader on the track. Only an occasional slip was noted, such as the apparent confusion on page 29 of buyers' inflation with cost inflation and sellers' inflation with demand inflation. Good use has been made of the available literature in the field, and the book contains an excellent bibliography.

PAUL L. KLEINSORGE

University of Oregon

Money and Income. By A. C. L. DAY and STERIE T. BEZA. New York: Oxford University Press, 1960. Pp. xv, 634. \$7.50.

As a systematic exposition of macroeconomic theory for both a closed and open economy, this book is probably superior to any undergraduate text currently available on the U. S. market. Based on a volume originally prepared by the senior author for British consumption it shows the felicity of expression that one has learned to expect of British authorship.¹ It is truly a literary work depending on the skillful use of language to persuade the reader rather than on copious statistical tables or their graphic representation. Discussions of suggested readings after each chapter guide the reader into the standard works and the most recent periodical literature. A mathematical appendix is provided explaining the derivation of the various algebraic formulas provided throughout the text. These characteristics together with the level of abstraction attained throughout suggest that students of economics in English universities in their second and third years (the intended market for the original volume) are made of sterner stuff than their American confreres.

The first two sections are taken up with the determination of income levels and the rate of interest. A simplified monetary economy is the basis of the analysis with only three kinds of transactors—households, firms and banks—and three wealth forms—undated bonds, money and real wealth—being involved. Where the analysis goes beyond conventional treatment is in its discussion of dynamic movements from one equilibrium level to another. Here as in other parts of the book imaginative tables are constructed which trace the sequence of changes in the relevant variables. An analysis of the mutual determination of income and interest rates along the lines of Hicks' famous treatment occupies a first-class chapter in the second section.

Most of the third section, on the present-day institutional framework, is taken up with a description of the commercial banking system and the operations of the Federal Reserve. The role of government in economic activity is elaborated. In a final chapter the authors explain their preference for a liquidity-preference approach to interest determination over a loanable-funds approach.

Up to this point, price level changes have been ignored. This is remedied in the fourth section which develops a theory of inflation. An excellent introductory chapter singles out the weaknesses of the quantity theory down to its recent reformulation by Patinkin. The analysis of inflation is then carried on in the context of the modern theory of income determination. Inflation is seen to be the outcome of an excess of planned demand for goods and services over planned supply at full employment. From this perspective, wage increases are "defensive reactions" to price increases rather than initiating impulses.

In Part V, "Stability and Instability," a theory of the trade cycle is advanced on the basis of the familiar interaction of the acceleration and multiplier principles. The most intriguing chapter summarizes the work of Tustin and Phillips in applying engineering techniques to problems of economic

¹ A. C. L. Day, *Outline of Monetary Economics* (Oxford, Eng., 1957).

policy-making. The final two sections "International Monetary Economics" and "International Monetary Experience" consume about one-third of the contents. The first provides a rigorous discussion of the conditions for external balance using the same methodology of planned and realized values (here payments to and from foreigners) that was employed in the income determination section. The final section tackles the policy problems of simultaneously achieving internal and external economic stability.

Accepting the avowed intention of this book to concentrate on macro-analysis, it has certain notable sins of omission. One finds no discussion of the national income accounts, their conceptual basis, construction and limitations. There is virtually no discussion of consumer credit and other selective credit controls because the authors mistakenly believe that nothing can be said about them in "general terms" (p. 176). One and a half pages on non-bank financial institutions, including a one-sentence parenthetical reference to the possible effects of money-substitutes on the demand for money (pp. 124-25), are an inadequate recognition of the kind of work being done by Gurley-Shaw and Goldsmith.

The book's greatest sin of omission is more of a commentary on the state of economic knowledge than a criticism of the book per se. Except in the most general terms, when the history of U. S. fluctuations since the first world war is being discussed, little attempt is made by the authors to demonstrate empirically the validity of their macrotheory. This is probably explained by our ignorance of the causes of economic instability.² But if so, the lucidity of this volume may be a snare and a delusion.

If present efforts to make the money and banking course a course in macrotheory succeed, there should be a substantial market for this book. But its significant gaps in the economics of the financial firm and the mechanics of the financial system suggest that there is still room for a high-level course in money and finance which would precede the course in macrotheory.

JACOB COHEN

Bowling Green State University

The Business Cycle. By R. C. O. MATTHEWS. Chicago: University of Chicago Press, 1959. Pp. xv, 300. \$3.00.

In the manner of its predecessors, this latest edition to the Cambridge Economic Handbooks is "intended to convey to the ordinary reader and to the uninitiated student some conception of the general principles of thought which economists now apply to economic problems"—in this case the business cycle. If this is indeed true, the present book must be said to fail miserably in its objective, or the author and the editors have not really made up their minds about the audience being aimed at. There is little doubt that most of its contents will sail right over the head of the "ordinary" or "uninitiated" reader. On the other hand, the materials of interest to the specialist are hidden under a bushel; the specialist is likely to be unwilling to wade through all the familiar materials to pick out the few points worth his attention. At best,

² For a recent succinct statement on the extent of this ignorance, see R. A. Gordon, "Research on Economic Stability" Soc. Sci. Research Council, *Items* 13, Dec. 1959, pp. 37-38.

a few chapters are probably useful on a reading list for graduate students.

The book opens with two chapters devoted to a good, but not simple, discussion of the generation of cycles through the interaction of the multiplier and acceleration principles—the latter in its “pure” and “flexible” forms. The roles of lags, constraints, and erratic shocks are discussed. These chapters are followed by three more dealing with other influences upon investment. Then follow chapters on the consumption function, money and finance, ceilings as an explanation of the upper turning point, the lower turning point, international aspects of the business cycle, and a (poor) chapter on periodicity and major and minor cycles. The penultimate chapter, perhaps the best in the book, is a very good discussion of the problem of long-run trend and the business cycle. The last chapter deals with problems of controlling the cycle, but the reader will get very little idea of the tools of compensatory fiscal policy and none of monetary policy.

The inadequacy of the final chapter is characteristic of a major failing of the book, viz., a questionable distribution of emphasis. All too often major points and conclusions are given short shrift, whereas minor topics and refinements are analyzed extensively. A related drawback is the author's frequent eagerness to cover all possibilities, e.g., all possible influences on investment. As a result, the reader is apt to lose sight of the forest for the trees and thus be unable to separate the important from the unimportant.

Readers of this review will be surprised to learn, *inter alia*, that the profit rate will not necessarily fall in the face of a declining marginal product of capital, because the profit rate (P/K) equals profit per unit of output (P/Y) times the average product of capital (Y/K), and a rise in P/Y may keep P/K from falling (p. 153). This not uncommon fallacy may immediately be exposed by recalling that $P/Y = MP_k/AP_k$, where MP_k is the marginal product of capital and $AP_k = Y/K$. Then:

$$\frac{P}{K} = \frac{MP_k}{AP_k} \cdot AP_k;$$

Since the AP_k 's cancel out, it follows that the behavior of the profit rate depends only on the behavior of MP_k , and the behavior of both is independent of both capital's relative income share (P/Y) and the average product of capital.¹

I should like to close this review on a more constructive note. The author attempts to give a theoretical underpinning to his discussion of the flexible accelerator by referring to Marshallian long-run equilibrium theory of the firm. A more direct and explicit approach might be to observe that, in equilibrium and in the absence of technical progress and population growth, capital stock is wanted by the firm essentially for two reasons: (1) to produce more output with the same techniques, and (2) to produce the same output with more capital-intensive techniques by substitution of capital for labor. In the

¹Incidentally, contrary to the author's belief (p. 242), only in the knife-edge case of a linear and homogeneous production function does a falling marginal product of capital necessarily imply a falling average product of capital or a rising “normal” capital-output ratio.

face of diminishing marginal returns, this second source of demand for capital must be a decreasing function of the rate of interest. Hence we can write a capital-demand function as:

$$(1) \quad K = f(Y, i)$$

Differentiating (1) with respect to time, we get:

$$(2) \quad \frac{dK}{dt} (= \text{Investment}) = g\left(\frac{dY}{dt}, \frac{di}{dt}\right)$$

or as a linear approximation,

$$(3) \quad \frac{dK}{dt} = I = a \frac{dY}{dt} - b \frac{di}{dt}$$

Investment is thus a positive function of income change and a negative function of change in the interest rate. To maintain a given rate of investment, income must keep growing and/or the interest rate must keep falling. The coefficient a in (3), of course, is the accelerator, $\Delta K/\Delta Y$, which is seen to define the increase in capital stock needed to produce a given increase in output at a given rate of interest and in a given state of the arts. The acceleration principle as a theory of induced investment is thus placed on simple and perfectly sound theoretical grounds. Any modifications needed to allow for excess capacity, finance problems, etc., can be and have been introduced in the form of a flexible or variable (nonlinear) acceleration coefficient. But the point is that any theory of induced investment must begin with equations like (1) and (2), with modifications introduced *therein*, because these equations provide the only sound "micro" basis for such a theory.

D. HAMBERG

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Philosophie des conjonctures économiques. By LÉON-H. DUPRIEZ. Louvain: E. Nauwelaerts, 1959. Pp. xiv, 506. 425 Bfr.

This book is a companion volume to Professor Dupriez's *Mouvements économiques généraux* (2 vol., Louvain 1947), and is based on the latter's empirical and statistical findings. While the *Mouvements* dealt with the phenomenological aspect of economic trends the *Philosophie*'s emphasis is on the conceptual aspect of these tendencies. In addition to its three main parts (the principles, long-term, and short-term conjunctures) the book includes a helpful glossary of technical terms, and a bibliography of related empirical studies made under the auspices of L'Institut de Recherches Économiques et Sociales of the University of Louvain.

The objective of the *Philosophie* is to replace specific techniques of the "cycle" analysis in favor of a general theory of economic conjunctures. The mechanisms and techniques of modern cycle theories are, in the author's view, more preoccupied with building mathematically coherent schemes than with the explanation of facts—more concerned with a rigorous logic than with a rigorous epistemology. By setting his book philosophically and logically apart from the main body of such theories, the author seeks to formulate a theory of unfolding economic "reality."

The central doctrine of the *Philosophie* is an "economic monism" which rules out conceptual and methodological dichotomies between statics and dynamics, short and long periods, real and monetary analyses, and even economic and noneconomic variables. The theory of economic conjunctures is a theory of the combination of *all* circumstances and movements that determine an economic order.

The fundamental postulate of this theory is that all economic movements emanate from specific human actions but tend toward certain equilibrium. Human actions being conscious and end-oriented, the theory of economic conjunctures is unavoidably teleological: it rejects Pareto's positivism and Keynes's "propensities."

The analytical frame of reference is found in Marshall's marginalist microeconomics (because it rejects mechanistic causation) and in Walras' theory of mutual interdependence (because of its integrative nature). But marginal analysis is considered "intemporal," and mutual interdependence too unequivocal regarding the rationalism of the adjustment process. The author's own theory is a theory of economic *successions* which allows not only for adjustment by small steps through time, but also for nonrationality of human behavior, political exigencies, and social aberrations.

The theory is based on three assumptions: (1) Economically motivated human actions, if left alone, tend eventually to establish a set of coherent economic relationships corresponding to the initial goals. But (2) in each successive step toward these goals, new situations and new states of human consciousness will arise to modify these relationships and to prevent their scheduled realization. Nevertheless, (3) the conjunctions of these forces tend to create an "evolving reality" in which economic relationships, though never precisely established, can be plainly recognized.

In spite of the awesome complexity involved in explaining such an all-embracing reality, the theory's analytical structure appears astonishingly uncomplicated. Statics and dynamics seem to be unified by attempting a "marginalist and temporal solution" between Say's timeless law of markets, and Malthus' instantaneous theory of effectual demand. The compromise: the equality of income and expenditure is neither definitional nor instantaneous, but tendential, allowing for appreciable latitude in the short run but only insignificant lapses in the long run.

The fusion of real and monetary analyses is also attempted in a similar fashion. In the approximation of economic equilibria, money is Keynes' "subtle device for linking the present to the future." Money allows individuals to choose not only among alternative goods at a given time, but also among alternative times for a given expenditure. Extending the "ability to choose" over time, money separates the existence of purchasing power from its exercise and thus relieves the equality of aggregate quantities, not only from their intemporal and instantaneous restraints, but also from their dualistic money-real character.

The combination of micro- and macroanalyses are resolved by the author's attempt to incorporate Say's law of markets and Walras' theory of imputation in a single "concrete context." The unifying element is found in the difference between functional and personal income distribution. The accumula-

tion of individual fortunes and the concentration of economic power due to the inequality in the ownership of factors of production tend to disrupt the equilibrium process of each successive time period, thus making the final outcome different from the scheduled norms. That is, the distribution of national product, by ever-modifying individual motivations and actions, affects the direction and magnitude of aggregate supply, demand, and income.

The combination of these real and monetary factors, in turn, determines the direction and intensity of general economic movements (i.e., secular expansion, long-term movements, and short-term fluctuations). Secular expansion reflects changes in real factors, i.e., quantity and quality of resources, organizational improvements, advances in technology. Long-term fluctuations (the Kondratieff), while basically conditioned by fundamental changes in science, politics, and institutions, are more immediately "caused" by monetary factors and political events. But whatever the conditions and causes, long-term movements are rooted in a host of complex phenomena that are not subject to a simple or specific explanation. Short-term movements (the Juglar) have no valid theory of their own; they are simply systematic and recurrent points in the process of approaching long-term equilibrium. They reflect, on the one hand, the conditioning environment under which individuals make their decisions (the *terminus a quo*) and, on the other hand, the goals toward which individual actions are directed (the *terminus ad quem*).

The book's value can be measured only in relation to the magnitude of its task. The theory of economic conjunctures admittedly lacks the precision of specific, fewer-variable models. And, as a "monistic" theory of all economic circumstances, it suffers from the same frailties and superficialities of all such manifold schemes. Yet in the midst of the intricately detailed issues with which economics copes these days, a work of the scope and orientation of the *Philosophie* is refreshingly nostalgic, and somewhat reminiscent of the intellectual interests of a Smith or a Marshall—Dupriez's own heroes.

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The Manchester School of Economics. By WILLIAM D. GRAMPP. Stanford: Stanford University Press; Oxford: Oxford University Press, 1960. Pp. viii, 155. \$4.00.

This able book is an enquiry into the intellectual content of the movement for repeal of the corn laws, which brought Britain to substantial freedom of trade in 1846. Grampp set himself a difficult task, for the movement had no over-all system of analysis unique to it, and, indeed, had no real consensus on the question why the corn laws were undesirable. The Manchester School, as Grampp defines it, consisted of five heterogeneous groups, united only by belief in the immediate repeal of the corn laws: first, businessmen who supported free trade from self-interest, hoping it would lower money wages or stimulate exports; second, businessmen who supported repeal on humanitarian grounds, believing it would increase real wages; third, pacifists, who believed that mutual interdependence would further world peace; fourth, the Philosophical Radicals, who expounded the objections of Smith and Ricardo

to protection; and fifth, the nonconformist radicals, intellectual descendants of the Clapham sect.

Much of the objection to the corn laws had a long history, particularly among the Philosophical Radicals, but the political effort at repeal was most vigorous during the existence of the National Anti-Corn Law League, founded in 1839 and disbanded upon victory. The intellectual history of the League and its sympathizers has been strangely neglected; Norman McCord in his recent history of the League¹ explicitly denied interest in the merits of free trade, and Mark Blaug in his treatise on Ricardian economics dealt with the Manchester School only incidentally.²

As a consequence of the neglect of the philosophy of the anti-corn law movement, the myth has been perpetuated that the Manchester School was a group of extreme believers in *laissez faire*, related to the classical economists perhaps as the followers of Ludwig von Mises to the Chicago School in our own time. With respect to the corn laws, the Manchester School clearly did take a more extreme line than the Ricardians. They advocated total and immediate repeal, whereas Ricardo suggested gradual reduction of the duty to minimize transitional problems. Ricardo defended retention of duty to the extent that domestic grain was taxed, and argued for tax reform as a necessary preliminary to repeal. Senior also proposed gradual repeal, and Torrens believed in reciprocity. Mild as these qualifications may seem, they were little less than dealings with the devil to the Mancunians.

On issues other than repeal, Grampp demonstrates that the Manchester School cannot be identified with particularly extreme devotion to *laissez faire*. Cobden defended granting the Bank of England a virtual monopoly on note issue through Peel's Bank Act of 1844, and favored certain restrictions on railway construction and on international capital movements. The Mancunians were united in a belief that the textile industry should not be subject to unique restrictions on its labor force, but otherwise Grampp is able to muster a variety of defenses of factory legislation on the part of the School.

The Mancunians dealt with their policy issues with a mixture of Ricardian analysis, common sense, and ordinary benevolence. Because of the lack of a conceptual scheme of their own, their thinking is far less interesting than the Ricardians' own works. Ricardian analysis loses no interest when it is wrong; in fact, it is not difficult to find writers who consider it most interesting when wrong. Thus, to some extent, Grampp's task was a thankless one; economists will never turn to Cobden and Bright for stimulation and wisdom, as they have turned for so long to Smith, Ricardo, and the lesser classicists. The fact remains that the repeal of the corn laws was one of the great victories for the sort of policies with which economists have traditionally been associated, and with which, let us hope, our profession will continue to be identified. Consequently, economists have reason to acquaint themselves with the intellectual history of this notable event, and Grampp provides them with a convenient and concise means of doing so.

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¹ *The Anti-Corn Law League 1838-1846*, London 1958.

² *Ricardian Economics*, New Haven 1958.

Schools and Streams of Economic Thought. By EDMUND WHITTAKER. Chicago: Rand McNally and Co., 1960. Pp. xvi, 416. \$6.50.

Here we have another workmanlike text for the history-of-economics course. The author, Professor Whittaker, made a name for himself some twenty years ago when he wrote a pioneering *History of Economic Ideas*, which presented the subject by tracing the history of various doctrines or topics rather than by discussing the contribution of one author after the other in chronological order. Since then Whittaker has added to his reputation by publishing scholarly texts on general economics and economic theory.

Unlike the earlier work, the new book is in the main arranged according to conventional lines. Considering the purpose of the book, this approach no doubt is the right one. At times, though, the form of the earlier "synopticon" reasserts itself, as, for example, in the case of John Stuart Mill. Mill's contributions are discussed in parts of three different chapters which deal, respectively, with "Population and the Laws of Returns," "Theories of Value and Distribution," and "The Law of Markets and the Problem of Depression," all referring to "Classical Economics after Smith."

The conspicuous features of the volume are its encyclopedic scope, its high degree of reliability, and its firm anchorage in the author's own first-hand investigations. His interpretations are eminently sound and based on a life-long familiarity with the subject. Throughout the book, attempts are made to relate subject matter to political and economic history, philosophy, and to the growth of thought in the neighboring disciplines of politics and sociology. The concluding chapters review the most recent developments in economic theory sympathetically and judiciously.

More than has been common in such texts, Whittaker draws attention to the development of quantitative analysis, beginning with John Graunt and Gregory King and later leading to Jevons, Henry Schultz, and the modern econometricians. Here as elsewhere the student is given not only barren facts and data but insight into the continuity of "schools and streams of economic thought." Thus the title of the book articulates what constitutes its unique character. The ever-present awareness of continuity is no doubt the happy result of the author's earlier concern with the development of individual doctrines and ideas. While the outward form of the volume no longer reflects this approach, it nevertheless suffuses much of its internal structure of thought.

This is indeed a worthwhile addition to the recent crop of useful texts for the history of economics. But, alas, as this crop is gathered, more and more colleges are following a policy of retrenchment in providing instruction in the course for which the texts are designed. It is to be hoped that the superb teaching aids which are now available in the form of texts by Bell, Lekachman, and the author of the book under review will attract the superb teacher and deliver the history-of-economics course from the more and more frequently found catalogue entry: "not given this year." At a time when there is a growing and vigorous interest in the history of the natural sciences, when strong efforts are made to humanize these sciences, economics can ill afford to sever its last, tenuous link with the humanities.

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Economic History; Economic Development; National Economies

Technischer Fortschritt und Produktivitätssteigerung. By HANS KRIEGHOFF.
Berlin: Duncker & Humblot, 1958. Pp. 151. DM 16.60.

The present volume is a doctoral dissertation at the Johann Wolfgang Goethe-Universität in Frankfurt a. M. Part I is devoted to a primarily micro-economic study of the concept of technological progress. Following Hicks' *Value and Capital* the concept is first defined in a marginalist framework. Following Dorfman and Koopmans the concept is then redefined in a linear-programming and activity-analysis framework. Here, technological progress is simply defined as the adoption of a more profitable program. Whether the new program was feasible all the time but not used previously because relative prices did not justify its use, or the new program represents a wider technological horizon, is immaterial. Krieghoff's defense is partly semantic. A more important defense is that one cannot observe the widening of the technological horizon statistically. Statistically one can merely observe the manifestations of such widening in input and output behavior.

In Part II Krieghoff examines technological progress and productivity for the economy as a whole. Productivity is traditionally defined as the ratio between output and input. To clarify the numerator and the denominator of that ratio Krieghoff distinguishes between three categories of economic goods. First, final goods leaving the production process as its net output. Krieghoff calls this category *P*. Second, intermediate goods produced and used up within the production process. Krieghoff calls it *K* (*Kapital*) and lets his capital include the not very significant land. Third, original goods not produced but used up in the production process. This category includes labor and is called *A* (*Arbeit*). Ricardo, of course, would have proceeded differently. To him, labor was reproducible at constant cost and would thus have belonged to category 2. Land would have been very significant and would have constituted category 3. But let us return to Krieghoff. There are now three alternative productivity concepts, he says, i.e., $P/(A + K)$, P/K , and P/A . The first two are dismissed on the grounds that capital (*K*) is not scarce in any absolute sense; it is reproducible within the system. Krieghoff concentrates, then, on the last productivity concept P/A as the theoretically relevant and the empirically most varying one. He studies labor productivity as the product of capital intensity and capital productivity: $P/A = K/A$ times P/K , and he quotes available time series by Kuznets for the United States and by Kregel for Germany. The familiar distinction between capital-deepening and capital-widening is made and familiar conclusions drawn.

Only on the last 25 pages of his book does Krieghoff get around to practical problems of measurement. Some, but by no means all, of the pitfalls of aggregation are mentioned, and ways to avoid them are described. The closing pages of the book are devoted to a very brief study of labor productivity in West Germany. Using differential-calculus notation the argument can, I think, be reproduced very briefly as follows: Disregarding technological progress one might assume a simple proportionality to exist between output *X* and input *x* of the form $X = ax$, where *a* would be a constant productivity coefficient. But under technological progress *a* itself will change. Dating all our

variables we would get a dynamic relationship of the form $X(t) = a(t)x(t)$. Take the derivative of this function with respect to time:

$$\frac{dX(t)}{dt} = a(t) \frac{dx(t)}{dt} + x(t) \frac{da(t)}{dt} . .$$

The first term indicates that part of the increase in output over time which is due to the increase in input. The last term indicates that part which is due to the increase in the productivity of input. For the German Federal Republic during the period 1950-1956 Krieghoff finds the total increase in industrial output to have been 93 per cent, of which 41 per cent was due to the increase of the labor force, and 52 per cent to the increase in the productivity of labor. The annual increase in productivity was 5.7 per cent, as compared with 3.9 per cent for the United States.

In his dissertation, Krieghoff has demonstrated his familiarity with the most important theoretical and empirical literature in the field, German and non-German alike. To Anglo-Saxon tastes there is too much semantics in the book. But the productivity concept and the concept of technological progress have also been examined from a statistical point of view. This emphasis on operability is hardly as obvious in Germany as it would be in the United States. If Krieghoff does not contribute anything new, he does at least provide a lucid introduction to the subject, blending theory and measurement well.

HANS BREMS

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The Economic Development of Communist China 1949-1958. By T. J. HUGHES and D. E. T. LUARD. Issued under the auspices of the Royal Institute of International Affairs. New York: Oxford University Press, 1960. Pp. viii, 223. \$3.60.

For those who have been looking for a short, nontechnical, but objective account of Communist China's economic development from 1949 through 1958, the volume under review is by far the best answer. It is a straightforward factual account, with no application of tools of economic analysis. However, facts are well chosen and presented with fairness, and the comments show well-balanced judgment. Both authors formerly worked for the British Foreign Office, and one (Hughes) also had experience with the British Treasury and the Ministry of Economic Warfare.

The book starts out with the background of the Chinese economy, the economic objectives of the Chinese Communist Party, and the period of rehabilitation from 1949 to 1952. The second part deals with the first and second five-year plans, including a succinct account of the development of the planning machinery and technique since early 1950. In their appraisal of the results of the first five-year plan, the authors have found that achievements in the industrial field were uneven, and that "the increase in agricultural production was not only certainly below what it should have been to keep pace with the increase in industrial production; it was probably barely enough to maintain the living standards of the rapidly increasing Chinese population, let alone to provide a surplus for export on the scale required" (p. 57). In 1958

the people's communes were developed, not on ideological grounds, but "in response to immediate practical requirements" (p. 69). On the subject of Soviet aid, the authors observe that by virtue of its significant contribution to the Chinese industrialization effort, the Soviet Union has occupied a commanding position in the Chinese economy, and the Chinese might welcome the prospect of some alternative source of capital to increase their bargaining power, especially over prices, although such an alternative source does not seem likely to be available in the foreseeable future (pp. 78-79).

The third and fourth parts treat the transformation of Communist China's economic framework. The authors maintain that the extensive development of communications is as much for political as for economic reasons, since it serves to merge the various parts of the country into a single integrated and organic unit (p. 109). The methods used and the problems encountered in the process of socializing private businesses and agriculture are traced step by step, with the conclusion that the transformation "was apparently brought about, not mainly by physical coercion or outright expropriation, but by the application of the strongest possible political pressure short of compulsion, coupled with almost unchallengeable financial inducements and penalties" (p. 95). The effect on agricultural production is taken up together with the official agricultural development program. According to the authors, the prodigious growth of population will greatly affect the achievement of the program goals as well as the country's capacity to export, and has already compelled the government to control private consumption since 1953. Another change in the economic framework is the organization of labor into unions which operate as agents of government policy and therefore do not have much to do with the development that "materially the industrial worker is probably in many ways better off than in earlier times" (p. 120).

In the last part it is pointed out that "the prospects for the future development of the Chinese economy depend largely on the successful limitation of the population's growth, on the capacity of the Chinese to acquire and develop modern industrial techniques, and on the success of the Government in holding in check the demands of the consumer while the capital resources of the country are being built up" (p. 207). The concluding note is that the Chinese economy will probably "continue to develop at an impressive speed" (p. 204).

Much of the authors' observations will most probably receive general agreement. The factual presentation, however, is liable to be criticized for certain inaccuracies. Anyone familiar with the Chinese scene will be quick to point out that two of the names of the so-called "four families" are completely miswritten (p. 83). But mistakes of this type are rare and insignificant. More serious are those errors of fact arising from the authors' reliance, for source data, on the *People's Daily* (the official newspaper of the Chinese Communist Party and government) and Peiping's English-language materials. For example, the statement that "according to official figures, the balance of imports and exports has not been in deficit since 1950" (p. 125) is taken as fact, whereas official data do show an annual deficit from 1950 through 1955. Land is said to have been "normally reclaimed, as in Russia, by state farms" (p. 167),

but actually in China the latter were responsible for only a little over one-quarter of the total area reclaimed during the period of the first five-year plan. Evidence is available to affirm that agricultural production is recorded in terms of "biological yield" rather than amount harvested (p. 165n). While the authors are careful to indicate that the 1958 statistics used in the volume were preliminary data, they have not mentioned that virtually all the 1957 data employed were also early (official) estimates and not final figures. Such defects, however, by no means detract from the value of the book as a general balanced account of Communist China's economic development.

CHOH-MING LI

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Economic Fluctuations in England, 1700-1800. By T. S. ASHTON. Oxford: The Clarendon Press, 1959. Pp. viii, 199. 21s. (U. K. only.)

The study of economic fluctuations for any but the most recent periods bears scant resemblance to modern business cycle analysis. Not only is there a paucity of reliable and relevant quantitative data—or of any quantitative data at all—but the structure of economies was so different as to present fundamentally different problems. The term "cycle" is scarcely appropriate for the eighteenth century, but the ups and downs in economic activity were no less pronounced than in more recent times. Professor Ashton has investigated meticulously almost all conceivable sources of these fluctuations: changes in the weather, which accounted not only for seasonal variations but, through their influence on agriculture, for annual variations which propagated themselves throughout the economy; the effects of the frequent wars on finance and commerce; the rise and fall of activity in the building trades and public works; and the influence of financial crises. In a final chapter he brings together all these various movements to establish the over-all general pattern of economic fluctuation. He finds that, as compared with the nineteenth century in particular, the fluctuations were of greater frequency, shorter duration, and had more abrupt turning points; the evidence does not permit a comparison of amplitudes. To account for the pattern he stresses the overwhelming importance of agriculture, the imperfection of markets for commodities, labor and capital, arising chiefly from inadequate facilities for transportation and communication; the inexperience of bankers and entrepreneurs; and—a familiar theme to readers of Ashton's other volumes on the same general period—the evils of the usury laws in preventing the interest rate from performing effectively its "natural" function as a governor of economic activity. (For a judicious critique of the latter argument see H. J. Habakkuk in the *Economic History Review*, April 1956, pp. 434-36.)

Ashton has done well to present his account in terms intelligible to economists, but like most good historians he has been mainly concerned to depict and explain historical phenomena in their own terms. The book, which took form initially as the Ford lectures at Oxford in 1953, may be regarded in some respects as a supplement to the author's *Economic History of England: The 18th Century* (1955). Although it integrates a surprising amount of quantitative data and some elementary economic theory into the analysis, the method

resembles that of the annalist more than that of the econometrician. In keeping with the tenor of his other works, Ashton stresses the continuity of economic change and plays down the "revolution" in industry in the second half of the century. He seeks to reconcile the indubitable growth of aggregate income and capital with the widespread contemporary (as well as more recent) accounts of hardship and privation by pointing out that examples of the latter occurred most frequently in periods of depression. But in so doing he neglects a factor which he has himself insisted upon previously, the rapid growth of population and urbanization. Moreover, he fails to take into account the possible effects of the distribution (or redistribution) of income, overlooking J. S. Mill's observation in the mid-nineteenth century that it was "questionable if all the mechanical inventions yet made have lightened the day's toil of any human being."

On the whole, however, there is little to criticize in this volume. For reasons already alluded to, it will be of little direct value to business-cycle theorists but, one may hope, will be consulted frequently by social and political as well as economic historians of eighteenth century England.

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Deutsche Volkswirtschaftliche Gesamtrechnung und ihre Lenkungsmodelle 1949-1955. By W. G. WAFFENSCHMIDT and a research team. Stuttgart: Gustav Fischer Verlag, 1959. Pp. 200. DM 14.80.

Here we have probably the first econometric macro-model of the German postwar economy. As such this study cannot fail to attract attention. While it suffers from severe shortcomings—an almost exasperating paucity of data and a statistical expertise that is not good enough for the formidable task—the descriptive substratum is bared, and one cannot out of hand preclude the possibility that the findings will by and large be corroborated by future more sophisticated studies.

First, the authors take annual macro-data (as published by the *Vierteljahreshefte zur Wirtschaftsforschung* between 1949 and 1956) and compute straight least-squares estimates of structural parameters in several linear single-equation models: They regress (undeflated) consumption expenditures of households on household income and on a price index for consumer goods; private investment outlays on the social product of the preceding year and on current profits; government expenditures on government revenues; etc.—all in all over a dozen equations.

In each case, regressors are chosen once for all, without scrutiny of alternative regressor combinations, their relative economic plausibility, and their respective residual variances. Apart from point estimates for the regression coefficients, the authors content themselves with standard errors of estimates as criteria for judging the results. They use uniformly seven degrees of freedom (the number of years of observation), rather than subtracting one for each independent parameter estimate, as standard texts counsel. For equations where they use two explanatory variables (i.e., estimate three parameters) the customary procedure would leave them with just four degrees of

freedom and consequently much larger standard errors than stated. Thus, e.g., in the case of the private-investment function, the standard error of estimate is likely to be about 4.5 billion D-Mark, not about 3.4 billion as quoted; and given the order of magnitude of investment (always below 18 billion during the period in question) a 25 per cent error margin, roughly, is hardly an improvement over casual qualitative appraisals. Likewise the authors estimate *five* parameters from the seven observations for their macro-production function, although not without scruples. Suppose the authors' standard-error definitions were appropriate. Then one could "recommend" taking six explanatory variables throughout; the seven points determine the corresponding regression hyperplane so tightly that the authors' statistic would be *identically* zero! Alternatively, even with only one explanatory variable arbitrary close fits could be enforced by polynomials, say, of sufficiently high degree. But in essence these are tricks of numerical, rather than statistical, probability-model-related, analysis.

Second, the authors devise a simultaneous-equation model. They introduce four definitional equations, discard some of the previously used variables altogether, and single out others as predetermined, *viz.*, welfare payments, government investment, consumption by firms, the social product of the preceding year, and the price levels of export, import, and consumer goods. From this 16-equation model with 23 variables the authors obtain the reduced-form equations, whose parameters are estimated (apparently by least squares; annoyingly we are never told; nor is there any hint about alternative estimation procedures). The deviations from reality are too wide to satisfy the authors; the estimate of the social product of 1955 (e.g.) understates it by more than 10 per cent.

Third, the authors discuss many of the findings mainly from a national-accounting, flow-of-funds, and flow-of-commodities point of view. Partly in order to do this, they investigate several markets in terms of *Warenauftrieb* and *Geldauftrieb*. Some of their economic terminology will strike most readers as avoidably bizarre. To begin with, it is not quite clear to what extent the *Auftrieb* notion ties in with Patinkin's demand (the conventional, Marshallian, plus Wicksteed's reservation demand). But apart from that, the estimation (called "graphical approximation") of such *Auftriebe* will be found entirely unacceptable by all those who insist that problems of estimation have to be approached by first making specific the probabilistic assumptions that underlie the model, instead of proceeding mechanically, with purely formal criteria. For one thing, Waffenschmidt's method, somewhat too involved to be expounded here, seems to beg the question about the constancy of the relevant slopes. For another, it sidesteps rather than solves the problems well known from Elmer J. Working's classical paper "What Do Statistical 'Demand Curves' Show?" (*Quart. Jour. Econ.*, Feb. 1927, 41, 212-35.)

This, then, is rather an unusual research report. While from a statistical point of view it is unfortunately impossible to herald it as a landmark of quantitative economics, one must, on the other hand, respect the hardihood it took to embark on such a project—with seven points of observation during a period of rapid change. But even extreme partisans of Waffenschmidt's

methodological preferences will perhaps concede that the whole presentation could have gained in appeal and persuasiveness by (a) resolutely conforming with contemporary statistical usages—e.g., interspersing a word here and there on such matters as the likely effects of presumably highly correlated error terms—or (b) pointing out precisely what has motivated departures from such usages and from well-reasoned strategies of recognized masters in the field; (c) including at least a few references to standard sources and to comparable endeavors by other researchers; (d) more austere editing; (e) better graphs with better legends; (f) an index.

Since the term *Lenkungsmodell* somehow suggests direct policy implications, it should be mentioned in closing that the authors expressly and wisely disclaim any “immediate, practical” applicability of their findings.

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Gezira: A Study of Development in the Sudan. By ARTHUR GAITSKELL.
London: Faber and Faber, 1959. Pp. 372. \$5.95.

The Gezira plain is a broad area 200 miles long and 80 miles across (about half the size of South Carolina or a little larger than Taiwan) lying between the White and the Blue Nile in the heart of Sudan. At the turn of the century, it was a hard land with few trees to break the monotony of the flat, dusty plain, supporting only a sparse population from the crops that were cultivated during the three months of the year during which rain fell. Fifty years later, it was a tremendously productive area, with an irrigated area of a million acres, about a quarter of which was planted in valuable long-staple cotton with a market value of over \$45 million. A greatly increased population now enjoyed a standard of living that would have been thought unthinkable fifty years earlier, at a time when the way of life in this barren area had probably not changed appreciably since Biblical days.

The economic development of this area over a relatively short period provides valuable lessons to a world that is deeply concerned with the problem of raising the living standards of backward peoples. The story and the lessons have been told in a recent book by Arthur Gaitskell, who was intimately concerned with the development of Gezira for over 30 years. This detailed and thoroughgoing study provides the stuff against which many an assumption and theory of development can be tested, and it seems likely that many of those so tested will emerge considerably modified.

The Gezira project was what today would be called a joint venture. There were three partners in the venture: (1) private entrepreneurs who put up the operating capital and managed the cultivation and marketing of the crop; (2) the government of Sudan, administered by the British, which provided most of the fixed investment, largely by floating foreign loans; and (3) the tillers of the soil, who operated as share tenants. The project required a heavy fixed investment in a large storage dam, irrigation canals and transportation facilities. It also required a sweeping land reform which made it possible to introduce efficient use of valuable irrigation water and other large-scale farming techniques. The unique feature of the land reform was that no property was

confiscated. The title to the land was left with its original owners, but they were required to lease it to the government at a reasonable rental for a period of 40 years. The land was divided into regular plots which made for efficient management of the irrigation process, and the plots were rented to the cultivators on a share basis. Landowners were permitted to take up such tenancies as they and the members of their family could cultivate. Inefficient cultivators were subject to dispossession by the management. The tenant rights could not be mortgaged, and it was provided that no debts except those incurred for labor performed could be secured by a legally enforceable lien against the crops.

Proper use of the precious water required a regimen totally new to the Sudanese cultivators. They had to follow a strict schedule of planting, weeding, watering and harvesting. It was the duty of the management, which was a private company, to see that all the tasks were performed and keep the operation going smoothly. This required a substantial force of field men to see that things were done properly. At the same time, the tenant had considerable freedom, and his success depended to a large extent on his own efforts. Unlike cooperative arrangements, the profits were not pooled, and so to a large extent success or failure depended on how well the individual managed his own affairs, rather than on how the entire group performed.

However, the individual tenant did get benefits that large-scale operations can bring. The plowing and spraying were done by machines provided by the company. Research, which was of crucial importance since plant diseases threatened for a time to overwhelm the entire program, was provided by the company. Credit to finance each stage of the farmer's operations was also made available to him on a systematic basis and at low cost.

Although the scheme has proven enormously profitable, Gaitskell emphasizes that the profit motive was not allowed to outweigh the interests of the native population. These were carefully safeguarded, though Gaitskell thinks in retrospect that not enough was done to see that their cultural development kept pace with their economic progress. Materially the lot of the people has been tremendously improved, and the country is economically viable.

It is unlikely that this would have come about without the private capital and personal spirit of enterprise which were responsible for beginning the project and advancing it against great difficulties. In reading Gaitskell's account, one can't but be impressed by the importance of the commercial motivations of the management in the over-all success. On the other hand, participation of the government was of vital importance, for it is doubtful that sufficient private capital could have been raised to carry out the project without granting or selling large tracts of land to foreigners. This would have run counter to the desire to safeguard the rights of the Africans and avoid the growth of serious social problems.

According to Gaitskell, high among the reasons for the success of the Gezira project was the policy of making haste slowly. Valuable experience was acquired by means of pilot projects, which took time but which in the end saved both time and money and perhaps averted complete failure. Gaitskell

concludes that "the establishment of equitable and practical principles of development is more important than the pace." This is worth considering these days when there is such a great temptation to throw huge sums of capital into doubtful projects merely because time is considered to be the element that needs to be most economized.

Gaitskell believes that some things ought to have been done differently; for example, he thinks an equalization scheme to even out the fluctuations in the year-to-year profits of the tenants would have been desirable and that progress might have been steadier if the high profits of the good years had been partly carried over to supplement the returns in the bad years. A few years of exceptionally high profits followed by a fall had unsettling effects on the cultivators. He does not believe that it would be feasible to meet this problem either by trying to stabilize the price of cotton on the world market or by diversification of production in Gezira. The former would be unworkable and the latter would run counter to the strong desire of the cultivators to maximize their earnings. No crop that could be grown in Gezira approaches cotton in profitability, and the possible safety that diversification might bring has little appeal when weighed against the loss of income that it would involve year in and year out.

The experiences gained in the development of the Gezira plain could be studied with profit in many countries of the world. One is tempted to agree with a Pakistani observer, Sayed Mohammed Afzal, who, hoping that it might prove to be a useful example for Pakistan and India, described the Gezira scheme as "one of those outstanding experiments on socio-economic problems of the current century which . . . deserves to go down in history as a great romance of creative achievements."¹

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Italy in International Cooperation. By KAREL HGLBIK. Padua: CEDAM, 1959. Pp. xv, 158. L. 2,000.

Mr. Holbik has written a stimulating analysis of Italy's postwar economic policies with particular attention to her international trade. It is an exciting story, which starts with an excellent account of Italy's economic difficulties, such as shortages of almost all minerals except sulphur, mercury and zinc (the supplies of which are great enough to allow exports), the important role of the government in the economy, and the tragic effects of fascist economic policies. Italy makes up for lack of fuel by extensive use of hydroelectric power (30 per cent of all energy consumed) and natural gas (methane). The per capita income, 40 per cent higher than 1938, was only \$428 in 1957. Venture capital is scarce and internal funds are expensive. Interest on bank loans averages 10 per cent. These factors combined with cheap labor have retarded the mechanization of industry. Chronic unemployment (10 per cent of the labor force) and excess population are a constant problem. The labor

¹ Quoted in *Gezira* from an article entitled "A Note on the Gezira Scheme, Anglo-Egyptian Sudan," *Indian Cotton Growing Rev.*, April 1949.

force lacks sufficient skilled labor and many workers are "underemployed." Outside capital (especially U.S. investment in oil and natural gas), Export-Import Bank loans, and World Bank loans have helped the economy. Another difficulty is the underdeveloped South with a per capita income of only \$175 per year. The *Cassa per il Mezzogiorno*, a special fund for development of this region, set up by the government, has brought some improvement.

Under the fascist regime, big business became monopolistic and has remained so. The Institute for Industrial Reconstruction (IRI), a government holding company started under fascism, still controls large segments of the economy. The fascist regime aggravated the structural weaknesses of the Italian economy, "a masterpiece of economic and financial lunacy" (Gaetano Salvemini, p. 15), by such means as the battle of the wheat and the revival of mercantilism. These policies led to a sharp decline in the standard of living.

Holbik believes, along with many other observers, that Italians have made their psychological and economic recovery from the war more quickly than some other Western European nations—to some extent because Einaudi brought back the policies of free trade and economic liberalism to Italy. Immediately after the second world war, the Italians were faced with severe shortages of coal and a decline in agriculture (wheat production was down from 75.5 million quintals in 1936-39 to 46.7 in 1947). Finance minister Einaudi carried out a most effective currency reform in 1947. High production costs had resulted from monopolistic practices, made worse by high labor costs, such as the law "blocking" workers in certain industries. Social security costs have increased threefold over 1938. Fluctuating exchange rates were adopted in November 1947. Coupled with effective internal controls this measure gave the Italians the chance to follow an "independent" monetary policy. As time went on, Italy increased its imports of foodstuffs and raw materials and decreased its imports of manufactured products, while exports of manufactured products were greatly increased. In 1946 and 1947, trade with the United States was a major factor (63.2 and 56.7 per cent of imports). This was a temporary expedient, since Italy's best export markets were still in Europe.

The Marshall Plan helped tremendously. As a member of O.E.E.C., the Italian government had to free import trade from quotas. Even before the European Payments Union was instituted, trade was facilitated by intra-European payments agreements. After 1948, Italy ceased to depend on the Western Hemisphere and turned to European countries which were recovering rapidly. West Germany resumed her traditionally prominent place in Italian trade in 1949.

The real solution lay in intra-European cooperation on a liberal basis. Imports poured into Italy because of the European Recovery Program. She used her industrial capacity more completely and net investment went up by 25 per cent from 1950 to 1951. Manufacturing production rose from 107 to 135 (1938 = 100). Along with these improvements tariffs, duties and quotas were reduced. Exports to the European Payments Union area increased and trade with the dollar area declined.

A new tariff law was enacted in 1949 to protect high-cost Italian industry. The average rate was 24 per cent. This was reduced by 10 per cent in 1951, to encourage imports and to reduce her credit balance with the European Payments Union. The Italians reduced their tariffs at the General Agreement on Tariff and Trade conference by about 28.5 per cent, covering about 50 per cent of all imports. At the Torquay conference, Italy made important bilateral agreements with 14 nations, including Germany, Austria and the United States. Imports in 1950 were higher than 1938 by 150 per cent; exports up by 120 per cent. Tariff reduction actually had little effect on the growth of exports, since the difficulties of Italian exporters were due not so much to foreign competition as to the high prices of their products. Import liberalization brought a rapid expansion which continued up to 1956.

The Schuman Plan has aided the Italian economy tremendously. Crude steel production in 1956 exceeded that of 1937 by 180 per cent. The basis of the plan was a more rational division of labor, removal of traditional discriminating practices and a decline in economic nationalism. The changes included greater use of natural gas; financial help from the community to modernize the Sardinian coal mines, toleration of monopolistic practices by Finsider, etc. Italy's dependence on U.S. coal has ended. Twenty-five hundred workers were dismissed in Sardinia, but were retrained or hired to work in foreign mines. Marginal concerns were forced out by technological improvements. This led to the discharge of 8,000 workers in 1953. The Plan helped finance their readaptation with a loan of 3 billion lire.

The lira has maintained an exceptionally stable value when compared with other currencies. The EPU helped in this stabilization program through the multilateral foreign exchange arbitrage, set up among several European nations in 1953, which Italy joined in August 1955. Even under this system Italy's "invisible trade" in convertible currencies provides essential balancing elements.

Holbik believes Italy has attained considerable internal economic growth and not only has expanded her foreign trade but has also recovered an important position in the Western European economy. Since Italy has a rather rigid system of internal prices, Holbik says she has attained a "pseudo-liberalism" in foreign trade.

As a postscript to Holbik's comments, Italy finds itself with a larger gold and foreign-exchange reserve than the United Kingdom (Spring 1960). Production is up 10 per cent over 1958, while exports are up 12 per cent. Exports to the United States are up sharply (40 per cent over 1958), as well as to the Common Market nations. Unemployment is a little lower, but still substantial in the South.

This volume adds a notable contribution to postwar economic history and analysis.

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Industrial Complex Analysis and Regional Development: A Case Study of Refinery-Petrochemical-Synthetic Fiber Complexes and Puerto Rico. By WALTER ISARD, EUGENE W. SCHOOLER, and THOMAS VIETORISZ. New York: John Wiley and Technology Press, Massachusetts Institute of Technology, 1959. Pp. xvii, 294.

Given an island rich in cheap labor but poor in most other resources, how is the regional analyst to choose among the many plausible industrial complexes which could be developed on the island for a given level of investment? Such, very broadly, is this book's ultimate problem. The island in question is Puerto Rico. Observing that Puerto Rico's small over-water distance from abundant Venezuelan oil is itself an important additional resource for the island, the authors narrow the problem to a comparison of a number of alternative complexes based on petroleum. Since still another resource is the island's duty-free access to U.S. mainland markets, each complex studied is assumed to send its product to the mainland. The proposed Puerto Rican complexes are to be compared, moreover, with respect to their advantage (in net revenue) over similar projects efficiently located on the mainland. The finding of several complexes for which Puerto Rico has a significant locational advantage would be an important step towards the final choice of good complexes.

Each of the complexes studied comprises three sets of production activities—oil refining, the production of petrochemicals, and the production of synthetic fibers. Since there are economies in spatially integrating petrochemical and synthetic-fiber facilities with a refinery, a study which amputated these facilities would, so the authors contend, be much less useful.

The comparison of alternative complexes proceeds as follows. An activity matrix describing the technology is compiled. Its rows correspond to primary, intermediate, and final commodities other than labor and equipment. Its columns define the proportions in which these commodities enter, or emerge from, a number of linear and additive processes (activities). They include (1) six different refinery prototypes; (2) a group of processes (including a number of alternative processes yielding identical outputs) which use the refinery gases and several exogenous materials to produce eighteen chemicals, some salable as such and others to be used in making synthetic fibers; (3) processes which use some of the eighteen chemicals to produce nylon, orlon, dacron, and dynel.

The description of the technology is next enlarged to include labor and capital. At "unit" level a given process requires a certain number of dollars of equipment and of man-hours per year—either man-hours of skilled labor (assumed to be needed in chemical and refining activities and to be imported into Puerto Rico as needed) or else man-hours of Puerto-Rico-type labor (assumed to suffice in synthetic-fiber production). At the level λ the given process requires these quantities of capital and labor multiplied, respectively, by λ^α and λ^β , where α and β lie between zero and one and are constants unique to the given process. For each process such smoothly decreasing labor and capital costs are assumed to hold for those values of λ which define the several complexes to be compared.

The next step in the procedure is to choose from the infinitely many possible complexes (each defined by a set of process levels lying in the decreasing-labor-and-capital-cost ranges) an interesting small collection to be studied. Several constraints are imposed on the choice: (a) Each process level in the complexes chosen is to be above a minimum level below which the process is not "economically feasible." (b) The refinery process in all the complexes chosen is to be the same one out of the matrix's six possible prototypes. (c) The refinery's crude capacity is to be the same in all complexes, as is the total synthetic fiber output; these capacities are to be "moderate" ones, common in mainland complexes. (d) Each complex is to contain only one of the alternative processes yielding a given output.

The 28 complexes chosen for study meet these constraints but differ with respect to the scale of a number of the chemical processes and the relative output of the four synthetic fibers. They are described in detail. With the aid of a number of assumptions, for which careful arguments are made, the costs and revenues of each complex are then calculated—for Puerto Rico and for the "best" mainland location (which is convincingly argued to be the Gulf Coast). All 28 programs turn out to have a net advantage for Puerto Rico—the cheapness of local (synthetic-fiber) labor outweighs Puerto Rico's transport disadvantages.

The 28 comparisons are next subjected to "differential-profitability corrections." These patch up the comparisons to allow for the fact that it would not be efficient exactly to duplicate on the mainland a given Puerto Rican complex. For a given Puerto Rican complex, the authors estimate the production costs of a mainland complex yielding the same output quantities but using, for this purpose, the petrochemical processes which are judged to be "best" on the mainland, operated at the scales which achieve "minimum," "moderate" or "maximum" economies. The three scale assumptions yield three alternative amounts by which the previously calculated advantages of the given Puerto Rican complex are to be reduced. The effect of the corrections is drastic. Under the maximum-scale assumption none of the complexes has a Puerto-Rican advantage and under the other scale assumptions only two or three. The study concludes by considering some "reduced" programs (in which important intermediate materials are imported into Puerto Rico). Some of these do appear to have a Puerto Rican advantage.

The drastic patching-up procedure raises the question why not even an exploratory attempt at linear programming was made in the study. Curiously, the authors briefly dismiss the possibility, blaming not only the "insufficiently developed" state of the art but asserting as well that "pressing" resource limitations are the major justification for a linear programming approach and that these are unimportant for industries which might have a Puerto Rican advantage. The best reason for not considering linear programming is surely not this puzzling claim but is rather the difficulty of incorporating decreasing costs (labor and capital) into a linear programming model. (It is only recently that integer programming has offered hope of doing so—see G. B. Dantzig, "On the Significance of Solving Linear Programming Problems with Some Integer Variables," *Econometrica*, January 1960.) If it were not for this difficulty, a

linear-programming model could yield (possibly at great computational cost) all the answers the authors want and more. It could yield, for example, the mainland and Puerto Rican complexes which maximize net revenue for a fixed investment cost. No numbers other than those the authors have painstakingly gathered (the activity matrix, the input, output and transport prices) would be needed; the constraints (a)-(d) and the cumbersome patching-up procedure would be avoided. The model would automatically work out the complicated dependence of optimal process choice on location. Perhaps these advantages might have made an exploratory relaxation of the decreasing-cost assumption worthwhile.

The study is, in any case, a useful and ingenious product of the tools now at hand. Its limitations make one wish the more fervently for the day when less restricted tools are cheaply available.

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Economies des démocraties populaires—les régions économiques en U.R.S.S.

Paris: Institut de Science Économique Appliquée, 1959. Pp. 111. 1,000 fr. [pre-reform rate].

Economic Regions in the USSR, No. 5 in a series of studies on the Economies of the Peoples' Democracies, is a translation into French of four articles from recent Soviet Russian periodicals, and a brief comment on one of them, the whole being introduced by a short well-footnoted survey of their significance by Henri Chambre of the Sorbonne.

While each of the articles is an independent entity, the first three illustrate the development of responsible Soviet thinking preceding the decree of May 1957 which decentralized economic organization in the USSR to some extent in the interests of local flexibility and more efficient national growth, it was hoped. The articles are as follows: "On Economic Regionalization and on the Complex Development of the Economic Regions of the USSR," by V. Kostenikov, in *Kommunist*, 1955, No. 14; "The Distribution of Productive Forces in the USSR," by J. Feygin, also in *Kommunist*, but dated September 1956; and "Division of the USSR into Economic Regions," by P. Alampiev, in *Planovoye Khozyaistvo* (The Planned Economy), 1956, No. 6. The two-page critique of the last article is from the same periodical, No. 2, 1957, by A. Danilov, and favors maintaining the economic integrity and individuality of the various republics instead of grouping some and dividing others for the purpose of economic planning.

The criticism by the first three authors of the system of regions then existing in the USSR changes, over the two years, from the rather tentative to the quite specific—but the writing is neither virulent nor dogmatic, as so often in the daily Soviet press. Contemporary failures in production and distribution are attributed, pragmatically, to out-of-date regional concepts, but the suggestions to future planners tend to be innocuous, such as a recommendation that planning be around the idea of a "key industry" in each region, and

that "regional material balances" not be disregarded. All the authors see the problems but fail to come to theoretical grips with them. Fundamentally they all agree that no scientific theory has been developed for guiding rational regionalization, and Feygin particularly criticizes the Academy of Sciences for concentrating on descriptive spot studies instead of establishing sound bases of economic geographical theory as a foundation for regional planning. But none of the authors proposes anything that would startle Adam Smith. In fact, the discussions labor the points of the relative efficiencies of centralized exploitation of natural resources for national commitments versus the uneconomic costs of excessive transportation—an old problem in all large countries. Uneconomic cross-haulages are a main target of criticism, and stress is laid on the need for regional self-sufficiency in ordinary consumer goods and producer goods for local industry (i.e., scaffolding, carpenters' supplies, small motors), but not autarchy, combined with efficient selection and exploitation of regions on the basis of natural resources and energy sources in the interests of the national plan. Manpower availability is mentioned particularly by Alampiev, who emphasizes the problem of reconciling short-range plans from an existing state of development and long-range plans of desiderata in growth, with allowance for previously unforeseen developments such as the enlarged international network of "friendly socialist countries" and the coming availability of huge hydroelectric grids and atomic energy. He also points to the lack of realism in equating vast unsettled nomadic regions, only potentially wealthy, with heavily populated complex entities in the older parts of the Soviet Union.

All three authors go into geographic detail as to the possible desirable regrouping of existing regions, and the reasons therefor, and since apparently many of their specific views were adopted in the 1957 decree establishing 104 Economic Councils (Sovnarkhoz) for the new regions, they are worth examination by economists interested in this field. Their other major value to the U. S. economist is not in the imprecise and somewhat circular theoretical considerations, but in the not inconsiderable factual data scattered throughout.

Of particular interest here are the specific production data (not plans) cited in the last article, "Specialization and Cooperation in Production in the Economic and Administrative Region of Gorki," by S. Prokhorov, likewise in *Planovoye Khozyaistvo*, 3, 1958. Absolute tonnages and volumes of specific items for the Gorki district (formerly Nizhnii Novgorod), a long-established industrial center, are given by the author, instead of percentages only, so that the statistically inclined specialist may find bases of comparison.

The absence of specifically Marxist doctrine is in line with much recent Soviet writing. But the lack of a suitable accounting theory and procedure, often lamented by Soviet economists, remains, in the absence of a free price system, a main stumbling block to evolving a satisfactory theoretical standard for regionalization.

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Statistical Methods; Econometrics; Social Accounting

The Measurement of National Wealth. Income and Wealth Series 8. Edited by RAYMOND GOLDSMITH and CHRISTOPHER SAUNDERS. Chicago: Quadrangle Books; London: Bowes and Bowes, 1959. Pp. xiv, 389. \$7.50; 45 s.

In recent decades a great deal of attention has been given to national income accounts while relatively little emphasis has been placed on the measurement of national wealth. Thus we have many well-developed national income statements but few and rather embryonic national balance sheets. This most recent volume of the Income and Wealth Series presents a large quantity of data on the national wealth of various countries and quite a lot of discussion of the methodology of estimating national capital accounts.

The book presents papers on the national wealth of eleven countries: Argentina, Australia, Canada, Colombia, West Germany, India, Japan, the Netherlands, Norway, South Africa and Yugoslavia. These papers were presented at the 1957 Conference of the International Association for Research in Income and Wealth. The results of these studies are summarized in tables that appear early in the book which also include similar data, gathered from other sources, for Belgium, France, Luxembourg, Sweden, the United Kingdom, the United States and Mexico. The tables include estimates of both reproducible and nonreproducible wealth for government and enterprises (military assets are omitted, as is subsoil wealth in some cases), consumers durables and foreign assets. Each of these headings is, of course, subdivided. The dates of the estimates summarized in the tables vary from 1950 to 1956.

Introductory essays by the editors and by T. Barna deal in general terms with the concepts, methods, special problems and purposes of estimating national wealth. There is little that is original in these essays but they help the reader in forming an over-all context for the more detailed papers that follow.

An interesting section of the book deals with the possibility of introducing financial accounts into a system of national accounts. The authors of this chapter, P. J. Bjerve and M. Selsjord of the Central Bureau of Statistics of Norway, observe that in most countries financial statistics are plentiful, but have not been designed to fit into a system of national accounts. To help fill this need the authors develop an equation system that would provide a suitable structure of financial accounts. The system is in some respects reminiscent of the flow-of-funds accounts, but is more structured and more clearly related to the usual national income accounts. It is apparent that many difficulties must be overcome before the proposed system could be put into practice. Aside from any data problems, the system makes no provision for the time dimension, and it was not entirely clear to this reviewer how discrepancies in some cases between nominal and market values would be handled in the accounts.

The remainder of the papers deal with problems and results of wealth measurement in particular countries. These are pretty nearly what one would

expect. The researchers experienced difficulty in obtaining data; they split rather evenly between use of the Goldsmith perpetual-inventory method of estimation and other methods; capital coefficients and other similar ratios change too irregularly to be of much help in describing economic growth.

Two of the essays concerned with particular countries should be singled out for special comment. In the paper on Norwegian national wealth, the authors, O. Aukrust and J. Bjerke, having obtained estimates of net domestic product, total fixed real capital and total employment, regress the first of these variables on the other two and time. A linear function in the logarithms of the variables is used. They find that, for the period 1900-55, the regression coefficient is much larger for labor than for capital. Then taking the first derivatives they conclude that, since the same regression coefficients appear in the derivatives, labor is much more important than investment in Norwegian economic growth. This conclusion is not warranted on the basis of the regression they obtain. It is clearly possible for total net capital to grow while net investment is declining and this in fact occurs in the Norwegian data. Hence a much different regression coefficient would have been obtained if investment had replaced total capital as a variable. Thus the revolutionary finding of the Norwegians appears to be based on a misunderstanding.

Alexander Ganz's essay on economic growth in Latin America is noteworthy. Ganz focuses attention on Argentina and Colombia, Argentina being a relatively mature economy while Colombia is just emerging as an industrial nation. Aside from interesting historical parallels and comparisons of factual information, Ganz brings out the effect of government on economic growth. It appears that there are some excellent object lessons on the way in which government can retard economic growth to be gained from detailed study of certain Latin American nations. In Ganz's opinion the failure of government to provide adequate social capital, especially in transportation and energy, has helped create economic stagnation in Argentina in the past decade.

The economic statistician may shudder as he contemplates the likely errors in most of the estimates of national wealth. The combination of numbers racket and guessing game goes literally unchecked in forming such estimates. But the first stone should be cast by whoever believes he could do better; the reviewer, for one, stands empty handed. The fault is with the data not with the statisticians, and something should be done about this.

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Economic Systems; Planning and Reform; Cooperation

Planning for Freedom—the Public Law of American Capitalism. By E. V. ROSTOW. New Haven: Yale University Press, 1959. Pp. x, 437. \$6.00.

This book represents a revision of the William W. Cook Lectures which the Dean of the Yale Law School delivered at the University of Michigan in March 1958. Its general theme is the legal control of the economy, with

special emphasis on the economic factors and forces that today seem to call for control, and on the adequacy of the legal institutions through which control is exercised. The author, who takes merited pride in his success at expounding "economic problems to lawyers, and legal problems to economists" (pp. vii-viii), has directed this book towards a general, educated audience rather than towards specialists in either economics or law, so that its interest for readers of this *Review* will lie in the author's expository skill, his synthesis and his perspective, rather than in the portrayal of anything they will find startlingly new or unfamiliar. A two-part appendix reproduces various selected statutory provisions (from the Sherman and Clayton Acts, and the Employment Act of 1946 as amended in 1956), and various statistical tables selected from the President's *Economic Report*, 1959.

The three main areas where Rostow finds that public responsibility comes, or should come, into significant contact with the economic world are: (1) the volume of effective demand as influenced by monetary and fiscal policy, (2) the market for goods and services as influenced by the Antitrust Acts, and (3) the labor market as influenced by whatever form public policy toward labor may eventually take.

On legal, economic and institutional grounds the central core about which the author's entire argument turns is the Employment Act of 1946. That is so legally, because that law serves as "a new field of magnetic force" giving the laws mentioned in the immediately preceding paragraph "new dimensions, a new orientation and a new momentum" (p. 368).

The Employment Act is the focus economically, because public assumption of responsibility for economic stability, even as a somewhat vaguely formulated aspiration, underscores (1) the importance of the magnitude of effective demand; for, even though "there is no one 'true' cause of trade cycles" (p. 117), the fact that "people may spend more or less than their current incomes for current production is the crucial mechanism of business fluctuations" (p. 70), and "the most striking characteristic of the saving-investment circuit is that it is never in balance" (p. 88). Public responsibility for stability underscores (2) the importance of production as "the main long-run force working against the pressure of inflation seemingly inherent in sustained periods of full employment" (p. 25); and "the social costs of monopolistic arrangements . . . as the plausible depression arguments for monopoly are stripped away" (p. 25); and the importance of paying attention to "what happens to prices and wages and profits in the rough and tumble of ordinary business life" (p. 211). It also underscores (3) the "danger of creating a new feudal system of employment with gain of security at the cost of efficiency" (p. 256), so that a new look should be taken at restrictions on apprenticeship and at the immobilities arising out of such things as pension plans and seniority rights. Rostow would like to see a really national labor market established, and he would also like to see productivity become the norm of wage policy.

Institutionally the Employment Act is significant as providing the means by which the President, his executive officers and the Congress *may* keep

abreast of the economic state of the nation, provided the Council of Economic Advisers is sufficiently forthright: "In the competitive world of Washington bureaucracy, shy theorists have never prevailed unaided" (p. 15).

In summary, the author wishes to see more competition, less rigidity, more sustained growth and, within the limits of the possible, more acceptance of competitive market processes as determinants of the structure of production and employment. This calls for some degree of planning, but planning which is oriented about indirect rather than direct controls, since the latter, indeed, represent simply an "attempt to mitigate the effects of a failure of planning" (p. 370). It should go without saying that Rostow is more concerned to plan for a viable capitalism than to essay even the mildest venture into socialism, whose procedures, he remarks, are not only erroneous, but irrelevant to the solution of contemporary problems.

The lectures of which this book is an outgrowth must have been rewarding to hear. They are meaty, clear, thoughtful, and filled with terse obiter dicta on, among other matters, education, foreign policy, our farm and transportation problems. The book does not provide the new ideology which some are beginning to demand, but it does provide a mature and balanced, though not unfamiliar, perspective on the working relationships between government and the economy.

GEORGE P. ADAMS, JR.

Cornell University

Overcentralization in Economic Administration—A Critical Analysis Based on Experience in Hungarian Light Industry. By JANOS KORNAI. Translated by John Knapp. London: Oxford University Press, 1959. Pp. xv, 236. \$5.20.

This is definitely an important book—candid in tone, rich in insights, abundant in valid analyses. The original was written as a dissertation for the title of Candidate of the Hungarian Academy of Science. The stated aim of the work was to analyze "actual practice" in Hungarian light industry with the aim of drawing up a proposal concerning the reduction in the number of compulsory indices for enterprises, in order to give them more freedom of action. The author did however much more than that: he drew both a detailed and an all-embracing picture of the *modus operandi* of a planned economy of the Soviet type.

The work is divided into six parts, examining respectively: (a) the system of instructions in centralized planning; (b) the system of incentives; (c) the interaction of instructions and incentives; (d) relations between enterprises; (e) the consequences of centralization; (f) the attempts to develop local initiative. Kornai's main idea is that industrial policy—understood as a set of ends translated into a given pattern of investment and targets—and the mechanism of the economy—i.e. the machinery for implementing these aims—are deeply intertwined. According to him it would be illusory to expect real changes in economic performance by tinkering solely with the mechanism of

plan implementation while the aims—i.e. the underlying policy—are left unchanged. Kornai hence concludes that as long as industrial policy aims at an overambitious development postulating rapid growth at any price, the consequences will be shortages of materials, overutilization of capacity beyond the optimum point, a perennial sellers' market, and in short a command economy disregarding completely the wants of the consumer. Within such a framework, reliance on instructions rather than on a balanced system of incentives, on overcentralization rather than on decentralization, is unavoidable. While the author believes that planning is superior to the free enterprise system because of the former's assumed ability to eliminate cyclical waste, he is careful to underline that the planning system has built-in tendencies toward waste of a different nature. Kornai shows in detail how waste results from the reliance on centralized instructions often contradictory in nature, the absence of co-ordination between plans, the conflicting interests of planners and managers, and so on.

After drawing a picture which confirms point by point the Western analyses of the Soviet-type economies, Kornai finally suggests the following proposal for improvement. Let us shift, says Kornai, from a perennial situation of underproduction to a perennial "relative overproduction": in other words, let relatively ample inventories be built throughout the economy and let certain capacities be left unused so that the whole economy would be able to move from a sellers' to a buyers' market situation. Industries will then be able, adds Kornai, to exercise pressures on their suppliers and force them to produce goods according to their actual needs. In turn, the distributing agencies could exercise pressures on the consumer-goods industries and force them to produce products corresponding in assortment and quality to the wants of the consumers. The change from a sellers' to a buyers' market would, along with an appropriate overhaul of the system of incentives, make the efforts of a planned economy to eliminate waste effective and efficient.

This is the gist of the book. As a good communist, Kornai does not dwell directly on the fact that Hungary is committed to a given model of industrialization which postulates the continuous growth of military and producers' goods without considering underlying factor endowments. It is adherence to this model which forces the planning authority to seek rapid rates of growth at any price and which prevents it from moving from perennial shortages and overutilization of capacity to "relative overproduction." But Kornai's suggestion does imply the need of a shift in the pattern of investment in order precisely to secure this "relative overproduction" and achieve a surplus of consumers' goods instead of the present shortage.

Be it what may, Kornai's book is an outstanding essay in the field of centralized planning and deserves to be carefully studied by all students interested in these problems.

NICOLAS SPULBER

Indiana University

Money, Credit and Banking; Monetary Policy; Consumer Finance; Mortgage Credit

A Program for Monetary Stability. By MILTON FRIEDMAN. New York: Fordham University Press, 1960. Pp. x, 110. \$2.75.

This provocative volume consists of a slightly revised and expanded version of four lectures delivered by Professor Friedman in late 1959 at Fordham University, under the auspices of the Moorhouse I. X. Millar Lecture Series. The first chapter analyzes the role of monetary factors in the major inflations and depressions that have occurred in the United States over the past 125 years. This historical review is followed by three chapters of public policy prescriptions (and proscriptions) intended to reform our present-day monetary institutions and policies. The proposals range from such sweeping changes as the complete abolition of discretionary countercyclical monetary policy to more detailed technical points, such as the preferred way to conduct auctions for Treasury securities.

Friedman argues that the economy has been and is now inherently stable, and that it would automatically tend to maintain high employment with a stable price level if only it were not being almost continuously thrown off the track by erratic and unwise monetary policies. Discretionary countercyclical monetary policies are seen as particularly pernicious; Friedman holds that since the Federal Reserve System was established there has been considerably more instability in the stock of money than previously, and (consequently?) more instability in prices and employment. Countercyclical monetary policies should therefore be quarantined so they can no longer impede the tropismatic response of the economy toward high levels of employment with stable prices. Of course, we will still have minor cycles—after all, “these milder fluctuations have been with us for at least two centuries and doubtless will be for a long time to come” (p. 23)—but in the absence of the aggravation caused by discretionary monetary policies they will be brief and self-correcting.

Three separable although related hypotheses are discernible in the book, only one of which is convincing to this reviewer. That one is the argument that instability in the stock of money has aggravated cyclical fluctuations during much of our history. This is an important and incontrovertible thesis. Indeed, it is a significant part of the case *for* discretionary monetary policies.

Less supportable is Friedman's view that monetary factors have been mainly responsible for our major inflations and depressions. He states that “the failure of government to provide a stable monetary framework has been a major if not the major factor accounting for our really severe inflations and depressions” (p. 9). The evidence adduced is a frequently forced and largely unconvincing interpretation of economic history since the 1830's. In the process, the author completely ignores the significance of such factors as the role of innovation and technological change, the relation between investment opportunities and the volume and type of existing capital, and the cumulative and interdependent nature of many other nonmonetary economic relationships. Even the monetary analysis is unsatisfactory. There is only the most cursory ex-

amination of the direction of causation between historical changes in the money supply and changes in economic activity, although it is inferred that causation runs primarily from money to prices and employment. This is not substantiated by the evidence presented, as indeed, in this reviewer's opinion, it can not be.

Even were Friedman's second thesis true, it would still not be sufficient to justify the third part of his argument: that countercyclical monetary policies should be abandoned since they are likely to do more harm than good. To reach this conclusion, he relies on the likelihood of errors in forecasting and in judgment, and on a rather primitive analysis of time lags in monetary policy. But one looks in vain throughout the book for any systematic examination of the channels through which changes in the stock of money influence the flow of spending, or for a discussion of the cyclical behavior of velocity. Equally surprising, Friedman has almost nothing to say about the conduct of monetary policy during the years since the 1951 Treasury-Federal Reserve accord, perhaps the only period in our history when we have had a reasonably knowledgeable and flexible monetary policy devoted primarily to the attainment of domestic economic stability. Friedman may well be correct in his view that the forecasting difficulties are so great and the time lags so perverse as to make stabilizing discretionary policies all but impossible. But the evidence he presents or that is elsewhere available does not justify that conclusion; short-term forecasting is continuously improving and too little is yet known about time lags to warrant more than the most tentative of hypotheses.

Friedman's two major policy proposals are the establishment of the 100 per cent reserve plan and the replacement of discretionary monetary policies with a fixed "rule." The first recommendation would require cash reserves of 100 per cent for all institutions that accept deposits transferable by check or payable on demand, "whether nominally demand or time deposits." These institutions would not be permitted to make loans except to the extent of their ownership capital. It is not clear whether he intends to include, as the definition would imply, mutual savings banks and savings and loan associations as well as commercial banks.

There were undoubtedly good reasons for advocating the 100 per cent reserve plan in the 1930's. Despite all its shortcomings, it promised to avert any repetition of the bank runs and bank failures that had long plagued our financial system. But today virtually every one of the advantages that might have been obtained from 100 per cent reserves has already been achieved by other means. Friedman admits, for example, that open market operations as currently practiced effectively offset the impact on bank reserves of seasonal and other short-term currency drains, and that the Federal Deposit Insurance Corporation has eliminated the likelihood of bank runs. In fact, he sees the FDIC as "the most important structural change in our monetary system in the direction of greater stability since the post-Civil War tax on state bank notes" (p. 21). What, then, are the benefits to be gained from instituting the 100 per cent reserve plan? They turn out to be only two: the Open Market Committee would no longer have to engage in "continuous jiggling of the monetary tools" to offset currency movements; and we could eliminate the FDIC. We

are told that in comparison with the FDIC the 100 per cent reserve system "would achieve its objectives more effectively and with less intervention into private activities" (p. 69). In order to reap these bounties, we would have to completely overhaul the present structure of the money and capital markets and establish a financial system that would contain within it any number of unexplored problems. Prominent among these would be the source of short-term business financing, now largely provided by the commercial banks, and the problem of evasion in the classification of deposits in order to avoid the 100 per cent reserve requirement.

To eliminate the possibility of discretionary countercyclical monetary policy, Friedman proposes that the Federal Reserve be instructed to increase the money supply at a steady and inflexible rate, regardless of economic conditions. The specific "rule" would depend upon the definition of the money supply adopted: 3 per cent per annum if the money supply is defined in the conventional way as demand deposits plus currency, 4 per cent annually if time deposits in commercial banks are added in as well. However, even a fixed annual rate of increase evidently still leaves too much discretion to the monetary authorities. He further specifies that they should not permit seasonal variations in the money supply and should keep the rate of growth steady "week by week and month by month." What we have then is a .057 per cent per week rule ($= 3$ per cent per annum) or a .076 per cent per week rule ($= 4$ per cent per annum). This is advocated on the grounds that to accommodate seasonal variations in the demand for money "would involve introducing an essentially arbitrary element into the behavior of the stock of money" (p. 92).

The issue of "rules versus authorities" in monetary policy seems to me to be a false issue, and the formula recommended appears to rest on shifting sands. Obviously, as Walter Morton has put it, "a fixed rule of monetary policy, or for that matter of any policy, is preferable to administrative discretion, provided that a rule can be found appropriate to all circumstances to which it must be applied. If such a rule can be discovered, then the distinction between rules and authorities will soon disappear because any sensible authority would apply the rule whether or not it was enacted into law. The issue is whether such a rule has been discovered" (this *Review*, May 1951, p. 198).

We certainly need better guides for central bank decisions and better performance criteria than we have today. However, the proposed rule falls far short of what is necessary. It rests upon an arbitrary definition of the money supply (as any such definition must be), and is based almost entirely on the extrapolation of broad historical trends into a continuously changing economy. A case in point is the confident projection of a downward secular trend in income velocity, a trend that has been heading in quite the opposite direction since the end of the second world war. Similarly, the cyclical behavior of velocity, and its implications for monetary policy, are barely touched upon. Increases in the velocity of money have shown themselves capable of supporting substantial cyclical expansions in prices and income during recent years, despite relative stability in the money supply. There is likewise no reason to suppose that a constant rate of growth in the money supply would make the

economy immune to substantial and prolonged recessions during which velocity contracts, as it generally has during such periods.

In addition, to adopt such a rule would arbitrarily prejudge future decisions regarding the policy mix between monetary and fiscal policy, and would thereby prejudge collective decisions regarding the allocation of resources for the production of consumer, investment, or government goods and services. Such a rule, for example, would preclude the adoption of a tight fiscal policy combined with an easy monetary policy in order to channel a larger fraction of resources into the production of investment goods. Similarly, there is nothing sacred about the present level of the federal budget, and substantial changes in that level would imply substantial changes in monetary policy, changes that are quite outside the scope of any simple rule.

Friedman cites the superiority of the rule in comparison with the recent actual behavior of the money supply. The 3 per cent rule generally would have expanded the money supply more in postwar recessions than has in fact occurred, and also would have expanded it more in boom periods. If the 3 per cent rule had been adopted early in 1952, at the start of the first full year after the accord, the money supply would have risen from the \$124.5 billion that existed at that time to \$157.7 billion by the end of 1959, an expansion of \$33.2 billion over the eight years. In fact, it was \$144.8 billion at the end of 1959, an increase of \$20.3 billion or only about three-fifths as much as would have occurred under the rule. Although the rule was ostensibly designed to yield a reasonably stable price level, its application would have meant a 60 per cent greater increase in the money supply than actually occurred between 1951 and 1959, a period during which the consumer price index rose from 113 to 126 and the GNP deflator from 98 to 113.

Although this is a book devoted to economic stability, Friedman never mentions fiscal policy, secular inflation, administered prices, or cost-push phenomena. He makes no policy recommendations outside the monetary area. Nevertheless, he alleges that the adoption of his program would be sufficient to yield fairly effective insurance against major cycles and greater stability from minor fluctuations. It was Henry Simons who said, in 1936, that "the problem of booms and depressions is one which must be attacked from both sides, (a) by policies designed to give us a more flexible price structure, and (b) by measures which will minimize the aggravations attributable to the character of the monetary system and the financial structure. The former attack, however, must always be regarded as primary. The problem of industrial fluctuations cannot be solved, and should not be attacked, exclusively by monetary devices" ("Rules Versus Authorities in Monetary Policy," reprinted in the *AEA Readings in Monetary Theory*, pp. 351, 354).

LAWRENCE S. RITTER

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The Federal Reserve System. Edited by HERBERT V. PROCHNOW. New York: Harper & Brothers, 1960. Pp. xiv, 393. \$6.50.

This book is a collection of seventeen essays which discuss the varied aspects of the organization and functioning of the Federal Reserve System. One

might question the publisher's use of the word "definitive" in describing the work; but it will, nevertheless, most certainly be welcomed by those who seek a better understanding of the role of monetary policy in any stabilization program and of the part played by the Federal Reserve System in initiating and carrying out such a policy.

Virtually all the authors may be considered specialists in their respective fields and to have more than an academic interest in the subjects on which they write because of professional connections with the System itself or with large commercial banks. Despite a large number of authors there is surprisingly little duplication of material and virtually no pronounced differences of style to mar the smoothness of the presentation. Each writer has been allowed full freedom of expression, and while there are some conflicts of opinion among them it would seem to this reviewer that this should add to, rather than detract from, the interest of the thoughtful reader. The editor has done his work with skill and discrimination.

The first chapter deals with "The Historical Background of the American Banking System." The attempt to compress the story of American banking from the colonial land banks of the seventeenth century down to the introduction of the Aldrich Bill in 1912 results in an excellent summary of the developments of that period, but one which would probably be meaningful only to a reader already somewhat familiar with that history. This introductory chapter is followed by one on "The Federal Reserve Act" which outlines the organization of the System, and by a third chapter which acquaints the reader with the meaning and significance of the various items found in a Federal Reserve Bank statement.

The portion of the book likely to be of most interest to those concerned with the role of monetary policy is the section comprising Chapters 4 through 10. Chapter 4 deals with the general nature and limitations of monetary policy. The following four chapters respectively describe the various devices through which monetary policy may be made effective: legal reserve requirements, the discount rate and rediscount policy, open market operations, selective credit controls. The ninth chapter discusses the effects of Federal Reserve policy on the commercial banks, and Chapter 10, the effects on nonmonetary financial institutions.

The next three chapters are mainly of a technical nature. Chapter 11 describes the supervisory functions of the System. Chapter 12 gives us a summary, necessarily brief, of the regulations of the Board of Governors, and Chapter 13 discusses the international activities of the Federal Reserve.

The last portion of the book is devoted to an excellent summary of the history of the Federal Reserve System. Chapter 14 deals with the period from 1914-1929. Chapter 15 covers the disturbed period of the 1930's, and Chapter 16 takes us through the 1940's down to the present. These accounts are mainly factual although there are some interpretations or criticisms with which some readers might disagree. On the whole, though, the opinions expressed pretty generally reflect the consensus of students of the period.

The final chapter is a brief but adequate account of foreign central banking and monetary policy. The author points out the differences in practice be-

tween the United States and foreign countries outside the iron curtain, differences due largely to the varying "economic, financial, and political framework in which they operate."

A careful reading of this volume by any reasonably informed person who desires a further understanding of the role of central banking in our complex economic society should be both stimulating and rewarding. He should come away from it with a greater appreciation of the possibilities and limitations of monetary controls, as well as with a number of questions in his mind to which he would like further answers.

WALLACE WRIGHT

Iowa State University

A Study in the Theory of Monetary Equilibrium. By D. J. BÖTHA. Leiden: H. E. Stenfert Kroese N.V., 1959. Pp. viii, 192. f 17.

The subtitle, "A Comparative Analysis," is appropriately chosen for this work. Essentially, it is a recapitulation and synthesis of selected contributions by writers on monetary theory. The emphasis is on the role of money in an economy of high employment on the verge of, or afflicted with, excess demands and rising prices. Roughly, the analysis divides into two major areas: (1) the development of a general theory of "monetary equilibrium" which is said to have bridged the earlier gap between monetary and price theory, and (2) the comparison of several models of monetary equilibrium and inflationary departures from it.

In the general theoretical vein, after the Walrasian general equilibrium system, Wicksell receives credit as the first to analyze the economic forces underlying a monetary equilibrium and to specify the criteria for equilibrium. Wicksell went beyond most neoclassical writers, who merely recognized the existence of the real balance effect, by applying it in his analysis. Patinkin fully incorporates the effect of real balances on commodity expenditures, which supplies the connecting link between the money and commodity markets and eliminates the indeterminacy of absolute prices. In so doing, he appears to have provided a "final solution" to the dichotomy between the real and monetary spheres. Within this framework, the bulk of the analysis consists of a synthesis of Patinkin's recent treatment, and a rather detailed presentation of Wicksell's equilibrium conditions, their re-interpretation by Myrdal, and the origin of the Swedish *ex ante-ex post* analysis. Reformulations of Wicksell's second condition involve variations on the theme of equality between *ex ante* saving and investment.

The remainder of the discussion outlines a number of "approaches" to the theory of equilibrium and inflation. In a long chapter on the inflationary process, various possible aspects are exhibited in a series of models. Models on the speed of inflation include a multiplier-accelerator process, a price-wage spiral, T. C. Koopman's "discrete-spending," single-period model of inflationary speed, and a variant of this model by Bötha in which production and spending over the period are continuous. A disaggregated, sectored model, that stems from Goodwin and employs matrix algebra, permits incorporation of different

market structures and a combined demand and cost-push approach. The final chapter is an extended outline of a Dutch money-flow analysis associated chiefly with J. G. Koopmans and M. W. Holtrop. It requires a distinction between spontaneous and induced creation or destruction of money, and a similar distinction between spontaneous and induced hoarding, some very laborious concepts. Equilibrium requires equality between spontaneous net creation of money and spontaneous net hoarding, a condition shown to be tantamount to the second Wicksellian condition. This discussion explores a body of literature whose original works are, with few exceptions, available only in Dutch. The Dutch central bank makes some use of a flow analysis by sectors, suggested by this literature, but only the theoretical underpinnings are treated here. Induced hoarding the author holds to be an elusive concept, one that could present difficulties in empirical work.

But this doubt is tolerated only in passing, in keeping with the character of the book. Amidst a procession of inflationary models, one finds only occasional, rather perfunctory conjectures about their applicability to meaningful empirical work.

NORMAN V. BRECKNER

University of California, Los Angeles

A Proper Monetary and Banking System for the United States. Edited by JAMES WASHINGTON BELL and WALTER EARL SPAHR. New York: Ronald Press Co., 1960. Pp. viii, 239. \$6.00.

This book resulted from a decision of The Executive Committee of the Economists' National Committee on Monetary Policy that "fundamental errors in principle and practice" (p. iii) in monetary and banking policy should be analyzed and a prescription prepared. The analysis and prescription do not vary substantially from what the same group has been saying for more than a quarter of a century. This is entirely appropriate if the only proper monetary standard is gold coin, without qualification as to the level of prices, output, or employment. Although there are nine contributors (Bradford, Spahr, Niehaus, Kemmerer, Wiegand, Palyi, Phelps, Patterson, and Bell), there is no significant diversity in outlook and only minimal repetition. This book is a well-argued, authoritative exposition of the faults of discretionary monetary and banking policy by dedicated proponents of the automatic full-bodied gold-coin standard.

Spahr believes that both quality and quantity of money are likely to be improper if currency is "managed." There is no need for management, for "all goods and services constitute a demand for all other goods and services, and these exchanges can be effected almost entirely and automatically by the use of self-liquidating credit, the remainder by paper and metallic money" (p. 32). "Credit . . . should rest upon productive activity that will automatically liquidate the credit upon maturity" (p. 33). The choice our society has made for substantial discretionary powers in the hands of government is considered not just a mistake, but positively wicked.

The authors of this book deprecate both the possibilities and need for cen-

tral bank policy as a weapon for attaining sustainable economic growth. The known limitations on the effectiveness of central bank policy (e.g., the absence of control over the velocity of credit) are praised as promoting "freedom." Such freedom is considered much more important than effectiveness of policy, because the economy really does not need much help from central bank policy. The major issues, the authors believe, are integrity of the currency and freedom from governmental restraints. Congress should voluntarily restrain its own power in order to develop the Federal Reserve into a check on the legislative authority.

Spahr argues that the Federal Reserve should be independent of the Treasury and should not become a part of any integrated monetary and fiscal authority. Loss of Federal Reserve independence would reduce market power over the Treasury, undermine the Constitution, and ". . . lead to Executive Dictatorship" (p. 46). This reviewer believes that forced coordination of monetary and fiscal policy probably would be a solution too extreme for the seriousness of the problem. But when two parts of government economic policy are frequently at odds this does constitute a problem worthy of deliberate and temperate consideration.

During recent periods of monetary stringency much discussion has centered on the effectiveness of quantitative controls. Kemmerer says that almost two-thirds of all savings accounts and one-seventh of demand deposits are beyond the control of the Federal Reserve. He also shows how savings and loan associations ". . . are to some extent in the banking business which involves an expansion of credit rather than being strictly in the business of receiving and lending savings . . ." (p. 90). In spite of this he fails to recommend placing nonmember savings banks or other financial institutions under Federal Reserve controls. The desire for "freedom" again overwhelms the argument for effectiveness in policy.

In the view of Kemmerer the "Federal Reserve System [is] too much under the influence of the Federal government and not enough under that of the banking and business world" (p. 66). It is therefore recommended that business interests should choose four members of the Board of Governors and the government three. Other recommended changes would end the Federal Advisory Council and the Federal Open Market Committee.

A model bill is included which provides for a gold-coin standard and some reforms which the contributors believe would increase the effectiveness of the gold-coin standard and the commercial loan theory.

This book is recommended particularly to the student who believes that the major objective of monetary and banking policy is full employment of resources. The authors believe no such thing. They are in the minority who believe that the American economy includes a self-adjusting mechanism which can yield sustainable economic growth with minimal governmental interference. In effect, the authors argue that the profession is wrong and that economists should return to the faith of their fathers.

NORMAN H. LEONARD, JR.

Ohio Wesleyan University

Public Finance

The British Budgetary System. By HERBERT BRITTAİN. New York: Macmillan; London: George Allen and Unwin Ltd., 1959. Pp. xii, 320. \$5.75.

Sir Herbert Brittain's purpose is to give a comprehensive description and interpretation of the British budget in the light of its growing economic importance since the second world war and its increasing capital outlays. He has portrayed in detail the general design of the budget, sources of revenue, elements of expenditure, the exchange equalization account, the national debt, and accounting and auditing procedures. He has refrained intentionally from pursuing an extended theoretical treatment of such topics as national income, incidence and effects of taxation and expenditures, and fiscal policy. Instead he has given an admirable exposition of the budgetary process from the vantage point of his long experience as Second Secretary of Finance in the British Treasury.

Both in concept and operation the British budget plan has had remarkable success and has served as a model for other Commonwealth nations. Since members of the Cabinet have seats in the House of Commons the budget emerges as a document of combined executive authority and legislative sanction. In this respect the procedure differs markedly from that in the United States where the executive and legislative arms of the government have co-ordinate status. Also, unlike the British budget, the U.S. federal budget is primarily an expenditure plan, revenue measures having been omitted from the functions of the budget.

A unique feature of the British budget is the traditional practice of separating income and capital transactions by drawing a line "across the middle of the Exchequer accounts" and placing operating revenues and expenditures "above the line" and placing receipts and expenditures applied to debt redemption and borrowing "below the line." Capital payments, however, may be charged either above or below the line, depending upon the nature of the outlay. This is a departure from the practice of commercial firms which rigorously separate income and capital accounts. The author regards as "radical" the proposal of J. R. Hicks that the trading or business activities of the government should be accorded different treatment in the budget from those of administrative departments. Hicks suggests that the accounts of the trading departments "ought to be modelled upon the accounts of a bank" and that the Treasury should exercise the sort of control over these departments which a bank exercises over its clients.¹

Effective control of the budget hinges on two major functions: first, the formation of estimates; and second, efficient supervision within departments and agencies. Responsibility for the annual estimates rests with the Chancellor of the Exchequer, but he leans heavily on officials of the Treasury who scrutinize the items and purposes of expenditures and the terms and conditions on which money may be paid out. The Financial Secretary of the Treasury presents the estimates for civil departments to Parliament, while defense esti-

¹ J. R. Hicks, *The Problem of Budgetary Reform*, Oxford 1948, pp. 16-17.

mates are submitted by the agencies themselves, though Treasury control also applies to these estimates. Responsibility for efficient supervision devolves primarily upon each spending department, the policy of the Treasury being to avoid as far as possible the details of administration.

The author takes an unequivocal position with respect to the incidence of a national debt, holding that the idea of shifting some part of the burden of war from the present to the future is an illusion. While future generations may be poorer because of the waste of war, "there is no shifting of the real burden from one generation to another." The only sense in which shifting may occur is through borrowing from a foreign country, repayment being made later through exports or some other transfer of value, which involves heavier taxation.² It is the opinion of the author that the use of annual sinking funds as a method of reducing the national debt is outmoded and "would seem to have gone forever." Future borrowing in substantial measure is likely to be applied to capital development programs of the nationalized industries.

Within the limitations prescribed, Brittain's book fills an important need for an authoritative treatise on the structure, technique and *modus operandi* of the British budget. It contains many insights on the procedures by which decisions are reached that are not to be found elsewhere. It is timely, thorough and carefully written. It should be of particular benefit to students of public finance, taxation and political science, as well as to government officials.

TIPTON R. SNAVELY

University of Virginia

International Economics

Trade and Economic Structure: Models and Methods. By RICHARD E. CAVES. Cambridge: Harvard University Press, 1960. Pp. viii, 317. \$6.00.

In one respect at least the decade of the 'thirties was a godsend for specialists in international economics. Iversen, Viner, and Wu added their impressive synopses of the theoretical literature on the international adjustment process and international price determination to Angell's pioneering study. Until recently, however, economists have not enjoyed the luxury of a major, up-to-date, English-language digest of the manifold and widely scattered theoretical contributions in international economics. A moment's reflection upon the theoretical and methodological advances in all fields of economics since that time suggests the enormity of this hiatus. The profession is deeply indebted to R. E. Caves' prodigious scholarship for filling in a significant portion of this gap. In *Trade and Economic Structure* economists again have available an imaginative, painstaking, and exhaustive survey of the pure theory of international trade. The book is, without reservation, a reference work of the first magnitude.

Caves' study is not a substitute for the earlier volumes. Rather it complements them. Its scope is limited to a consideration of the theoretical aspects of the simultaneous determination of commodity and factor prices, the quanti-

² For a contrary view on the shifting of the primary real burden of a national debt, see J. M. Buchanan, *Public Principles of Public Debt*, Homewood, Ill., 1958, Ch. 4.

ties of factors supplied, and the quantities of goods produced, consumed, and traded internationally, in a world of two or more countries. It is, therefore, concerned to show how the concepts of general equilibrium analysis have been used to build and modify the corpus of pure trade theory. Continuing in the purest of theoretical traditions, its analytical approach is that of positive rather than normative economics. It is a tribute to Caves' intellectual self-discipline that these focuses are rarely lost. But even more pervasive is the author's interest in the application and testing of trade theory. This last perspective, from which he does not deviate, has led Caves to view trade theory as consisting ideally of a "body of well-reasoned alternative hypotheses and models rather than as a monolithic formal structure resting on a minimal set of general assumptions."

This conception has put its impress on the organization of the book. The substantive gambit is an explication of the neoclassical and Heckscher-Ohlin models of trade and production. Haberler's theoretical model is subsumed under the latter formulation since, as Caves explains, the opportunity-costs version enters into the general equilibrium at an intermediate stage and yet is able to derive comparative advantage in money-cost rather than real-cost terms. The succeeding chapter on income distribution summarizes the impact of import duties on relative factor prices, faithfully records the development and qualification of the factor-price equalization theorem, and, especially pertinent to economic development policy, reviews the issues of the Australian tariff controversy. In discussing the role of factor supplies, Caves gives a novel interpretation of the effects of factor-supply variation on the transformation function. A short chapter on modifications of the conclusions of international trade theory necessitated by a relaxation of the assumption of international factor immobility is followed by a discerning exposition of the consequences of alternative hypotheses concerning production functions in the classical and Heckscher-Ohlin models. Here too the general equilibrium effects of technological change, nonconstant returns, and monopoly are examined. The final chapter within the general equilibrium format considers the technical difficulties trade theory faces in incorporating demand influences, analyzes the consequences of shifts in taste patterns, and briefly ponders the absence of any rigorous treatment of demand as an endogenous variable.

Caves' final three chapters develop peripheral subject matter. In the preceding sections welfare considerations occasionally intrude. These scattered comments are systematized by discussing the results of imperfections in the preconditions for a Paretoan optimum and by summarizing the welfare criteria for judging policy alternatives. To partially compensate for the predominance of comparative statics analysis in the major portion of his study, Caves scrutinizes the dynamic adjustments entailed in the processes of economic growth, capital accumulation, international transfer of factors, and technological innovation. The final chapter recounts the results of efforts to test empirically the classical and Heckscher-Ohlin formulations, the theoretical issues raised by Leontief's paradoxical study receiving particular attention.

From all this one vivid impression emerges. While classical trade theory may still retain an advantage in terms of statistical verification, *Trade and Eco-*

conomic Structure testifies to the enormous theoretical fecundity of the Heckscher-Ohlin model. To gain maximum appreciation of this fact the reader must be thoroughly versed in the nature of the questions that pure trade theory seeks to answer. That Caves does not adequately provide this background is the volume's only appreciable defect. Yet, in view of the book's immeasurable contribution to the understanding of the current state of pure trade theory, it is an insignificant blemish.

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Obchodni operace v československém zahraničním obchodu. By JAROSLAV NYKRYN. (Commercial Operations in Czechoslovak Foreign Trade.) Prague: Orbis, 1958. Pp. 915.

This book, along with three others¹ authored by Professor Nykryn of the School of Economics in Prague, should be unusually useful for students of the Eastern foreign trade monopolies. There is much in the present volume that will interest not only the economist and businessman, but also the lawyer. Although Nykryn deals primarily with the theory and administration of Czechoslovak international trade, this is not a limitation for one-third of Czechoslovakia's national product depends on external commerce—which is a larger proportion than that of any other Soviet bloc economy.

This review concentrates on only two aspects of Nykryn's work: the communist foreign-trade theory (gains from trade); and the planning process which Nykryn describes in unprecedented detail.² The parts omitted from this review contain (1) a comprehensive survey of the organizational hierarchy and the decision-making process in Czech foreign trade; (2) the contractual bases of the country's external economic relations; and (3) a description of the legal settlements of commercial disputes between Czechoslovakia and both the socialist and capitalist countries. The emphasis placed throughout Nykryn's volume on contrasts between the Soviet and Western trade systems answers many Western "why's" and sheds some welcome "Eastern" light on the problems of economic relations between planned and market economies.

The communists assert with noticeable satisfaction that one of the distinguishing characteristics of their foreign trade is the mutual assistance promoted by continuous efforts in planning and coordinating their commercial and financial policies. This cooperation allegedly leads to a higher type of international division of labor and thus facilitates progressive expansion of socialist trade. The reciprocal benefit hinges, for instance, on *direct* trade relations (the exclusion of intermediaries), the certainty of planned shipments, and the absence of exploitation.

¹ *Ceny a jiné problémy zahraničního obchodu* (Prices and other problems of foreign trade), Prague: Statní pedagog. nakl., 1953; *Mezinárodní platební styk* (International payments), Prague: Statní pedagog. nakl., 1954; *Organisation und Technik des Aussenhandels*, Berlin: Verlag die Wirtschaft, 1956.

² Generally, his account of Eastern foreign trading is superior to that found in the well-regarded Soviet publications: S. V. Serebriakov, *Organizatsia i tekhnika sovetskoi trgovli* (Moscow, 1949) and *Organizatsia i tekhnika vneshnei trgovli* (Moscow, 1958).

Government monopoly imparts additional advantages. It protects the domestic economy against unfavorable external influences (such as cyclical fluctuations) and creates contractual conditions for trade free of the "elements of negotiation" found in capitalist countries which permit strong trade partners to take advantage of weaker ones. A communist government, with vast information at its disposal, is in a unique position to survey, compare, and evaluate sources of supply and sale outlets (which capitalist governments cannot do). In addition, the Soviet-type governments have the financial means to cope with changes in both demand and supply of international goods. These governments enjoy the traditional benefits of bigness, can resort to arbitrary measures, and are capable of giving preference to a political gain even at the cost of economic loss.

East European Communists contend that the classical liberal explanation of foreign trade is based on the capitalist system of production which disregards (Marxian) historical considerations and is inherently exploitative. The liberal theory is incompatible with socialist thinking and practice which rely on planning.

Communist foreign trade theory justifies the existence and desirability of an international exchange of goods and services in four main ways: (1) by internal saving of the labor time needed to produce imported goods; (2) by increased productivity which may result from international division of labor; (3) by reduction of labor input whenever the factor of production labor is replaced by capital (machinery and other labor-saving devices); and (4) by the downward pressure which foreign trade tends to exert on domestic costs of production.

Accordingly, the Soviet bloc countries find that composition of commodity imports and exports is most advantageous which enables them to achieve the maximum saving of social work (effort). But foreign trade has more than an *economic* effectiveness estimated from the point of view of productive factors; it has a *social* effectiveness measured by its influence on national defense, intrabloc cooperation, international political relations, etc.

Productivity (in communist terminology "rentability") of commercial operations is both a clue to, and an indicator of, the financial and real economies to which foreign trade gives rise. (Although "rentable" trade may be defined in several ways, the definition which is least likely to cause disagreement refers to it as that trade which does not require government subsidization and can do without special price considerations by industries.)

The determinants of productivity in trade with capitalist countries differ from those applied to trade among communist countries. In the former case, "rentable" trade depends on the commercial conditions of concluded contracts and on distribution costs, both of which find expression in terms of trade (as a price phenomenon). The communists consider the productivity of capitalist commercial operations to be significantly influenced by the quality of goods sold (since higher quality is expected to result in larger sales, and vice versa), by exporters' ability to sell, by importers' ability to buy, and by other qualitative aspects of commercial transactions.

In addition to the commercial terms of contracts and distribution costs,

productivity of socialist operations is a function of the trading partners' "mutual advantage." This is allegedly easy to determine in communist commerce inasmuch as prospective customers and suppliers are known when production gets under way. In intrabloc trade, price differences and discrimination presumably play a minimal part. At any rate, there is no need for them. Socialist prices are not influenced by factors which loom large in capitalist pricing, such as the time of shipment, the type of foreign exchange used in payment settlements, etc. Furthermore, direct exports and imports minimize charges for banking, transportation, insurance, and other services. Because promotion efforts and expenses are relatively unimportant, they cannot cause price differentials. The same applies to market risks, which are absent.

Since in communist economies the economic effects of *individual* foreign trade transactions are considered inconclusive, the principal theoretical criterion for them rests on whether they are "optimal." This depends on whether international transactions ensure, or guarantee, that the national foreign trade plan will be fulfilled most economically—taking into account all the major objectives of international relations. Clearly, this is an extremely vague criterion.

The optimum is a function of different factors or considerations depending on whether "capitalist" or communist trade is in question. In contrast to commercial relations among free economies, for the Soviet bloc countries prices of exchanged commodities are frequently of secondary importance. As a result, an import transaction, for example, may be optimal as long as the value of the imported good meets the requirement of the existing foreign trade plan. To add to the vagueness of this concept of "optimum," the territorial optimum, which is relevant primarily in trade with capitalist countries and indicates areas with which the most beneficial trade takes place, depends on commercial policy. Little imagination is needed to realize that in this, the bloc countries can proceed in a quite arbitrary manner.

Still another dimension of optimal transactions concluded with the capitalist nations is based on the contribution of export trade to a communist country's foreign exchange reserves. Soviet bloc economists have attempted to measure the increments in foreign exchange supply which result from different amounts of domestic labor engaged in external trade. According to these theorists, the gains from trade are arrived at by taking into account the composite of all foreign operations effected during a definite time period, and are ultimately evaluated in real cost terms.

The long-term economic plans drawn up by the East European countries to achieve "proportional" development of their economies call for corresponding foreign-trade plans. These are complicated because relations with capitalist nations and with the bloc countries follow different patterns. With regard to the former group, communist planners attempt to estimate developmental trends so as to be prepared to change methods of selling, buying, financing, and sales promotion. Because of the co-ordination of the East European

economies, foreign-trade plans have established and preserved a substantial degree of cohesion among them.

Some of the essential considerations underlying a long-term plan for foreign trade are (1) the anticipated composition of imports and exports, (2) the rate of growth of foreign trade turnover, (3) domestic investment as related to foreign trade (and imports in particular), (4) the need for foreign exchange reserves, and (5) availability of, or dependence on, credit.

Vertically, a comprehensive foreign-trade plan is broken down into commercial-financial plans (for imports, exports, and foreign exchange) and into others pertaining to transportation, propaganda, labor and wages, and capital investment. Import and export plans are divided into partial plans for geographical areas. Nykryn discusses in detail the extent to which individual agencies participate in producing long-term, yearly, and quarterly plans, and the statistical-economic analyses used to control fulfillment. Presumably, the Soviet-bloc countries have adopted a system of uniform analytical principles for foreign trade to ensure comparability of planned achievements.

The planners of communist foreign trade believe that the success with which their programs may be put into effect is influenced in considerable measure by the following features of the plans:

1. The foreign-trade plans, like other plans, acquire a binding force similar to that of a law. This plus the responsibilities imposed on those who carry out the plans presumably has had a mobilizing impact on plan execution.

2. The plans are broken down into a large number of tasks and performances whose maximum fulfillment is assured by premiums and prizes awarded to those who make special work efforts.

3. It is possible for long-term plans to be continuously improved on the basis of experience gained in the execution of short-term plans.

4. The planned symbiosis between the economy as a whole and its international sector accounts for the fact that foreign-trade benefits from particularly favorable conditions prevailing in the internal economy. The opposite is, of course, also possible.

5. Foreign-trade plans are set up with regard for a number of economic indicators which are general and therefore reliable enough to ensure optimal results in any phase of international commerce.

6. All activities connected with the completion of foreign-trade programs are subject to reviews and controls instituted not only by the various organs of the ministry of foreign trade, but also by other organizations having some responsibility for, and interest in, external commerce. All these organizations are in the position of jointly curbing any undesirable spreading of disproportionalities in trade flows.

Most readers concerned with the potentialities and limitations of East-West trade will find in Nykryn's work another proof of the existing, sometimes irreconcilable, differences in the nature, administration and objectives of Soviet-type and free foreign-trade systems. The business criteria and commercial considerations which have been sacrosanct for the West are not usually

applicable in the East. On the other hand, it is not likely that the free world will ever succeed in neutralizing the doctrinaire nature of communist trade.³

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Economic Development and International Trade. Edited by PAUL D. ZOOK.

Dallas: Southern Methodist University Press, 1959. Pp. viii, 134. \$3.00.

This collection of eight papers was selected from those presented at two conferences held at Southern Methodist University in 1958 and 1959, under the auspices of the Jno. E. Owens Memorial Foundation. The papers deal with the conferences' subjects, "International Trade and Economic Development" and "United States Foreign Economic Policy," but they are not closely related to each other. In the development field, emphasis is given to Latin America. Written by academicians and international and United States officials, the papers are addressed to the general public as well as to a university audience, and they reflect in varying degrees this dual objective.

In the first paper, Howard Ellis forecasts an increasing absolute volume of international trade. He bases his optimism on such factors as population increase, development aspirations of the underdeveloped countries, reduced flows of labor and capital (making commodity trade more profitable), and the transfer of techniques—conditions different, at least in degree, from those when Robertson and Viner made international trade predictions in the 'thirties and 'forties (reprinted in the *AEA Readings*). Contingencies that Ellis says could qualify or even reverse his forecast are a new wave of trade restrictionism or possibly the really economical harnessing of atomic power. Ellis concludes that the United States gains from increased trade stemming from foreign economic development, and he advocates an increased foreign economic-aid program.

Howard Piquet, in his paper, implies that United States aid policy is not deficient, but he deplores the present policy of drift in the trade field. Recognizing that free trade is politically impossible and that protectionism is internationally dangerous, he proposes a more liberal policy based upon government assistance to domestic industries that are forced to adjust to increased imports or a policy of permitting imports to increase on a selective basis, using some variant of the tariff-quota device.

J. Carter Murphy, applying Viner and Meade customs-union theory to the European Common Market, regards favorably its possible impact on the pattern of free-world production and trade. He is also optimistic as to possible economies of scale, and he concludes that the Rome Treaty provides a recipe that economists can endorse.

Wendell Gordon distills several chapters from his recent text on international trade in a paper on international investments. In questioning the contribution that foreign investment makes to economic development, he cites historical evidence that such investment grows by accretion (reinvested earn-

³ Cf. Oscar Lange, "The Economic Laws of Socialist Society in the Light of Joseph Stalin's Last Work," *International Economic Papers*, Vol. IV, London and New York 1954, pp. 145-80.

ings, capital gains, etc.) rather than through the export of real capital goods. He concludes, therefore, that foreign investment, as distinct from technical knowledge, managerial skill, and entrepreneurial initiative, does not contribute much to economic development. He suggests that underdeveloped countries should meet their capital-equipment needs by paying for them with exports, thus avoiding the service and repayment burdens of foreign investment. He does not, however, explain how most underdeveloped countries can generate enough exports to pay for substantial capital imports without first undergoing some economic development and how this initial development can take place without foreign capital imports. Nor does he explain why such capital imports should be discouraged if they more than pay for themselves out of the new wealth that they help to create. Gordon also criticizes the view that average interest- and profit-rate differentials explain foreign investment. He says that such investment is made on the basis of individual projects and is influenced by social, technical, and political considerations. However, to attribute to Ohlin and Iversen average interest- and profit-rate differentials as the explanation for foreign investment is an oversimplification of their views.

Richard La Barge's interesting case study on the economic development of Honduras singles out foreign investment as the initiating cause for the economic growth that has occurred there since 1943. He claims that foreign investment stimulated domestic savings and investment and that it should have the same effect in other capital-poor economies where foreign-owned enterprises have an important role in producing goods and services.

Reynold Carlson and Jorge Del Canto write on how the International Bank for Reconstruction and Development and the International Monetary Fund, respectively, aid Latin American economic development. Carlson emphasizes the Bank's noncapital assistance and Del Canto concentrates on the Fund's efforts in combatting inflation. Sydney C. Reagan discusses United States export programs for surplus agricultural commodities since their inception in 1954 to 1957, briefly analyzing their effects on the recipient countries, on other agricultural exporting countries, and on the United States.

For the most part, this is an interesting, albeit uneven, collection of papers. Like most compendiums of its kind, it would have been much improved if the papers had discussed various aspects of some central topic rather than various topics in the same general field.

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Capitalismo mondiale e cartelli tedeschi tra le due guerre. By J. J. LADOR-LEDERER. Turin: G. Einaudi, 1959. Pp. xviii, 400. L. 2500.

World Capitalism and German Cartels in the Interwar Period ought to have a translation in clear and nontechnical English. In a cheap edition and well advertised the book would reach the wide audience it fully deserves. The book retells the story of the German "miracle" of the years after the first world war. The parallel is West Germany's economic rebirth after 1947. The *leit-motif* is the machinations of international finance and the cartel structures

of the 'twenties and 'thirties; the counterpoint, the men in Brooks Brothers suits now crowding Brussels and Geneva.

The stress that Lador-Lederer gives to the "functional character of international cartels," to their almost "necessary" development, leaves the reader with the certainty that a structure as gigantic and as efficient as that described—a structure, incidentally, that remained intact in spite of the second world war—is more than a fact of history. Its impact on current economic affairs must be recognized and needs continuous study. Consideration of existing cartellization and its effects upon economic interdependence must form part and parcel of any analysis of developments in the European Common Market and consequently of the European economic policy currently followed by the United States. The author's central thesis helps validate recent rumblings characterizing our European policy as self-defeating. This point of view, apparent, *inter alia*, also in the Staff Report of the Joint Economic Committee of Congress, argues that in promoting the Common Market we have helped the development of German hegemony in Western Europe which—through a new Russo-German Alliance—will eventually challenge the leadership of the United States in the Western World.

In relating developments of the interwar period the book shows in detail the interdependence of the requirements of the cartels with those of the European states; the requirements of both expressing themselves at the "supra-national" rather than the international level. Similar interdependence of economic interests is at the root of contemporary action advocated and implemented under the banner of integration. When the historical data provided by the book are viewed as building blocks and joined with the mortar which is the work of men such as C. Wright Mills and Burnham—and the book is replete with their ideas—an imposing analytical structure is developed. What follows from it, implicitly rather than explicitly, is the thesis that a truly cosmopolitan power elite is one of the central social facts of the contemporary world.

Beyond its timeliness and intrinsic importance the book deserves notice on three counts: it is a serious work based on a minute and thorough dissection of available facts all of which are carefully documented; it is a clear contribution to the economic history of Europe for the interwar period; and it throws much light on the use of economic power for war. The book's arrangement is essentially chronological. The disorder and the morass of 1918 to 1923; the recovery from 1923 to October 1929; the crises of 1929 to 1933; the transformation to a war economy, 1933 to 1939 are reviewed in turn. A concluding section describes the economic problems of the war and provides a set of "Conclusions on Germany." There is an extended bibliography.

The use made of Germany's international business relationship for war is shown in the economic use of the fifth column, in the frustration of the economic development of her allies and in her capitalizing on foreign industrial expertness. Differences between German and non-German industrialists are viewed in terms of the commitment of the first to war and of the second to the maintenance of business. American business interests are thus painted as having interests in cartels that go beyond battle lines, as having equated op-

position to the New Deal with isolationism and, to the extent that they were influenced by economic consideration linking them to German financial ventures, as having reduced "the potential of the arsenal of democracy."

A work as embracing and as ambitious as this one necessarily has already-known material, has occasional errors and at times overstates its case. The author reflects his own life—as a Marxist economist, the victim of German concentration camps, minister in charge of economic planning for Tito, and now with the same duties in Israel—in an approach critical of capitalism and with a deep attachment to human values. This explains his conclusion that technical solutions of the cartel problem are useless as they must necessarily crumble in the face of "the satanic laughter of economic realities." His pessimism, disturbing though it is, cannot be vouchsafed. The book deserves careful reading.

OSCAR ORNATI

New School for Social Research

Business Finance; Investment and Security Markets; Insurance

Les choix des investissements. By PIERRE B. D. MASSÉ. Paris: Dunod & Co., 1959. Pp. xvi, 489. 4,900 fr.

This remarkable study of the normative theory of investment has no real parallel in the literature in its unique blend of theoretical and practical work, nor are conditions elsewhere than in France likely to make such work possible (except perhaps in the Netherlands and Norway). The author has been Associate Director General of Electricité de France (the nationalized electricity industry) and is currently High Commissioner for Planning, the position created by Jean Monnet. What is extraordinary is that the practical problems of the electricity industry and particularly of its investment program should be governed by a rigorous theoretical analysis which indeed turns out to be the only truly practical way of proceeding. While the application of mathematical methods to industrial production and transportation problems has gone on apace in this country, I believe it correct that very little indeed has been done with fixed investment as far as practical problems are concerned. Even the study of inventory policy has so far been more theoretical than applied.

The important strides in the joining of rigorous analysis to planning problems in France by M. Massé and his younger colleagues (among whom might be mentioned, without any pretense of completeness or ranking, Maurice Allais, Marcel Boiteux, Robert Gibrat, Jacques Lesourne, Edmond Malinvaud, and Georges Morlat) are too little known in this country. To judge by the ignorance of the French language among our graduate students, the situation is unlikely to improve in the near future. Incidentally, it is doubtless significant that nationalization in the United Kingdom has produced no comparable literature.

The present book deliberately avoids any claim to completely systematic treatment. Almost every aspect of the problem of investment choice is touched on and the general principles discussed, but within each topic intensive dis-

cussion is confined to selected topics, usually arising out of the work of Electricité de France or one of the other nationalized energy industries. With the exception of a few simple examples for didactic purposes, the cases discussed involve real decisions of considerable magnitude, a fact which contributes greatly to their fascination.

The first four chapters deal with investment choice under certainty. The general criterion is that of maximizing the sum of discounted profits, where, however, the opportunities for reinvestment must be included in the decision process. The second chapter immediately shows the practical and theoretical character of the work; it deals with criteria for the economic length of life of equipment taking account of both wear and tear and obsolescence. Even under simple hypotheses about technical progress, the problem is difficult.

After a good, though standard, treatment of linear programming in general, the author proceeds to an exposition of the development of the investment plans of the French electricity industry through 1975 by means of linear programming. The various complications are worked out in considerable detail, in such a way as to give the reader an excellent feel for the process of formulating a model for analysis of a real situation.

The next four chapters deal with investment planning under uncertainty and are, to me, the most exciting part of the book. The first of them deals with the criteria for behavior under uncertainty. In addition to an excellent discussion of the various concepts in the literature, such as expected utility, subjective probability (he follows here de Finetti's formulation rather than Savage's) and minimax, he gives an excellent discussion of the conditions under which the law of large numbers does or does not permit one to replace probability distributions by their expected values. The following chapter gives the best discussion I have seen anywhere of the concept of a strategy and its special role in dealing with uncertainty. This is illustrated by a masterly discussion of the proper behavior of an insurance company in limiting its risks. Though a strategy in principle requires considering consequences into the indefinite future, some conditions are given under which there is no loss in considering only a few periods ahead.

The major applications are to inventories, the use of hydroelectric reservoirs with both demands and water inputs random, and production of coal with random demands and import prices (due to P. Gardent). These are handled with admirable clarity and originality. There follow discussions of the rate of exploitation of a mineral resource of unknown magnitude, electric plant under conditions of uncertainty as to water supply and demand, and problems of meeting disaster (e.g., floods) and achieving flexibility by appropriate equipment. It is impossible to summarize easily the rich variety of results.

The last two chapters are more general in nature. There is a summary of modern welfare economics, with particular regard for its implications for the behavior of a nationalized enterprise. For the first time in the book, there is a discussion of criteria appropriate to conditions of decreasing cost. It is interesting indeed that so much planning seems to be perfectly possible in practice

on the basis of the constant returns assumptions of linear programming. It suggests to me that the problem of allocation under increasing returns may not be as difficult in practice as it seems to be in theory.

In the final chapter, the author eloquently expresses his "neoliberal" viewpoint—the competitive system within a governmental framework designed to remove its deficiencies, particularly those arising from increasing returns and the need for coordination of attitudes towards the future.

The reader by now has doubtless inferred that I regard M. Massé's book as well worth study.

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The Management of Corporate Capital. Edited by EZRA SOLOMON. Graduate School of Business, The University of Chicago, Series 3. Glencoe, Illinois: The Free Press, 1959. Pp. 327. \$7.50.

For those who have not followed too closely the growing literature on the subject of capital allocation decisions, the title of this work may not appear particularly attractive. One's first instinct would be to view it as a book solely of interest to specialists in the field of business management and finance. Nothing could be farther from the truth. Although it is difficult to conceive what other title the editor could have chosen, the fact is that the rich contents of this work are of essential importance to many different disciplines, including economic theory, accounting, statistics, and managerial economics. Their ultimate significance for business consultants, economists, and executives—indeed, to all concerned with the most rational and efficient disposition of capital resources—cannot be too strongly emphasized.

As an addition to the new series of publications sponsored by the Graduate School of Business at the University of Chicago, this valuable work more than justifies itself as a source of new knowledge and ideas contributing to the improvement of business practices in this vital area of decision-making in the capital investment process. Solomon has done a masterful editorial job. Twenty-two essays have been carefully selected from journal literature of the past seven years and reprinted here in an order that insures continuity of thought, a progression of more involved analyses, and a deepened insight into the more intricate problems surrounding the subject. The work is methodically divided into seven sections: (1) introduction, (2) measuring investment worth, (3) the cost of debt and equity funds, (4) general solutions to optimal investment decisions, (5) special aspects of capital measurement, (6) discount tables, and (7) classified bibliography. Most of these sections contain the finest and most closely reasoned disquisitions that have been offered to deal with the management of corporate capital. The bibliography is similarly well organized and reflects the distinctively comprehensive character of the work.

In his introduction, the editor performs a most useful service by reviewing the common objectives of the articles, the mutually held assumptions of the authors, the areas of agreement among them, the problem areas, and the un-

settled issues. The reader is given a full view of the forest before he ventures into the woods. All the writers are bent upon the formulation of a rational approach to capital-allocation decisions. All are concerned with how this problem should be solved rather than how it is being currently solved by business. Each recognizes that the optimum solution presupposes interrelated decisions on the chosen magnitude of gross investment, the composition of expenditures, and the composition of financing; and each analyst also agrees that present business practices in this area are not based sufficiently on a rational approach. For example, the measurement of profitability on the basis of the pay-out period or the ratio of net conventional income to average book-value of utilized capital is rightly deemed as an inadequate solution. Instead, the discounted-cash-flow method is advocated, which is essentially the process of finding the interest rate that discounts future earnings of a project down to a present value equal to the project cost. Throughout all the articles a persistent attempt is made to develop rigorous, logical concepts for capital budgeting; and every conceivable factor, ranging from opportunity costs to residual values, comes in for incisive analysis.

The outstanding value of this volume is that it lays before the reader the breadth and depth of logical thought on processes and methods for rational capital budgeting. The work is in essence an embodiment of source material for constructive use in the various disciplines mentioned above. Joel Dean's contributions, for example, reveal a systematic endeavor to apply formal economic theory to the general problem of capital allocation; and the articles by Alchian, Solomon, and Durand, not to mention the others, bring into play interest theories, cost-of-capital concepts, and imperfect market considerations. Although the patient reader will be duly impressed by the strides made toward a sound conceptualization of the many problems converging in this area of business operation, he will doubtless form certain balancing reservations supported by the roles of uncertainty, experience, and even business intuition in our permanent environment of economic contingency.

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Studies in Company Finance: A Symposium on the Economic Analysis and Interpretation of British Company Accounts. Edited by BRIAN TEW and R. F. HENDERSON. National Institute of Economic and Social Research Economic and Social Study, Vol. 17. New York and Cambridge, Eng.: Cambridge University Press, 1960. Pp. xix, 301. \$6.50.

Heuristic in nature, *Studies in Company Finance* is a pioneering empirical inquiry into the financial characteristics and practices of the bulk of publicly owned British companies engaged in manufacturing and distribution whose securities are listed on a stock exchange in the United Kingdom. While this volume involves a different analytical technique and is more comprehensive than its earlier companion book, *Company Income and Finance*,¹ it may, nonetheless, be regarded as both supplementary and complementary to the latter.

¹ The National Institute of Economic and Social Research, *Company Income and Finance 1949-1953*, London 1956.

In the first of three major sections is presented an analysis of the aggregative information on 2549 continuing firms in the period 1949-1953. Basic data are derived from the published consolidated financial statements required of publicly owned firms by the Companies Act of 1948. The second section consists of detailed analyses of the financial characteristics of particular industries, among them brewing, building, electrical engineering, and retailing; in view of the significant part that industries such as the chemical, ship-building, and automotive played in the postwar renaissance and structural rearrangement of British industry, it is regrettable that individual treatment was not extended to them. The third section contains the statistical appendices where the aggregative data on firms included in the study are classified according to industry, size, and rate of growth.

Perhaps one of the most interesting features of this study is the classificatory system employed to organize and evaluate the data. Sixteen "indicators" are used to classify the various financial relationships existing in a company and/or industry. Included are: net investment-net income ratio; thrift (ratio of net retained income to net income before interest and dividends); self-financing (ratio of thrift to net investment); liquidity (percentage of net liquid assets to net closing assets in 1953); capital issues of all kinds; and growth in net assets.

While many of these indicators have been commonly used in other studies, the addition of several new ones by the authors, such as thrift, net investment-net income ratio, and self-financing, should prove of interest and value to others. In addition to the classification by "indicators," firms are further classified into 21 industrial groups, 7 size groups, and 4 growth groups. Cross-classification by two or more indicators or groups permits the subdivision of the 2549 companies into 147 subgroups.

Effectively refuted by the findings are certain widely accepted beliefs about the postwar economic behavior of British firms: (1) Although it is ordinarily believed that the majority of investment by British firms was financed through borrowings, this study shows that 83 per cent of their investment in tangible fixed assets and inventories was self-financed from gross savings, i.e., retained income plus depreciation. (2) The burden of taxation was met at the expense of lower dividends, not, as is often supposed, by a lowering of the ratio of retained earnings. For the period 1949-1953 listed companies retained about 25 per cent of their pretax net income compared to 18 per cent in prewar years. On the other hand, in the prewar years dividend payout accounted for 68 per cent of pretax income but only about 33 per cent in the early postwar period. (3) Definitely shown by the authors is the importance of capital issues as a source of external funds for many companies. Capital issues (common and preferred stock, and debt of all kinds except trade credit) accounted for 28 per cent of total investment and 27 per cent of the growth in net assets. The observation is made that reliance on the new-issues market appears much more closely related to the rate of growth of an industry's or firm's net assets than to any one use of funds such as net expenditure on tangible fixed assets. (4) Contrary to common opinion, bank credit in the aggregate did not represent an important source of funds. Approximately 46 per cent of all com-

panies were never in debt to their banks, and bank credit accounted for only 3.8 per cent of the growth in net assets. Larger firms, not the smaller companies, utilized bank credit more often as a means of financing.

The findings on concentration, profits, and growth for this period are not dissimilar from experience in the United States. The study concludes that on the basis of size: (a) profits, on a percentage basis, for the larger companies rose more rapidly than for others; (b) larger companies carried out relatively more investment per pound of income; and (c) larger companies obtained more capital from the open market. Mergers were more important to the expansion of smaller firms than to the larger units. Of particular industries, those not technically stable and those most capital-intensive grew at a faster rate and consequently required large amounts of funds from all sources.

In summary, *Studies in Company Finance* constitutes a substantial contribution to knowledge of the financial relationships of the most important publicly owned British companies in manufacturing and distribution. The analytical system is excellent, and the coverage, both quantitative and qualitative, is thorough. Empirical studies of American firms could be wisely based upon the approach used in this book.

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Untersuchungen über die Investitionsentscheidungen industrieller Unternehmen. By ERICH GUTENBERG. Köln and Opladen: Westdeutscher Verlag, 1959. Pp. 230. DM 23.-.

In this book on decision-making for capital expenditures, Erich Gutenberg restricts his study to a microeconomic point of view. His main interest is to bring into focus the motivating forces behind management's decisions to commit itself to a major capital investment program. It is not a study of cause and effect of investment decisions on the total economy. It is not a quantitative correlation-regression analysis of capital expenditures and determinants. Rather, the author uses the more direct individual interview approach to probe into the concrete investment decisions actually made by major German firms during the three-year period 1954 to 1956.

The inquiry then is not how an enterprise nor an entire industry would propose to act under certain conjectures but rather how they actually did react to economic forces during the test period. The "why" rather than the "how" is the focus. It is precisely by this approach that the author is able to make a contribution to the otherwise profuse and in many cases, incongruous field of inquiry on the general subject of capital investment expenditures. This research into motivations distinguishes Gutenberg's report to a rather marked degree from similar studies by Schindler, Katona, Eisner and Terborgh.

The first two chapters are devoted to the incidence of capital expenditures. Chapter 3 develops the interview approach as used by Gutenberg.

The three main areas of investigation used in the study are discussed in Chapters 4 to 6. They cover the problems of timing and motivation, planning

and evaluation (*rentabilität*). The area of timing and motivation is further sectionalized into replacement and expansion expenditures with the latter furthermore subdivided into investments made either for the purpose of vertical integration, product diversification or added capacity per se. The impact of such factors as competition, cost behaviors, social and legal implications, labor markets, etc., is incorporated in the findings. These are based on comprehensive interviews with 76 national firms in 9 basic industries: coal, oil, steel, chemicals, electronics, machinery, automobiles, textiles and brewing.

In the remaining chapters (7 to 10) the author reviews organizational budget and control aspects as well as "justification" and "cost evaluation" methods used by these plants in conjunction with investment planning. Still, it did not escape the author's attention that the impact of that vast area of imponderables which constantly defies measurement, motivated to a large degree the final decision in many instances; and it is not surprising that he should come to the conclusion that the ability and maximum financial strain on the total financial position of the individual enterprise influenced strongly the final decision. It would appear that the author accomplished his objective by establishing, at least to a degree, the "weighted" impact of each motive on the total capital investment policy.

The work concludes with a summary and comparative analysis of investment motives and appends a detailed case study describing motivating forces behind a decision to construct a new plant for the manufacture of synthetic fabrics.

The author quotes and compares U.S., English and German publications. The work presents a scholarly and well-organized approach to this very controversial area. Since by far the greater part of the report is based on the actual decisions made, it affords an opportunity for comparison with the strategies used by U.S. firms as against those made by similar enterprises on the Continent. Also, the study should be of interest especially at a time when U.S. capital is seeking investment opportunities in the European market.

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Corporate Financial Management. By RAYMOND P. KENT. Homewood, Ill.: Richard D. Irwin, 1960. Pp. xv, 851. \$7.50.

This book is a new addition to the growing list of high-quality texts in the business finance field. As the name suggests the presentation is management-oriented, the emphasis at all times being upon the way in which business executives formulate policy and arrive at decisions. To enable the student to understand this process an adequate description of institutions, arrangements and techniques is presented. However, the author always stresses "why" rather than "what" or "how," since he feels the study is quite incomplete if the student does not clearly comprehend management's reasoning in arriving at a decision.

The organization of the text departs somewhat from the traditional pattern in that techniques of financial analysis and control as well as factors to be

considered in determining the financial structure are discussed before the sources of financing themselves are described in detail. Parts I through IV take up the business corporation, finance functions and cash flows, asset management, and cash budgeting and control in that order. In Part V the issue of choosing the financial structure is considered. The next five parts then cover the major sources of financing which the author divides into the categories of common stock, preferred stock, retained earnings, bonds, and financing without securities.

Two aspects of the organization of the text deserve special mention. First is the heavy emphasis the author places upon internal financing. Second is his treatment of recapitalization, reorganization and expansion. Rather than covering these topics in separate chapters he integrates his discussion of them into the presentation of sources of financing. This approach has the distinct advantage of making the student relate these problems to the types of sources of financing and their characteristics. Unfortunately it also has the disadvantage that a unified treatment of expansion and problems of dealing with financial difficulties is nowhere available.

The book has several interesting features. For example, the author develops the idea of cash flows by means of a series of balance sheets depicting the changes produced by a series of transactions. This treatment, which is quite similar to that employed in developing basic concepts in elementary accounting, is both clear and effective. Throughout, the author has been successful in avoiding the prolonged description of minutiae which all too frequently makes finance texts both lengthy and dull. His literary style is clear and straightforward.

This text can be adapted to either a one- or two-semester course. The addition of extra readings or case assignments would make it suitable for the latter, while a few exclusions can easily pare it down to single-semester proportions. Most of the ideas developed apply to all types of business and are not confined to corporations alone. Consequently the book can be used in any basic business finance course. This is a high-quality text which should be given serious consideration when a finance text is selected.

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Industrial Organization; Government and Business; Industry Studies

The Price Discrimination Law. By CORWIN D. EDWARDS. Washington: The Brookings Institution, 1959. Pp. xxii, 698. \$10.00.

This massive work was originally a follow-up study of the effect of lawsuits brought under the Robinson-Patman Act. But the extensive interview material was disappointing, proving again that while interviews are valuable for hints and leads, they cannot form a body of evidence. Unwilling to garnish ignorance with trade opinion and call it research, Edwards made the book a review of the records and decisions in 311 cases, of which 84 were extensively

analyzed. Practicing lawyers in particular will find it useful to have the cases all set out intelligibly, according to the principal legal issues. After a historical sketch and a legislative history, there is a brief synopsis and an analysis largely of procedure but including also the industrial composition of the cases. There follow chapters on brokerage; proportionality in payments and services to buyers; volume, quantity, and functional discounts; territorial price differences, including the abortive campaign for legislation to undo the *Cement* decision; and cases against buyers; "injury to competition," and the defenses of meeting a competitor's price and cost justification; concluding with an appraisal of the Act, and suggestions for legislation.

Edwards' basic method derives from his well-known essay [3]. George Stocking's comment [3] demonstrated that Edwards' "economic power" confused economies of size and monopoly, which two concepts ought to be kept distinct. Edwards' next important work [4] responded to Stocking's criticism by avowedly taking the whole discussion out of the realm of economic analysis; what had been "structural requirements for workable competition" now became "competition, as a political concept," and so on.

The deliberate separation from economics is carried further in the study of a law prescribing price equality. Edwards does not share the illusion that better resource use is a conscious and primary antitrust objective. (Perhaps it should be, but it is not.) Well enough, but in nearly 700 pages there is no economic analysis and no statement of objectives.

Edwards treats economic theory as a set of purposes or values, which it is not, and ignores it as a mechanism for discovering how costs, prices, and outputs are related through a market process. To the "economic idea" that "discrimination exists whenever prices and costs do not vary concomitantly" (p. 2) Edwards contrasts "the political idea," embracing much more than price phenomena, according to which "discrimination is found only in unequal treatment [e.g., unequal prices]. . . . Undue equality is never subject to criticism." But a single example shows this "equality" to be hopelessly ambiguous, and the statement incorrect. "Equal" tax treatment could be lump-sum equality or percentage equality or after-tax equality or some other equality.

Edwards' "equality" therefore is too vague to exist even as a "political" object. By contrast, the economic concept of nondiscrimination, i.e., the complete concordance of prices and costs, is nobody's ideal; it is simply a definite unambiguous point of reference or departure, essential for measuring and appraising facts. If nothing else, it shows that a mere price difference, or a non-difference, means nothing whatever until we can specify a corresponding cost difference or nondifference. A may pay a lower price than B, and yet be discriminated *against*. Nondiscrimination is like a specified latitude and longitude, while "equality" is a point without coordinates. Nothing can be referred to it, and no implications can be drawn from it. Since equal prices charged to A and B can mean discrimination in favor of A, or of B, or no discrimination, a mere equality, or an inequality, without reference to costs, means nothing. "Discriminatory" applied to price differentials is simply a redundancy; yet this is the book's constant usage. Usually, though not always, Edwards has not told us what he is talking about; we are as free to

supply the meaning in each instance as the Greeks consulting the oracle of Delphi, and there is no way of adding up separate facts for a general statement. If "equality" had some definite meaning (such as income equality), reasonable men might well differ as to whether it was worth its cost, but as things stand we have no idea what if anything we are getting from a price equality law, and no standard for appraisal.

Edwards thinks that the Act thwarts efficiency, and he is deeply concerned with the conflict between "efficiency" and "competition," though without explaining either concept, or why there is a conflict. Yet aside from the special case of natural monopolies, where the market is too small to support more than one or very few firms of minimum efficient size, the conflict does not exist; on the contrary, in an unplanned private-enterprise economy competition is the only means to efficiency. Not considering this, Edwards can only envisage an endless discretionary process, of *ad hoc* solutions in particular markets, government "balancing" competition against efficiency, neither concept explained.

The economic theory which Edwards eschews is that, given active competition, the margin over cost on any given product cannot permanently remain greater than on others, for firms will move as soon as they are able from the lower- to the higher-profit activities. With ever-changing supply-demand conditions, new profit discrepancies are constantly being created and destroyed. The transitory discriminations are incentives for better resource use, because they occur as part of a competitive process which eventually liquidates them.

A single-firm or group monopoly is free of the compulsion to compete away discriminations, and may be able to maintain them permanently for their greater profit. Hence stable discrimination signals a noncompetitive market and usually wastes resources, particularly when customers are business firms, among whom the less efficient may thus survive.

Two examples of price equality will show the gap left by Edwards' lack of analysis. International airline fares, set by a trade association, are roughly the same per mile on all routes. But the cost per mile is of course very much lower on the heavily traveled North Atlantic run, so that there is really discrimination against passengers on it. "Equality" can be argued either way, without end. But "the economic idea" lets us predict that the airlines will try to get passengers on the North Atlantic runs by every expensive device of advertising or service except the forbidden price-cutting. One would also predict that if any group of passengers could charter the use of a serviceable aircraft, they could travel to Europe at considerably less than conventional rates. And so it has turned out. Substitute the food industry for the airlines; the Robinson-Patman Act, prescribing equal prices, for the international airline cartel; the chain stores and other "powerful buyers" for the clubs and other charterers, and you have it exactly. The key to the facts is the "economic idea" for which Edwards has no use, and the "political idea" does not advance our understanding an iota.

Let us now consider the Robinson-Patman case which Edwards discusses—with his usual sound instinct for what is important—more than any other.

Standard Oil Co. of Indiana sold gasoline at $1\frac{1}{2}$ cents per gallon less to jobbers, who bought in large amounts and provided storage and delivery to service stations, than to retailers, who took delivery on their premises in small amounts. Several jobbers who also operated service stations were included in those who paid the lower jobber price. Under active competition, the price of a given product taken under given conditions will be the same to all buyers, regardless of what they in turn do with it. But Indiana Standard was called upon to "justify" this discrimination on the ground that these integrated customers were retailers, and therefore should by right pay the same price as other retailers. What then happened is cited by Edwards as an example of the "complexities" of ascertaining cost. On the contrary: the complex and exhausting rigmarole served to block the recognition of facts, and it enmeshes every person, however able, who cannot stand off and apply to it those methods of economic analysis which Edwards has renounced.

Outside the world of Robinson-Patman, a half-day's research would have settled the matter. There was no dispute that these customers actually did storage and delivery, and that everyone else buying under the same conditions received a $1\frac{1}{2}$ cent discount. Furthermore, there was uncontroverted evidence that this company saved approximately $1\frac{1}{2}$ cents in other areas by selling to jobbers. A reputable economist [1, pp. 222-27] had faced this very problem of applying to one area storage-delivery cost data from another area, and having shown that it was proper to do so, had estimated the cost as between $1\frac{1}{2}$ and 2 cents. The only conclusion compatible with this evidence is that there was *some* cost saving greater than zero, probably no less than $1\frac{1}{2}$ or more than 2 cents, while the best estimate was $1\frac{1}{2}$ cents. But the ritual of "cost justification," costing thousands of dollars and man-days of effort, picking at trifles and losing the forest not for the trees but for the broken twigs, used the formula "no probative value" to hold that the cost saving was zero. In the contemplation of law, storage and delivery were free gifts of nature.

Edwards is bothered by this, of course, and makes no bones of his general dissatisfaction with "cost justification." (Incidentally, one must also respect his willingness to change his published views materially.) But reflection on "the economic idea" in the light of such cases demonstrates that most if not all cost differentials will simply not be recognized by law, so that many buyers are forced to pay wider profit margins to their suppliers. This is particularly interesting because Edwards would like to get rid of the brokerage clause (2c), which makes no pretense of considering costs but forbids unconditionally any payment or allowance even when the services rendered are obvious, and which therefore leads to unnecessary distribution costs. But if cost justification is largely ineffectual, then the Act as a whole is nothing but the brokerage clause repeated several times. (The Robinson-Patman Act subsidizes higher-cost buyers by not letting lower costs be passed on as lower prices.) Any group of cooperating firms will be under constant tension because they will be tempted to sell a little more of the extra-profit items, and to give away some of the superprofit per unit in order to increase their sales of the item. Hence the sellers must either have a firm understanding, or else there

must be some kind of policing, and this is what the Act provides. The incessant attempts to burrow under it, or wriggle around it, are nothing but competition, or wasteful substitutes therefor.

Edwards might well agree on this last point, for he repeatedly condemns the usual Robinson-Patman standard of "injury to competition" as being nothing but protection of groups of competitors. Yet it cannot be avoided in a price equality law. To this "narrow" view, he opposes the "broad" concept of injury to competition itself, but we never are told what this latter means in theory or practice, and in the end both are in effect rejected: "... the objective of establishing equality . . . is not a necessary part of the objective of preserving competition." "... the objective of preserving equal opportunity is an inappropriate focus for a law of price discrimination" (p. 641). Intentionally or not, this destroys both "broad" and "narrow" concepts, since both are based on "the political idea" of "equality." And in a remarkably confused footnote (p. 638), he also rejects "a third objective, that of promoting the . . . efficient use of economic resources," apparently because he thinks it would require a sort of universal OPS, neglecting as elsewhere that it is the competitive process, which the law could safeguard, that would push costs and prices into line.

One wonders what Edwards would think if he were not concerned with something called the "large and powerful buyer," heavily emphasized throughout the book. The nearest we get to an explanation is an unbelievably casual "asset size, percentage of the total product bought, or some similar convenient measure" (p. 650). Edwards assembles (pp. 652-53) an indictment of chain stores which can occasionally be understood, but for which he offers no factual support. What these integrated firms did during the 1920's is simple enough as a matter of economics. J. M. Clark [2] pointed out early in the decade that manufacturers tried to help their established dealers "who [desired] protection against direct buying on the part of retailers" [2, p. 415]. The FTC chain store investigation showed that at most some 15 per cent of the chains' lower selling prices was accounted for by lower buying prices; but little—if any—of this could be called discriminatory in economics because the FTC made no allowance in these calculations for the chains' performance of wholesaling, brokerage, or similar functions. Furthermore, the economist in charge of the investigation wrote elsewhere that quantity and related discounts usually did not cover the full cost saving to the seller [5, p. 44]. The only theory to fit these facts is that the chains managed to get lower prices that roughly corresponded to the costs they saved their suppliers. Of course this "aroused the indignation that led to the Robinson-Patman Act"; hell hath no fury like the man deprived of a vested interest. Be this as it may, surely something more is needed than a comparison of chains' absolute sales in 1927 and 1958 (p. 625), with no attention to the rest of the statistical universe, of which they could have been a growing or declining per cent.

"Power" or "bargaining power" or "countervailing power" among buyers shuffles together at least two very different market structures. A buyer with monopsony power (which the chains rarely had) only discriminates among sellers; this problem is outside the present law, and the book. The ability to thwart discrimination, including Robinson-Patman discrimination, or even to

obtain favored treatment, by contacting enough suppliers and offering enough business to make it worth their while to quote lower prices (discriminatory or nondiscriminatory) is simply an economy of scale, and a response to monopoly in selling, without which sellers' discrimination could not exist. We are back, obviously, at Stocking's two categories. Monopoly either in selling or buying can be attacked by law. But if the purpose is "equality," Edwards has himself destroyed its claims to validity. As a standard of public policy, it is, as the lawyers say of a statute, void for vagueness. Hence the chapter appraising the Act says very little: "That it has substantially reduced the discriminatory advantage in price enjoyed by large buyers" (p. 622) may mean that the Act has brought about more or less discrimination. His chief criticism is that the "powerful buyers" have only been attacked indirectly, hence ineffectively; while small buyers banding together have been hurt. Furthermore, the law reached "beyond the problem it was meant to meet" (p. 617), and has protected brokers, promoted wasteful advertising, and damped down competitive vigor. A better law would give more effect to cost differences, and to the "broad concept" of injury.

The Robinson-Patman Act is an economic phenomenon (as well as much else) and must be analyzed as such. But Edwards has renounced the usual analytic apparatus and replaced it with nothing but "power." "Nothing will come of nothing." Stocking's comment in effect warned that meaningless concepts would lead, and they have led, to meaningless results. For all its length, the book is not informative. It is sprinkled with insights reflecting Edwards' impressive experience and knowledge, but this is all frosting without any cake. An acceptable study of the Robinson-Patman Act has yet to be published.

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The Corporation in Modern Society. Edited by EDWARD S. MASON. Cambridge: Harvard University Press, 1959. Pp. xv, 395. \$6.75; Text ed., paper, \$3.50.

This book is a collection of fourteen new essays contributed by seven economists, three lawyers, two political scientists, a sociologist and a former member of Parliament. The central theme is not the corporation as such but the giant publicly owned concern, its internal control, and its broader social and economic impact. While the essays are independent rather than integrated,

they tend to cluster around a few traditional themes, such as the concentration of economic power, the autonomy of management, and related basic questions that were raised a quarter of a century ago by Berle and Means in *The Modern Corporation and Private Property*. Some of the authors have ranged widely, and the book is broad in perspective, providing insights by students of several branches of the social sciences; the subject matter includes the British corporation, the position of industrial management in Soviet Russia, and the corporate role in international development.

As might be expected, the essays vary considerably in length, quality and content. Some are genuine contributions to our knowledge of the large corporation in its present setting—contributions based on careful and extensive research and scholarly judgment. Others fall short of that standard, and some seem to be largely repetitious of the sweeping generalizations that have plagued the approach to this subject for so long.

It is impossible here to do full justice to all fourteen essays. They cannot be treated as a whole because the subject matter is as diverse as the opinions of the authors. Many seem intent on sounding alarms about the dangers of giant size and all-powerful managements responsible to no one. Most of the alarmist essays dress up old assertions rather than provide a balanced evaluation of them.

In the introductory chapter, E. S. Mason sets the tone of the book as he expresses concern, or even fear, because the large corporation, a permanent part of our economy, is subject to no visible controls that will insure operation in the public interest. He distrusts even the favorable verdict of experience, fearing that the generally good performance of the economy to date might have been fortuitous, offering little assurance for the future. He takes small comfort in the fact that concentration has not been growing in the past 50 years, since absolute size has grown and that, together with irresponsible management, is the nub of the problem. Much of what he has to say is debatable, in light of available evidence, but the issues are forcefully presented.

Several of the essays are first-rate and represent valuable additions to our much-needed knowledge of how the large corporation really behaves. Among these is John Lintner's highly competent research study of corporate financial policies during the past half century, with particular emphasis on the Berle and Means theses of increasing concentration, and how large corporations escape the discipline of the capital markets through internal financing. These theses he finds questionable. The proportion of outside borrowing has been growing in recent years for most industrial groups, and large manufacturing corporations are now more dependent on outside funds than they were thirty years ago. Moreover, the rate of expansion and shifts between bond and stock financing have been pretty much consistent with a market-oriented economy rather than with the hypothesis of arbitrary and autonomous management power.

Jacob Schmookler's essay on technological progress and innovation will also be welcomed as a contribution based on extensive research. He finds that a surprising amount of applied research and development is carried on by individuals and small concerns, with over 85 per cent of the 15,000 industrial

concerns doing research having fewer than 500 employees each. He doubts the need for giant enterprises to stimulate technological development, and he finds research an ally of small enterprise that tends to keep the economy competitive. He questions the wisdom of present patent laws and urges more government support for basic scientific research.

W. Lloyd Warner's carefully documented study of 8,300 top corporation executives dissipates the popular image of the "organization man" who owes his position to circumstances of birth or social connections, and is a mere cog in a bureaucratic machine, with little imagination or initiative. Apparently there is an increasing degree of mobility up and down the executive scale and between enterprises, with more apparent emphasis on ability than hereditary position or connections.

C. A. R. Crosland's essay on the private corporation and the public corporation in Great Britain is both descriptive and analytical. He detects little evidence of increasing concentration in British private industry in the past half century (although British industry is more concentrated than American), and he sees no dangerous degree of private power there. The theory of the public corporation, with its autonomy of operation and final accountability to Parliament, has not in either respect been fully realized in practice.

The position of management in Russian industry is the subject of an informative essay by Alexander Gerschenkron. There have been swings of the pendulum of official policy between extreme rigidity and some measure of managerial freedom. He sees real dangers of such freedom to the dictatorship and for that reason expects the recent policy to be reversed.

Raymond Vernon's essay on some timely international aspects of the business corporation in its relation to underdeveloped areas explores another facet of big business. After examining needs and present tendencies, he concludes that only if business and government join forces are we likely to provide the know-how and capital that are necessary to meet the Russian challenge.

Carl Kaysen's chapter on the power of big business does not get past the stage of assertion. He implies that power is in proportion to the range of choice, and he measures concentration by the indiscriminate lumping together of the public utility, transportation, financial, manufacturing, and mining industries which are responsible for 51 per cent of the national income. He leaves the questionable impression that a few corporate giants exercise almost unlimited power over most of the leading industries, if not the entire economy, creating economic inefficiency, instability, impediments to progress and inequities. He repeats most of the clichés about the power of big business over foreign policy, its control of the press, and its dictation of policies to supine state and local governments. There is none of the careful analysis of market structures or the plain facts of political life that a useful approach to this subject requires. The essay is essentially the brief of the attorney for the prosecution.

In sharp contrast to Kaysen's presumed all-powerful corporation is Neil Chamberlain's portrait of labor unions as essentially conservative and benign, with little monopoly power (or seldom using it), and having few basic con-

flicts with management (except to impress the membership). Prosperity rather than union power is the explanation of wage increases in recent years; and unions are mostly interested in job security, fringe benefits, grievance procedures, and the like. Certainly it is difficult to reconcile the concepts of arbitrary power and responsibility in these two chapters.

The essays by lawyers and political scientists are in the main related to the internal affairs of the corporation, and the distribution, derivation, and use of power. Abram Chayes finds the corporation so all-powerful in modern society that it must be made subject to the rule of law. Presumably it is constrained neither by competition, by stockholders, nor by countervailing power. He suggests more control over corporate directors by labor unions, dealers, and others, rather than by stockholders. Stockholders presumably have the least need for protection—after all, they can sell their stock! Chayes does not pursue the economic implications of his suggestions.

Kingman Brewster, Jr., explores analogies between corporations and sovereign governments and finds that the former do not have the checks and balances necessary to the democratic process. He would limit corporate size to the requirements of economic efficiency to gain the advantage of dispersion in decision making, thus preserving the social value of the small concern in a sort of "economic federalism."

In a well-reasoned essay, Eugene V. Rostov correctly notes that stockholders' influence or control over management is by no means a rarity, although they do not use their franchise as effectively as the political citizen. With refreshing insight he perceives the dangers of the "new capitalism" in which public-conscious management conceives itself as responsible to everyone—stockholders, workers, customers, dealers, and all—and no longer seeks to maximize profits. He concludes that, both in law and economics, management is likely to perform best when it follows traditional profit motivation.

Earl Latham's efforts to squeeze the large corporation into the mold of the sovereign state appear strained, over-drawn and unrealistic, although some similarities do exist and have been long noted.

Norton Long contributes a realistic essay on the place of the corporation in the local community. He suggests that managers have become sensitive to local needs and are jealous of good public relations, avoiding the older positions of dominance and paternalism. On the whole, they are likely to have little local political influence—much less than labor unions and other groups.

These brief comments indicate the range of subject matter and quality found in the essays. In the quarter century that has passed since the publication of Berle and Means, a large number of useful and competent studies of the large corporation and concentrated economic power have appeared. Many of these cast serious doubts on the validity of some of the sweeping generalizations of that challenging book. Moreover, as Berle notes in his foreword to this new volume, many new laws and regulations have altered in some degree the power of the large corporation and its management. Yet, much of the advance seems to be inadequately reflected in some of the essays.

Students of corporate enterprise would welcome a new synthesis that embraces the research of the past thirty years. This is not that synthesis; al-

though some of the essays, without doubt, would qualify as contributions to it. More than half of the volume deserves a wide audience both in college classes and among the general public. And even those parts of the remainder that seem inaccurate, oversimplified, or unconvincing contain thought-provoking ideas that provide springboards for discussion. It is not basically a book for beginners, who need something more systematic and who lack the background to separate the wheat from the chaff. It can be used to real advantage for reference or as a supplementary textbook where thought-provoking ideas are more important than widely accepted conclusions.

A word must be said about the quality of the writing: Almost without exception, it is excellent for this type of book. The style is crisp, direct, brief, and sometimes brilliant. If the book was designed to stimulate controversy, the issues are presented with vigor. But it would have been improved by more emphasis on objectivity and balance.

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Integration and Competition in the Petroleum Industry. By MELVIN G. DE CHAZEAU and ALFRED E. KAHN. Petroleum Monograph Series, Volume 3. New Haven: Yale University Press, 1959. Pp. xviii, 598. \$7.50.

The petroleum monograph series which is sponsored by a grant from the American Petroleum Institute finally contains a work of solid merit. Whereas the first two items in the series¹ were generally disappointing, in this third volume de Chazeau and Kahn have made an important contribution to literature concerning the oil industry. Their book is a detailed and penetrating analysis of vertical integration in that industry; in addition, it essentially redoes a good deal of the work attempted in the first two volumes—on pricing and conservation—and does it better.

The broad questions posed for analysis are whether the present form of organization of the petroleum industry is essential to provide an adequate supply of petroleum products now and henceforth, and whether the observed massive vertical integration and horizontal concentration are compatible with the competitive objectives of our society. More specific general inquiries concern whether this integration is a necessary and desirable consequence of the characteristics of the industry, whether integration and concentration have gone further than efficiency requires, what are the conditions of workable competition in petroleum, and whether the American proration system is the best road to conservation. These latter questions suggest that the book is to be a good deal more than a narrow study of integration, and this is indeed the case; it is effectively a study of operation of large integrated firms in the petroleum industry, and of the industry in which they operate. In the authors' words, "this study represents a search for judgment as to whether the combination of public controls, private policy, and competition in the petroleum

¹ Ralph Cassady, Jr., *Price Making and Price Behavior in the Petroleum Industry*, 1954; E. W. Zimmermann, *Conservation in the Production of Petroleum*, 1957. Both volumes have been reviewed in this journal—the Cassady book, Sept. 1954, 44, 691-95 and the Zimmermann, Dec. 1958, 48, 1066-67.

industry has produced and is likely to continue to produce socially acceptable behavior and results."

In spite of the breadth of their general inquiry, however, the authors do repeatedly come back to the narrower question of how much difference does vertical integration per se really make with respect to this or that phase of industry behavior. The reviewer found the attempts to answer this question in various contexts slightly less satisfactory than other parts of the book, perhaps mostly because the question itself is somewhat akin to those of how much difference the hydrogen makes in an organic compound built up of hydrogen, carbon, and oxygen, or how much difference the front wheels make to an automobile.

The book is broken into four main parts, of which the first is introductory and deals with the general structure of the industry and with the economic and historical setting of its vertical integration. Part II is concerned broadly with crude oil production and conservation and their relation to integration; and in a series of excellent chapters discusses regulation of oil production by state authorities (prorating), the pricing of crude oil in the domestic market, its pricing in the world market, and the role of imports in the making of domestic production and pricing policies.

Part III considers the alleged advantages of vertical integration in the "non-price" areas of policy and competition, with especial attention to the planning and execution of investment and innovation at various levels in the vertical sequence of industry operations. Part IV analyzes competition in the refined-product markets and the effects of vertical integration thereon, giving considerable attention to the impact of size *cum* integration on product prices and to the competitive places of nonintegrated, "independent" refiners and marketers. It concludes with an all too brief summary chapter concerning the over-all effect of vertical integration on petroleum-industry competition and concerning the general workability of this competition.

Covering a great deal of territory in less than 600 pages, the book would not be expected to be, and is not, one based upon and reporting results of extensive primary research. It is based, however, on an extremely comprehensive reading—and, better yet, a good critical understanding—of voluminous relevant secondary materials. A great deal thus depends on the general analytical ability and good judgment of the authors. Although the reviewer occasionally disagreed with their judgment, he would rate them high on both counts. And he was especially impressed both with their ability to cut through the trivia in order to concentrate on essential issues, and with their objective, dispassionate, and unbiased approach to a range of quite controversial questions.

The book is so packed with factual and interpretive content (it definitely "reads long") that it is only possible in a brief review to suggest its flavor by characterizing a few leading conclusions. As to the reasons for integration, the authors conclude after extended discussion that whereas early vertical integration by the old Standard Oil (up to 1911) had a fairly clear monopoly motive, the major rationale of integration since has been stabilization of markets and prices, or a defense by the firm against inherent market instabilities, and

the capture by the firm of secure and dependable sources of supply for refineries and dependable outlets for their products. Both competitive and monopolistic considerations enter here. Since the introduction of prorationing in the 1930's, dampening of the instability of crude supplies and prices has become less important, but integration of secure crude supplies has become more so. Except with reference to the early days of the industry, the authors pay relatively scant heed to possible efficiency-increasing effects of vertical integration in oil, and certainly do not assign it a major role in the explanation of such integration.

The historical emergence and characteristics of the control of oil output through state prorationing are nicely covered in a very sophisticated chapter. The strategic role of Texas oil and the Texas regulatory commission in stabilizing American domestic oil supply and raising and stabilizing crude oil prices is made extremely clear, as is the indirect influence of the integrated major oil companies on the policies and decisions of the Texas commission. As to the domestic pricing of crude oil, arguments to the effect that oil prices must and do respond in any necessary or systematic way to the long-run costs of finding oil are considered and appropriately rejected; instead it is emphasized that the interaction of the decisions of the Texas commission and the market power of the major integrated firms essentially makes and stabilizes American crude oil prices at a quasi-arbitrary level.

The making of foreign oil prices, their shifting relationship to Texas Gulf prices, and the predominant role of integrated-firm market power plus political considerations in fixing world oil prices and determining imports to the United States are explained. And it is lamented that the pivotal Texas price is more influenced by a casual consideration of domestic cost than by a competent appraisal of the world demand-supply situation. Crude-oil integration by large oil firms in conjunction with domestic production control is considered a bad thing. The integrated majors think like producer-sellers rather than buyer-sellers of crude; there is a corresponding lack of a strong bona fide buyer interest in proration deliberations; and the result is an undue pressure to elevate domestic crude prices and maintain them at elevated levels. Meanwhile, untoward conservation results ensue: proration is clearly inferior to the rejected alternative of compulsory unitization, and the policy of drawing heavily on expensive domestic oil while shutting off cheap foreign imports is poor conservation without convincing national-defense justifications. Unitization instead of proration, and an abolition of quota limits on imports, are favored.

The section on investment and innovation in relation to integration is much less satisfactory than the extremely strong one on crude oil. It is concluded on balance that integration may favor progressiveness in technique and product, and encourage adequate pipeline investments, although it does perhaps implement the incursion of excessive costs of distribution and selling. The section as a whole, though sensible, is strongly taxonomic in its approach, unduly general, and rather inconclusive in its outcome.

The ensuing discussion of product markets and prices is in general quite competent. It is appropriately recognized that "spot" markets have been

systematically overrated and that major-company tankwagon prices are the leading instrument of price policy—also that whereas price leadership in making these prices for gasoline may be somewhat barometric, the prices arrived at are not necessarily purely competitive ones. Nonetheless, there is a considerable degree of intermajor competition in the product markets, for which the incompleteness or imbalance of the integration of many majors must be thanked. (Over-all balanced integration would probably be anticompetitive in the net.) The current general picture is that existing price-making in the product markets does not engender monopolistic margins or raise product prices much farther than they have already implicitly been pushed by crude output control under prorationing. Following this discussion, there is a rather perceptive analysis of the evolving roles and declining numbers of independent refiners, ending with the observation that unitization without proration, plus freedom of oil imports, would do a great deal to restore and sustain the competitive position of firms in this category.

In general conclusion, the authors feel that there is “a workable balance between monopolistic and competitive aspects of this industry’s structure and performance—beyond the crude oil level.” Integration per se is not held to be the real culprit in the case against monopolistic tendencies; in any event, monopoly power stems from horizontal size rather than vertical integration. The blame must be placed on prorationing, the Texas regulatory commission, the policy with respect to oil imports, and the operation of integrated firms in areas much broader than existing regulatory jurisdictions. These things combine with integration, and especially crude-oil integration by the major refiners, to engender quite unsatisfactory results. But the remedy of vertical divestitures should be rejected, or at least postponed until the preferred remedy of eliminating prorationing, instituting unitization, and allowing free imports has been given a fair trial. These are broad and sweeping conclusions, but they have been adequately qualified in the supporting chapters.

The reviewer has a few quarrels with the authors. For example, insufficient attention is given to the possibility that the major firms may elevate refinery-distribution margins by systematic price discrimination among various types and grades of refined products; and the over-all policy prescription, while attractive, is not necessarily very practical. In general, however, this is a strong and important book, which should at once please the experts and be especially appreciated by students who want to learn a great deal about an important industry in a short time.

JOE S. BAIN

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Le pouvoir de monopole. Essai sur les structures industrielles du capitalisme contemporain. By JACQUES HOUSSIAUX. Paris: Sirey, 1958. Pp. iv, 416. 3,800 fr.

Professor Houssiaux’s essay on monopoly power consists of two books. Each book is divided into two parts (*titres*) and each part is further divided into two chapters and a number of sections. Monopoly power (*pouvoir de monopole*) is defined as the resultant of two elements: the economic power of the

firm (*puissance économique*) and its behavior pattern (*comportement or action stratégique*). The first element is determined by the degree of concentration. The most important ingredients of the second element, the firm's behavior pattern, are: price behavior, decisions with regard to variety and quality of products, attitude towards innovations, sales efforts (*comportement publicitaire*), and conditions of entry (pp. 2-3, 65-71). The *comportement* thus is the exercise of its economic power by a firm. Only the conditions of entry do not quite seem to fit into the list. What Houssiaux apparently has in mind is the firm's decision to use its economic power in order to restrain potential newcomers from entering the market. For the purpose of empirical investigation of economic structures and of monopoly power, the author falls back on measurements of the degree of concentration. This he justifies by an extensive analysis of the relation between the degree of concentration and the monopoly power of the firm which indicates that on the whole monopoly power varies directly with the degree of concentration.

The first book (177 pp.) deals with theoretical systems of market classification and with measures of market imperfection in terms of demand elasticities and cross elasticities (Part I); and with the concepts and the methods of measuring concentration (Part II). It is a very complete and painstaking survey of the major contributions to monopoly literature of M. Adelman, J. S. Bain, R. Bishop, E. Chamberlin, W. Fellner, N. Kaldor, A. P. Lerner, A. Papandreou, K. W. Rothschild, R. Triffin, *et al.* This analytical digest of a quarter century's discussion will be of great value to French economists unable to read English. To the English-speaking economist it has little to offer. Incidentally, this very fact raises the question of the near-monopoly position held by English and U. S. writers on monopoly power. Houssiaux apparently fails to see that there is a problem which is at least indirectly relevant to his investigation.

The second book is devoted to an empirical study of the degree of monopoly in the French economy, supplemented by a comparison of the development of the French and the U. S. industrial structures since the beginning of this century. It is quite independent of the first book. Houssiaux states (p. 181) that his empirical study "requires methods of research entirely different" from those of the first book, without, however, indicating why this is so. At any rate, these methods consist essentially of simple statistical measures of concentration, historical description, and some case-study techniques. Judged by itself, the second book is an excellent investigation of the present state and of the evolution of competitive conditions in France. Part I is devoted to a comprehensive verbal description of market structures and competitive conditions in the main sectors of the French economy (Ch. 1); and to a broad statistical analysis of the prevailing degree of concentration by firms and by plants (*établissements*) using number of employees, gross revenue and gross assets as criteria (Ch. 2). Chapter 1 of Part II is concerned with the historical evolution of effective monopoly power in France and—for the purpose of comparison—in the United States. The examination of the major relevant developments is subdivided into a verbal discussion of the critical factors determining monopoly power (size, number, and interde-

pendence of markets, innovations, technologically determined size of the enterprise, institutional framework) and a statistical analysis of the changes which have occurred in the competitive conditions in France (1912-52) and in the United States (1899-1947). This analysis proceeds by measuring changes in the degree of concentration by industrial sectors and industries using value added (for the United States), output, gross assets and changes in the composition of the group of the 100 largest firms as main criteria. The final chapter contains a detailed, well-documented investigation of the role and the effect of mergers on monopoly power and a statistical study of the merger movement in France (1919-1954) and the United States (1887-1947).

There is nothing really new in the methods of the analysis. What is new and of great value is the systematic and careful application of these methods to all available French statistics and to supplementary descriptive evidence. The investigation indicates that unlike the industrial structure in the United States and in England, the French economy is still largely dominated by quite small enterprises: technological concentration has been slower here than in other industrialized countries. On the other hand, monopoly power measured in terms of concentration on the firm, rather than the plant (*établissement*) level has increased in France since the beginning of the century (while during the same period it has remained stable or has even declined in the United States). In the light of these findings Houssiaux formulates the interesting hypothesis that it is precisely the lag of technological concentration which has led in France to horizontal and vertical integration; that is, to the emergence of large firms owning or otherwise controlling groups of small enterprises, and to collusive arrangements. In this development mergers have played an important part. In order to make the French economy more competitive, the author suggests that technological concentration must be stimulated, so that the "most dynamic enterprises may grow to a size consistent with the most efficient production" (p. 296). In addition he recommends, of course, stricter legislative and administrative controls.

By raising the question of why the technological concentration in France has lagged so far behind that in other industrial countries, Houssiaux might have provided his results with a firmer understructure. Indeed, this is one of the central questions of French economic development since the 18th century and possibly since the gold and silver inflation of the 16th and 17th centuries. But an author should not be blamed for the limits which he chooses to impose on a study and for leaving some work for others to do. As it stands, Houssiaux's general hypothesis offers a number of new and important insights into the processes which formed and continue to form the structure of the French economy.

The major defects of this study are in the form of presentation. The book is poorly organized, and the writing is often verbose and repetitious. The total absence of cross-references and of a summary of major conclusions in a work of this length puts a heavy burden on the reader. There is no index. There is also an excessive number of misprints, some of them quite disturbing. On the asset side, there is an excellent and very complete bibliography of

the literature on monopoly. Whatever the irritating defects of the presentation, Houssiaux's study, and in particular its second book, represents an important and welcome contribution to monopoly literature.

EMILE GRUNBERG

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Die Eisenbahntarife im Güterverkehr. By NORBERT KLOTEN. Tübingen: J. C. B. Mohr (Paul Siebeck), 1959. Pp. 224. DM 24.80.

This book is the thirteenth volume of the publications of the List Gesellschaft in Basel, Switzerland. Under the editorship of E. von Beckerath and E. Salin, this society has published a number of outstanding studies concerning a variety of important contemporary economic problems..

Mr. Kloten's book is one of the few publications originating in postwar West Germany about railroad rates. The author attempts to establish a theoretical foundation for the determination of railroad rates; but little is said about policies actually pursued by railroads. The American reader who wishes to learn, for reasons of comparison, how railroad rates are actually made in West Germany may therefore be slightly disappointed.

In the first of three parts, "Foundations," the author deals initially with production and cost structure of railroads, emphasizing especially such features as simultaneous, alternative and joint production. Several pages are devoted to the well-known problem of fixed cost; some cost data are furnished, mainly referring to the former German *Reichsbahn* or the *Bundesbahn* of the Federal German Republic. (Throughout the book the author refers to German railroads, though a few interesting international comparisons are made.)

Part I then turns to demand—first the problem of demand for transport in general—then specific features of the demand for railroad transport. Considerable attention is given to the price elasticity of railroad transport demand. The problem of the relation between the level of national income and the levels of demand for transport in general and railroad transport in particular is mentioned but not investigated in detail. Part I closes with an attempt to explain the market behavior of railroads in terms of the von Stackelberg classification of market forms (which the author follows closely). The author recognizes, however, that market behavior may be conditioned not only by the nature of the market form, ranging from perfect competition over oligopoly to monopoly, but also by considerations of "the general public interest" as well.

Part II is lengthy and deals in great detail with railroad rates, their structure, classification and particular characteristics. The author thoroughly investigates what the conceptual elements of railroad rates are (*Elementar Tarife*) and how these can be combined into a rate system. The approach in this part is highly taxonomic and less interesting for the economist who looks rather for meaningful relationships than for new definitions and precise classifications.

Part III, dealing with "rate systems" (railroad rates proper), is more interesting. After some comments on older German concepts about railroad rates

(with reference to the writings of F. Ulrich and E. Sax), the features of a rate system which aims at maximizing profits are discussed. The analytical tools are those of conventional marginal analysis. Using a number of simplifying assumptions and with some resort to *ceteris paribus*, the author shows under what conditions a railroad—which can split the demand for its services—can maximize profits.

He suggests that an alternative aim of a railroad may be to cover total cost. If this should be done in a socially optimal way (assuming competition) price must be equal to marginal cost and average cost. It is assumed that average cost is falling and that it will rise steeply once the existing equipment is used nearly to capacity. A railroad will not under normal circumstances—for lack of a large enough demand for its transport services—use its equipment to such an extent that marginal cost is equal to average cost. Operating in the range of falling average cost poses then the problem how to cover the gap between marginal and average cost. A possibility would be to maintain marginal-cost pricing and subsidize the railroad through the budget, which is the theoretically appealing solution analyzed by Hotelling. The author does not go into this but investigates the more practical way of setting rates higher than marginal cost. The condition imposed is that total cost must be covered with a minimum average rate level. To achieve this it is necessary to split the demand for transport services according to categories of goods transported and “charge what the traffic will bear.”

After the discussion of these two basic alternatives, some of the stringent assumptions are relaxed and the author deals in a descriptive way with the problem of competition of other carriers (inland waterway shipping and trucks). The book ends with observations on locational implications of railroad rates.

Kloten's book reflects a desirable revival of the interest of German economists in problems of railroad rates. His application of conventional marginal analysis to problems of railroad transportation is, considering the state of traditional German theories about railroad rates, a laudable endeavor; but he is fully aware of the limitations of the marginal analysis in this field.

A few remarks may be made about the author's contention (p. 171) that, from the point of view of the total economy, no rate system is as favorable as that which attempts cost coverage through minimum average rate levels by means of price discrimination. The fact that a railroad can cover cost—especially if this is done through price discrimination—evidently does not enable us to make statements about the quality of resource use and general welfare. To substantiate his position the author would have to supply the difficult proof of the proposition that price discrimination improves both resource allocation and general welfare. It is a pity that he did not investigate the problem of marginal-cost pricing and subsidization of railroads in detail. If this had been done and due attention been paid to excise tax features of a railroad rate, the author might have reached a somewhat different conclusion regarding a most favorable rate system.

A general criticism of the analysis is that it is presented in too narrow a conceptual framework. There is too much emphasis on the railroad as a firm

and too little on its important position in and its impact upon the total economy. But the analytical content of the book is the better part. Less inspiring are the history of ideas about railroad rates. The reader is exposed to a variety of concepts, classifications and definitions offered by various writers (most of them German) in this field. In the opinion of this reviewer this could have been much shorter without loss to the really important things the author has to say.

All chapters are well documented and there is an extensive bibliography of the important German writings in this field.

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Land Economics; Agricultural Economics; Economic Geography; Housing

Economics for Agriculture: Selected Writings of John D. Black. Edited by JAMES PIERCE CAVIN. Cambridge: Harvard University Press, 1959. Pp. v, 719. \$12.00.

The main feature of this book is: it pays tribute to a person who made tremendous contributions to the development and knowledge of agricultural economics in particular and to the subject of economics in general. As a tribute to John D. Black's contribution, the book was indeed timely. His life ended shortly after publication.

Black covered an extremely wide range of subjects and analyses in his writing and teaching. The book has been organized accordingly. It includes selections of his writings not only in the individual fields of specialization in the broad area of agricultural economics, but also in political science, economic theory and world economic development. The book has been divided into thirteen parts, each including some of Black's writing in the particular area. The first part includes a portrait of J. D. Black written by J. K. Galbraith in his most lucid style. Each subsequent part includes an introduction by one or more of Black's former graduate students.

The areas covered include: development of production economics, economics of land use, economics of forestry, labor and tenure, marketing and cooperation, price analysis, consumption and nutrition, population and world food supply, agriculture in the national economy, agricultural policy, political science and the public interest, research methods and economic theory. In addition to an introduction by one of his former students, each part includes two chapters from Black's writings. The introductory chapter of each part includes an analysis of the setting and time when Black did the writing, with an interpretation of Black's interest and contribution. But the introductions extend beyond these interpretations and summarize developments in research and methodology in the particular subject-matter areas. For example, in Part II on production economics, Sherman Johnson and Kenneth Bachman begin with the origin of farm management by Spillman, Hayes, Boss and Warren. After discussing the research tools used by these persons, they discuss the trend towards use of economic principles by such persons as Black, Taylor,

Ely and others. They summarize the reorientation of production economics research in the 1920's, its modification during the war and depression and the "new ferment" coming from younger workers relying more on basic economic and econometric tools in the postwar period. Johnson and Bachman also discuss some of the problems in research and methodology to be anticipated in the future.

In the introductory chapter on economics of land use, to take another example, Marion Clawson reviews the development of concepts in and definitions of the field. He discusses important variables affecting land use and devotes a short section to the contribution of Black. In the part on consumption and nutrition, Willard Cochrane traces Black's contribution here, as well as the thinking of persons such as Sir John Boyd Orr, H. R. Tolley, Margaret Reid and others. Similar broad treatments of Black's contributions and development of thought in particular fields are included in the introductory chapters by Black's former students who served as editors.

Most of the selections from Black's work are from journals and other articles which contain ideas not presented by him in his numerous books. These articles, plus the interpretations given them, often relate to modern economic problems of agriculture, as well as to the problems which existed at the time of the original interpretation. Numerous problem solutions suggested by Black are being suggested at the present time, thus indicating the permanence of both problems and alternatives in their social treatment as well as the fundamental nature of analyses by Black.

Fortunately this tribute to John D. Black was gathered together before his death. No other person will likely contribute as much to a particular field of economics as he did. However, it is much more than a tribute. The numerous editors not only pulled together a systematic group of Black's papers, but also summarized, in an excellent way, important developments in agricultural economics, research methodology, and progress towards solution of important and continuing problems.

EARL O. HEADY

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Toward a New Energy Pattern in Europe—Report of the Energy Advisory Commission, O.E.E.C. Paris: Organisation for European Economic Co-operation, 1960. Pp. 125. \$1.50.

This report is the latest analysis of the Western European energy situation sponsored by the O.E.E.C. Based on a study carried out by an international group of experts headed by Professor Austin Robinson, it succeeds the well-known Hartley Report, now almost four years old. In the tradition of energy studies, the Robinson Report forecasts future requirements for energy and then considers how these needs may be met. The empirical estimates serve as basis for policy recommendations which are, of course, the real heart of the Report.

Economic expansion in Europe will be accompanied by further rapid growth of energy consumption. The forecasts, approximately a 30 per cent increase

in primary energy consumption from 1955 to 1965 and again from 1965 to 1975, are of the same general magnitude, slightly higher in fact, as the estimates of the Hartley Commission, despite the Suez crisis and the recession which have intervened. Indigenous European energy supplies are also growing but it is difficult to forecast quantitatively which sources of the energy will actually be drawn on. The Hartley Commission, working in a setting of energy shortage, could estimate the indigenous production potential and then view the residual as an import gap. But now the problem has become one of competition. Energy, domestic as well as foreign, is in surplus and more and more consumers have been turning to that source of supply which is economically most advantageous.

With regard to supplies, the Robinson group deals with broad magnitudes. It assumes a growing but limited potential for hydroelectric power, and somewhat more rapid development of indigenous natural gas and indigenous petroleum than had previously been foreseen. Nuclear power, which will not be competitive until the 1970's on the basis of a realistic appraisal of its capital cost, will until then make only a small contribution to the total energy supply. The coal industry, until recently the focus of European efforts to boost indigenous energy output, now finds itself with a large oversupply. The problem of coal is basically one of high cost, and steps to improve the position of the industry by closing marginal mines are consistent with some reduction in its output.

This evaluation of indigenous energy leads the Robinson group to anticipate a sizeable and growing need for imports, principally of crude oil and some coal, though natural gas presents a visionary but yet not fully tested potential. These imports, amounting perhaps to 32 per cent of European energy consumption in 1965 and 39 per cent in 1975, will be considerably greater even than had been forecast by the Hartley Commission. The latter expressed considerable concern about this "energy gap" and urged the rapid development of indigenous sources of energy. The Robinson Report, on the other hand, views the growing European import requirements for energy as a normal development, not significantly more serious than similar "gaps" in food supplies, in textile materials, etc. The Robinson group does not see any danger of a persistent long-term shortage of energy. It finds that Europeans will be able to pay for their vast energy imports and that security may be better achieved by drawing on a diversity of supply sources than by development of high-cost indigenous resources.

This means simply that Europeans should consume that energy which costs least, that interferences with pricing and other restrictions on energy use should be eliminated, and that the full costs of developing indigenous sources of energy should be recognized. Only two exceptions are considered. One is the case of the coal industry which requires time and financial aid to effect a technological and social transition. The other is the limited development of commercial nuclear energy which may be viewed as a purchase of knowledge in a new technology.

What then are the critical changes which account for the reorientation in

point of view reflected by the Robinson Report? Certain significant changes have indeed occurred: (1) There has been a realization that there is a surplus of energy. Large new discoveries of petroleum and natural gas have been made recently, but even prior to these discoveries, reserves and potentials were known to be very extensive. In fact, the plentiful availability of energy has become apparent on the market place. Imported fuel oil and coal are now in many parts of Europe cheaper than indigenous energy. (2) New discoveries of oil and gas outside the Middle East have reduced the dependence of European consumers on one, politically vulnerable, source of supply. (3) The balance of payments of most European countries is now greatly improved.

These changes in the setting of the European energy economy, considered in some detail in the Report, are essential to its policy recommendations. But the Report is not simply the result of these changed circumstances. On the basis of the same evidence, other groups have argued for increased protection of domestic resources. Protectionism in the field of energy is far from a dead issue in Europe. The Robinson Report is a welcome expression of liberal principles at a time when Europeans are giving serious thought to energy policy and to its coordination.

F. GERARD ADAMS

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Copper Costs and Prices: 1870-1957. By ORRIS C. HERFINDAHL. Baltimore: Johns Hopkins Press, for Resources for the Future, 1959. Pp. xi, 260. \$6.00.

The author has investigated in detail the annals of the copper industry for a period of almost 90 years. He wanted to test the generalizations of mineral economics by learning whether the accidental discovery of new sources of mineral wealth and government subsidies for development have resulted in discontinuity in introducing new productive facilities or whether the processes of investment are systematic and continuous, responding to the signals of profit and loss like competitive nonmineral industries.

Direct determination of costs is of course difficult because accounting data often conceal the costs of discovery of new deposits. The author has therefore chosen to approach the subject indirectly by the use of deflated prices. He argues that in the absence of collusion, competitive prices are equivalent to costs. To eliminate periods of noncompetitive prices, he undertakes an examination of the degree of competition and concentration in the industry—an illuminating study to which he devotes about half the volume. According to his findings the periods of collusion are clearly separated from the periods of competition. He proceeds then to develop a table of historical prices, classifying individual years as normal or abnormal and considering as normal the years in which there is competitive pricing. From the normal years he eliminates not only years in which price manipulation is known to have occurred but also years in which war or depression exercised extreme influences on demand or supply.

Even if the author's methods made it possible to eliminate individual years

as not normal—and this remains open to question—economists may well argue that the residual prices are not equivalent to costs in the sense the author intends. Nor does his method throw light on the behavior of profits and losses.

The elimination of abnormal years is a problem because war and depression are not coterminous with the calendar. They result from complicated antecedent causes and leave behind an inheritance which changes the nature of succeeding events. Similarly, despite much convincing evidence that concentration in the copper industry has resulted from changes in leadership of the big units, it is hard to agree that the author is quite secure in characterizing individual years as competitive or noncompetitive. The nature of evidence about competitive practices in the United States makes it difficult to interpret; industry spokesmen are necessarily less than candid about the price-making process. Although he has weighed this evidence with care and attention to detail, his conclusions must be looked at as tentative, representing a novel approach to a difficult problem.

Accepting the author's method and assuming it is possible to pick out individual years that are normal in his meaning, we have in the whole span of 90 years, only 49 he believes can be called normal. Even within the 49 are 7 which he does not exclude although they involve the operation of Copper Exporters (1926-1928) and the international cartel (1936-1939); he judges these two episodes to have had but a limited effect on price. The 49 years also include 14 based on London prices, the argument being that during these 14 years United States prices were dominated by the Michigan combine. Of the 49 years, excluding the 14 and the 7, none comes after 1925. For the last year, 1957, the author uses, in lieu of "cost," a range of what he considers to be representative industry opinion of an adequate long-run price.

Difficult as it is to characterize a great industry with neat generalizations, the present detailed examination of the periods of price manipulation, the behavior of various organizations of producers, the measurement of the degree of concentration—including comparisons with other industries, and the shifts in geographical distribution and rank of producers, add importantly to our understanding of the push and pull of actual industry. For example, in the period of 90 years, there has been substantial change in the sources of the world's copper supply; new areas have come into production as old areas have tapered off. Efforts of organized producers have encountered serious obstacles in deciding upon an agreed basis for restriction of production because of the changing constituency of national producers and even of individual companies. During periods of collusion, producers characteristically fail to control additions to producing capacity so that they have never exercised enough control to increase prices substantially. In general then price manipulation has not been cumulative but discontinuous and episodic.

In looking at the history of deflated copper prices, the author concludes that prices have been relatively stable. Important technological improvements about the time of the first world war resulted in substantial economies which were passed on to consumers. Had it not been for a declining rate of growth in the demand for newly mined copper, the deflated long-run price of copper

would have increased after the first world war. This decline in demand resulted from the increasing importance of copper recovered from old scrap and the large decline in the relative price of aluminum. The conclusion is that discovery, development, and production of copper are systematic. While there may be accidental discoveries of new sources, they occur in a field which is so broad that their influence does not dominate. The estimate of long-run price is high enough (and near enough to present levels) to attract sufficient exploration and bring into production new areas as needed. If we may judge by projecting past experience, we need have no fear that we shall shortly run out of copper.

ELIZABETH S. MAY

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Labor Economics

Labor in a Free Society. Edited by MICHAEL HARRINGTON and PAUL JACOBS. Berkeley and Los Angeles: University of California Press, 1959. Pp. xi, 186. \$3.00.

This book is the result of a conference held in May 1958 at Arden House, Harriman, N.Y., under the sponsorship of the Basic Issues Program of the Fund of the Republic. Its theme was apparently suggested by the then pending plans for new federal legislation to regulate the unions, plans which have since become realized in the Landrum-Griffin Labor Reform Law of 1959.

Opening with a foreword by Clark Kerr which stresses the importance of clarifying basic social issues through many-sided discussion, the book contains seven papers which were delivered at the conference, followed by a brief summary of the principal points made in the discussions.

As to the papers, three were given by men from the academic ranks in the United States: psychoanalyst and author Erich Fromm, the late Harvard economist Sumner Slichter, and Archibald Cox of the Harvard Law School; two others by experts from different walks of the practice of labor relations in the United States: David A. Cole, one-time director of the Federal Mediation and Conciliation Service, and Arthur J. Goldberg, counsel for the United Steel Workers of America; finally two more by citizens of the British Commonwealth: Hugh A. Clegg of Oxford University, and James R. McClelland, Australian labor attorney.

With the exception of the paper by Fromm ("Freedom in the Work Situation") which raises—though naturally does not answer—broad questions as to the meaning of "freedom" and its role among other social and human values, all papers deal quite specifically with the problem of whether and in what respects legal regulation of trade unions is desirable. Summing them up briefly and all-too-incompletely: Slichter ("The Position of Trade Unions in the American Economy") finds, in part, that collective bargaining by the powerful unions which have developed in the United States may eventually need government regulation unless unionism is capable of transcending itself in the direction of broader social goals. Cox ("The Role of Law in Preserving Union

Democracy"), insisting on the need of legally safeguarding the rights of individuals and minorities in the interest of sound unionism, appraises existing and proposed legislation for the regulation of internal union policies. Cole ("Union Self-Discipline and the Freedom of Individual Workers") warns against facile analogies between political and union democracy, claiming that, with collective bargaining the primary purpose of unions, the rights of individual workers, are not under all circumstances a prime consideration. As to dealing with corrupt union leadership, he thinks that legally to strengthen the powers of the AFL-CIO federation might be wiser than to increase direct governmental powers. Goldberg ("A Trade-Union Point of View") argues against all government regulation of internal union affairs which go beyond the "setting of limits to injustice"; regulations which try to do more than this are likely to hinder the growth of unionism in the many areas where it is still very weak, and altogether constitutes an essential loss of freedom to society. Clegg ("The Rights of British Trade-Union Members") asserts that for a variety of historical and sociological reasons the internal policies of the British unions do not seem to be in need of government regulation; while McClellan ("Experiences of the Australian Labor Movement under Government Control") argues that in Australia labor-management relations in general and internal union policies in particular are and must remain subject to the democratic governments—which in fact are often labor governments—of that country.

It is evident that the collection of these papers with their many-sided, well-argued views based on thorough knowledge of facts and rich personal experience, and with many additional points of view brought out in the discussions, will be most valuable to any reader who wants to form an intelligent opinion about these problems, and of particular value also as reading material in connection with courses in labor legislation, trade union developments, and similar ones.

As regards the "Basic Issues Program," however, this symposium seems to prove that the question of legal regulation of trade unions in a free society is really not a basic issue but rather one of expediency only ("expediency," according to Webster, is "that which is suitable to the ends in view"). Given the basic freedoms of a democratic society, the extent and nature of governmental regulation of trade unions can and do vary greatly with economic, social, and historical circumstances: they do differ widely as between free Great Britain, free Australia, the free United States, and still other free countries; and they also differ widely between the United States of 1935, of 1960, and presumably of 2000 and beyond.

More truly basic issues which may come to be increasingly in need of clarification, on the other hand, might be those which remained more or less peripheral at this conference: the question of whether "freedom" is necessarily an absolute social value, or whether it is meaningful only in the context of other, more basic issues such as the purposes, human and social, of a society.

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Understanding Labor Problems. By DALLAS M. YOUNG. New York: McGraw-Hill, 1959. Pp. xii, 477. \$7.95.

Professor Young states in the preface that his book "has been written for anyone who wants to understand labor problems. It is an attempt to present professionally sound material in a clear, informal, and somewhat conversational style" (p. ix). He further claims that if one is a supervisor the book is designed for him, that if one is a labor leader the book is written for him, and that if one is a college student the book will provide him with "some insight."

Apparently the only people for whom the book was not written are professors of labor economics or professionals in the field of labor relations. Young candidly admits that if one is either or both he knows "far more than is found herein" (p. x).

It is perhaps not fair to review such a book in a professional journal unless the reviewer keeps in mind the book's intended audience. Even keeping this limitation firmly in mind, however, I am not sure that Young's approach is the best one for achieving the commendable end he has in view. The author wishes to interest uninformed students in labor problems. He also wishes his book to serve as a tool for training relatively uninformed line supervisors and union leaders. His method of accomplishing these objectives is to alternate massive doses of verbatim factual materials (i.e., texts of laws, contracts, arbitration decisions, etc.) with chatty conversational sections, ostensibly designed to make the reader think for himself in various roles in problem situations. The book is virtually devoid of serious analytical discussion on the author's part.

This reviewer does not believe that one can intrigue reader interest at either professional or subprofessional levels by addressing him always as "you" and coming perilously close in many of these chatty interludes to "talking down" to said reader. I think Young may have underestimated the reader. If he has not done so, then the level of information and understanding on the part of the claimed audience for this book is distressingly, if not disastrously, low.

The book's organization is reasonably conventional. It begins with a section devoted to exhaustive definitional analysis of such terms as "labor" and "labor problems," followed by a brief historical survey of the growth of American unions and employers' associations. Parts 3 and 4 constitute the heart of the book, containing respectively a fairly detailed analysis of current issues in collective bargaining and the history and current content of basic labor legislation, including the text of the Labor Management Reporting and Disclosure Act.

Part 5 is a factual summary of pertinent state and federal legislation in such areas as social security, unemployment insurance and minimum wage legislation. Part 6 is another factual section on National War Labor Board and National Wage Stabilization Board experience. The concluding section is provocatively entitled "Where do We go from Here?" Unfortunately, no attempt is made to answer the posed question. We are simply reminded that the whole book deals with the United States and we must be aware that labor problems exist elsewhere in the world. No bibliographies or suggestions for additional

reading have been included for the benefit of the reader whose interest might have been aroused.

When he is not quoting verbatim at length from some law or contract, Young writes well and objectively. He also displays some humor on occasion, a rare commodity in the field of labor economics and labor relations. Young is an experienced arbitrator. Thus, as one might expect, the section dealing with collective bargaining problems and grievance arbitration is the most rewarding in the book, both from an informational and an analytical standpoint. In other sections, the author appears to be less sure of himself and takes refuge in heavier injections of quoted materials.

This book may well enjoy a considerable vogue as a "practical" and objectively prepared "tool" for use in workers' education or foreman training programs. From an academic or professional standpoint, however, its contribution must regretfully be regarded as minimal.

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Population; Welfare Programs; Standards of Living

- ENKE, S. The gains to India from population control: some money measures and incentive schemes. *Rev. Econ. Stat.*, May 1960, pp. 175-81.
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Related Disciplines

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NOTES

SEVENTY-THIRD ANNUAL MEETING OF THE AMERICAN ECONOMIC ASSOCIATION

Chase Park Plaza Hotel, St. Louis, Missouri—December 28-30, 1960

Preliminary Announcement of the Program, July 15, 1960

The sessions are organized around the broad theme "Frontiers of Economic Knowledge."

Tuesday, December 27, 1960

6:00 P.M. EXECUTIVE COMMITTEE DINNER MEETING

Wednesday, December 28, 1960

9:30 A.M. MONETARY THEORY: NEW AND OLD LOOKS

Chairman: ELI SHAPIRO, Massachusetts Institute of Technology

Papers: JAMES TOBIN, Yale University

JOHN KAREKEN, University of Minnesota

KARL BRUNNER, University of California, Los Angeles

Discussants: To be announced

ANTITRUST PROBLEMS

Chairman: EDWARD S. MASON, Harvard University

Papers: The Anti-Merger Act, 1950-1960

M. A. ADELMAN, Massachusetts Institute of Technology

Policy Implications of A Theory of Inter-Firm Organization

ALMARIN PHILLIPS, University of Virginia

Mergers and Cartels: Some Reservations about Policy Trends

DONALD J. DEWEY, Duke University

Discussants: JAMES W. MCKIE, Vanderbilt University

REUBEN E. SLESINGER, University of Pittsburgh

JEROME B. COHEN, College of the City of New York

2:30 P.M. ECONOMIC ANALYSIS OF URBAN PROBLEMS (Joint Session with Regional Science Association)

Chairman: SHERMAN J. MAISEL, University of California, Berkeley

Papers: Intra-Urban Location Theory

CHARLES M. TIEBOUT, University of California, Los Angeles

Contrasts in Agglomeration—New York and Pittsburgh

BENJAMIN CHINITZ, University of Pittsburgh

Pricing Policies in Urban Redevelopment

LOUIS WINNICK, New York State Commission on Economic Expansion

Discussants: BARBARA R. BERMAN, Harvard University

BRITTON HARRIS, University of Pennsylvania

IRVING MORRISSETT, Purdue University

PUBLIC UTILITIES AND TRANSPORTATION

Chairman: BEN W. LEWIS, Oberlin College

Papers: Fully Distributed Costs in Utility Ratemaking

JAMES C. BONBRIGHT, Columbia University

Evaluation of Statistical Accounting as Applied to Transportation Industries

JOHN R. MEYER, Harvard University

GERALD KRAFT, Harvard University

Discussants: H. THOMAS KOPLIN, University of Oregon

DANIEL MARX, JR., Dartmouth College

RICHARD A. TYBOUT, Ohio State University

FRONTIERS IN UNCERTAINTY THEORY—THE EVIDENCE OF FUTURES MARKETS
(Joint Session with Econometric Society)

Chairman: D. GALE JOHNSON, University of Chicago

Papers: An Introduction to Behavior Analysis

HOLBROOK WORKING, Stanford University

Systematic and Random Elements in Short-Term Price Movements

HENDRIK S. HOUTHAKKER, Harvard University

Common Elements in Futures Markets in Commodities and Bonds

PAUL COCTNER, Massachusetts Institute of Technology

Discussants: RUTH P. MACK, National Bureau of Economic Research

MICHAEL BRENNAN, Brown University

MARC NERLOVE, Stanford University

8:00 P.M. PRESIDENTIAL ADDRESS

Chairman: PAUL H. DOUGLAS, United States Senate

Presidential Address: THEODORE W. SCHULTZ, University of Chicago

Thursday, December 29, 1960

9:30 A.M. ECONOMIC DEVELOPMENT IN MAINLAND CHINA

Chairman: FRANKLIN L. HO, Columbia University

Papers: National Income of the Chinese Mainland 1958-59

TA-CHUNG LIU, Cornell University and

K. C. YEH, The RAND Corporation

The Statistical System of Mainland China With Particular Reference to Agriculture

CHOH-MING LI, University of California, Berkeley

Sino-Soviet Economic Relations: A Reappraisal

ALEXANDER ECKSTEIN, University of Rochester

Discussants: FRANKLYN D. HOLZMAN, University of Washington

SIDNEY KLEIN, Rutgers University

MACRO-ECONOMIC THEORIES OF INCOME DISTRIBUTION

Chairman: MARTIN BRONFENBRENNER, University of Minnesota

Papers: Conflicting Views on Relative Shares

MELVIN W. REDER, Stanford University

Real versus Monetary Theories of Income Distribution

SIDNEY WEINTRAUB, University of Pennsylvania

Distributional Effects of Alternative Monetary and Fiscal Policies

OSWALD H. BROWNLEE, University of Minnesota and

ALFRED H. CONRAD, Harvard University

Discussants: ROBERT W. OZANNE, University of Wisconsin

BORIS P. PESEK, Michigan State University

CAPITAL THEORY

Chairman: ROBERT DORFMAN, Harvard University

Papers: Implications of Capital Theory for Economic Development Programs

OTTO ECKSTEIN, Harvard University

Implications of Capital Theory for Corporate Investment Decisions

JACK HIRSHLEIFER, University of Chicago

Implications of Capital Theory for Statistical Measurement of Capital and Wealth

JOHN W. KENDRICK, George Washington University

Discussants: FRANCIS M. BATOR, Massachusetts Institute of Technology
 VERNON L. SMITH, Purdue University
 ZVI H. GRILICHES, University of Chicago

12:30 P.M. ECONOMIC ADJUSTMENTS AMONG NATIONS (Joint luncheon with American Finance Association)

Paper: Economic Adjustments Among Nations
 THEODORE O. YNTEMA, Ford Motor Company

2:30 P.M. ECONOMIC EDUCATION: CHALLENGE TO OUR PROFESSION

Chairman: ARTHUR F. BURNS, Columbia University

Papers: This is Economics in the Schools

PAUL R. OLSON, State University of Iowa
 This is Economics

HOWARD S. ELLIS, University of California
 The Responsibility of the Profession

G. L. BACH, Carnegie Institute of Technology

Discussants: G. SHOREY PETERSON, University of Michigan
 E. T. WEILER, Purdue University

WHEAT: A PERMANENT NEED FOR A GOVERNMENT PROGRAM (Joint Session with American Farm Economics Association)

Chairman: G. E. BRANDOW, Pennsylvania State University

Papers: Titles to be announced

HELEN C. FARNSWORTH, Food Research Institute, Stanford University

JOHN A. SCHNITTKER, Kansas State University

Discussants: O. H. BROWNLEE, University of Minnesota

L. E. FOURAKER, Pennsylvania State University

CALVIN B. HOCVER, Duke University

PROBLEMS OF ECONOMIC INSTABILITY IN OTHER COUNTRIES

Chairman: R. A. GORDON, University of California, Berkeley

Papers: Postwar Developments in the Scandinavian Countries

ERIK LUNDBERG, University of California, Berkeley

Growth and Stability in the Postwar Italian Economy

GEORGE H. HILDEBRAND, Cornell University

Business Cycles in Postwar Japan

SHIGETO TSURU, Hitotsubashi University

Discussants: GARDNER ACKLEY, University of Michigan

8:00 P.M. INVITED LECTURE

Chairman: LLOYD G. REYNOLDS, Yale University

Paper: The General Theory After Twenty-Five Years

HARRY G. JOHNSON, University of Chicago

Discussants: ALVIN H. HANSEN, Harvard University

DAVID McCORD WRIGHT, McGill University

ABBA P. LERNER, Michigan State University

LAWRENCE R. KLEIN, University of Pennsylvania

RUSSIAN WAGES (Joint Session with the Industrial Relations Research Association)

Chairman: ABRAM BERGSON, Harvard University

Papers: Recent Developments in the Soviet Wage Structure and the Work of the Wage Commission

WALTER GALENSON, University of California, Berkeley

Comparative Developments in Wage Structure of the Steel Industry in the Soviet Union, Poland, Yugoslavia and Western Countries

GARDNER CLARKE, Cornell University

Discussants: EMILY C. BROWN, Vassar College

HAROLD M. DOUTY, Bureau of Labor Statistics

Friday, December 30, 1960

9:30 A.M. THE BALANCE OF PAYMENTS OF THE UNITED STATES: PROBLEMS AND PROSPECTS

Chairman: CHANDLER MORSE, Cornell University

Papers: Disturbances and Adjustments in Recent U.S. Balance of Payments Experience

HAL B. LARY, National Bureau of Economic Research

Unbalanced International Accounts: Diagnosis and Therapy

J. HERBERT FURTH, Board of Governors of the Federal Reserve System

The Adequacy of United States Gold Reserves

E. M. BERNSTEIN, E.M.B. (Ltd.)

Discussants: JAMES BURTLE, W. R. Grace and Company

JAROSLAV VANEK, Harvard University

ECONOMICS AND NATIONAL SECURITY

Chairmen: T. C. SCHELLING, Harvard University

Papers: The Propensity to Reduce The National Debt Out of Defense Savings

EMILE BENOIT, Columbia University

Which Industries Would Be Most Important in a Postwar U.S. Economy?

DONALD V. T. BEAR, Stanford University

PAUL G. CLARK, Williams College

Strategy in Active Defense

W. THORNTON READ, Bell Telephone Laboratories

The Interrelationship of Strategic Objectives

DANIEL ELLSBERG, The RAND Corporation

The Economics of Stand-By Capacity for Military Emergencies

ALLEN R. FERGUSON, Northwestern University

DISTRIBUTION COSTS—CONCEPTS AND MEASURES (Joint Session with American Marketing Association)

Chairman: WILLARD W. COCHRANE, University of Minnesota

Papers: The Effects of Distribution Costs on Demand

LESTER TELSER, University of Chicago

Specialization, Scale and Costs in Retailing

RICHARD H. HOLTON, University of California, Berkeley

An Interpretation of Changes in Agricultural Marketing Costs

F. V. WAUGH, U.S. Department of Agriculture, and

KENNETH OGREN, U.S. Department of Agriculture

Discussants: WARREN J. BLKEY, University of Notre Dame

REAVIS COX, University of Pennsylvania

2:30 P.M. THE INFLUENCE OF MORAL AND SOCIAL RESPONSIBILITY ON ECONOMIC BEHAVIOR

Chairman: LELAND J. GORDON, Denison University

Papers: The Influence of Moral and Social Responsibility on Advertising and Selling Practices

COLSTON E. WARNE, Amherst College

The Influence of Moral and Social Responsibility on Executives of Large Corporations

ERNEST DALE, Cornell University

The Influence of Moral and Social Responsibility on Selling Consumer Credit

ARCH TROELSTRUP, Stephens College

Discussants: HOWARD M. TEAF, JR., Haverford College

RAYMOND T. BYE, University of Pennsylvania

DEXTER M. KEEZER, McGraw-Hill Publishing Co.

THE CASE OF INDIA

Chairman: WILFRED MALENBAUM, University of Pennsylvania

Papers: Development Horizons for India

GEORGE ROSEN, United Nations Secretariat

The Strategy of Planning with Special Reference to the Third Plan

DON D. HUMPHREY, Fletcher School of Law and Diplomacy

Performance and Plan: Analysis of the Gap.

ASHOK MITRA, International Bank for Reconstruction and Development

Discussants: WILLIAM W. HOLLISTER, Washington, D.C.

CHARLES WOLF, JR., The RAND Corporation

MANAGERIAL ECONOMICS: A NEW FRONTIER?

Chairman: MERTON H. MILLER, Carnegie Institute of Technology*Papers:* The Current State of Managerial Economics

W. W. COOPER, Carnegie Institute of Technology

What Can Economic Theory Contribute to Managerial Economics?

WILLIAM J. BAUMOL, Princeton University

What Can Managerial Economics Contribute to Economic Theory?

CHARLES J. HITCH, The RAND Corporation

Discussants: JULIUS MARGOLIS, University of California, Berkeley

ALAN MANNE, Yale University

FRANCO MODIGLIANI, Northwestern University

5:00 P.M. BUSINESS MEETING

6:00 P.M. EXECUTIVE COMMITTEE DINNER MEETING

NEW PUBLICATIONS

The first issue of *Yugoslav Survey*, a quarterly journal in English, was published in April 1960. The *Survey* contains documentary and informative articles on the economic, social and cultural life of Yugoslavia. Communications about the journal should be sent to Jugoslavija Publishing House, Nemanjina 34, Belgrade, Yugoslavia.

The International Training and Research Center for Development, IRFED, is now publishing a quarterly review *Développement et Civilisations* devoted to problems of undeveloped countries. Communications and manuscripts should be sent to the editor, Madeleine Trebous, 29, Place du Marché-Saint-Honoré, Paris 1^{er}, France.

Announcements

Creation of a National Task Force on Economic Education has been announced by Theodore W. Schultz, president of the American Economic Association and by Donald K. David, chairman of the Board of the Committee for Economic Development, which is sponsoring the undertaking. The primary objective of the Task Force is to attempt to define what economics high school students should and can be taught for effective citizenship and participation in our democratic system.

Professor Schultz has made the following appointments to the group: George L. Bach, Carnegie Institute of Technology, chairman; Lester V. Chandler, Princeton University; Robert A. Gordon, University of California, Berkeley; Ben W. Lewis, Oberlin College; and Paul A. Samuelson, Massachusetts Institute of Technology. Recommendations will represent their independent views and not the views of the American Economic Association, the Committee for Economic Development or any other special economic group.

The Social Science Research Council's annual announcement describing fellowships and grants to be awarded in 1960-61 is now ready for distribution. It lists the following pro-

grams to be continued without major changes: research training fellowships, faculty research fellowships; grants-in-aid of research; grants for research on national security policy. Under joint sponsorship with the American Council of Learned Societies, grants are to be offered to mature scholars for research in the social sciences and humanities on certain foreign areas.

Applications for some categories of awards will be due not later than November 1. Inquiries should be addressed to the Social Science Research Council, 230 Park Avenue, New York 17, N.Y.

Economists from about 40 nations will meet in Geneva, Switzerland in the summer of 1961 for an International Conference on Input-Output Techniques. The meeting will take place in the Palais des Nations from August 28 through September 1, 1961. The Conference is being organized by the Harvard Economic Research Project of Harvard University in association with the United Nations Secretariat and is supported with funds from the National Science Foundation and the Rockefeller Foundation. Wassily Leontief, Henry Lee Professor of economics at Harvard University, will be chairman of the conference. Dr. Elizabeth Gilboy, lecturer on economics at Harvard, will be secretary general.

The Fund for Social Analysis is again offering a limited number of grants-in-aid for studies of problems posed by Marxist theory and its application. Grants will ordinarily range from \$500 to \$3,000. Address the Corresponding Secretary, The Fund for Social Analysis, Room 2800, 165 Broadway, New York 6, N.Y.

Deaths

Carl R. Bye, Syracuse University, May 23, 1960.

Fred C. Croxton, Washington, D.C., April 3, 1960.

Heinrich W. Lück, Godesberg, Germany.

Archibald M. McIsaac, Syracuse University, January 11, 1960.

Curtis H. Morrow, May 7, 1960.

Edwin E. Witte, emeritus, University of Wisconsin, May 6, 1960.

Harry D. Wolf, University of North Carolina, June 8, 1960.

Retirements

Thomas A. Budd, Wharton School, University of Pennsylvania, June 1960.

Paul J. FitzPatrick, Catholic University of America.

Floyd B. Haworth, University of Illinois, September 1960.

David B. Jeremiah, Wharton School, University of Pennsylvania, June 1960.

A. L. Lomax, University of Oregon.

Elinor Pancoast, Goucher College, September 1960.

Wilbur C. Plummer, Wharton School, University of Pennsylvania, June 1960.

Robert Riegel, University of Buffalo, July 1960.

Visiting Foreign Scholars

Bruno Leoni, University of Pavia: distinguished visiting scholar, Thomas Jefferson Center for Studies in Political Economy, University of Virginia, fall term, 1960.

Erik Lundberg, University of Stockholm: visiting research professor of economics, University of California, Berkeley, 1960-61.

Robin Marris, Kings College, Cambridge University: visiting professor of economics, University of Texas, fall semester; lecturer in economics, spring semester 1961, University of California, Berkeley.

Hla Myint, visiting professor of economics, Cornell University, spring term, 1961.

J. N. Wolfe, University of Toronto: visiting professor of economics, University of California, Berkeley, 1960-61.

William Woodruff, Melbourne University: University of Illinois, 1960-61.

Promotions

Edward Ames: professor of economics, School of Industrial Management, Purdue University.

E. J. Ball: professor of business administration, University of Arkansas.

Gary S. Becker: professor of economics, Columbia University; Ford research professor, 1960-61.

Joseph W. Conard: professor of economics, Swarthmore College.

Paul G. Darling: professor of economics, Bowdoin College.

R. Kirby Davidson: professor of economics, School of Industrial Management, Purdue University.

Robert R. Dince: associate professor, College of Business Administration, University of Georgia.

Norton T. Dodge: assistant professor of economics, University of Maryland.

Richard A. Easterlin: professor of economics, Wharton School, University of Pennsylvania.

Otto Eckstein: associate professor of economics, Harvard University.

Robert R. Edminster: associate professor of economics, University of Utah.

Eber W. Eldridge: associate professor of economics and sociology, Iowa State University (Ames).

Frederic N. Firestone: assistant professor of economics, Wellesley College.

Leslie Fishman: associate professor of economics, University of Colorado.

Gilbert L. Gifford: professor of economics, University of Arizona.

John G. Gurley: professor of economics, University of Maryland.

Howard H. Hines: professor of economics and sociology, Iowa State University (Ames).

A. M. Huq: associate professor of economics, University of Vermont.

Donald P. Jacobs: associate professor of finance, School of Business, Northwestern University.

Alexandre Kafka: professor of economics, University of Virginia.

Mark L. Kahn: professor of economics, Wayne State University.

John W. Kendrick: professor of economics, George Washington University.

Clifton H. Krepes: Professor of Banking, School of Business Administration, University of North Carolina.

Harold C. Krogh: professor of business administration, University of Kansas.

Harvey Leibenstein: professor of economics, University of California, Berkeley.

John M. Lettich: professor of economics, University of California, Berkeley.

E. E. Liebafsky: professor of economics, Agricultural and Mechanical College of Texas.

F. Ray Marshall: professor of economics, Louisiana State University.

Edwin S. Mills: associate professor of economics, Johns Hopkins University.

John R. Moore: professor of business administration, University of Tennessee.

Richard R. Newberg: professor, department of agricultural economics and rural sociology, Ohio State University.

Hugh S. Norton: professor of transportation, University of Tennessee.

Arthur M. Okun: associate professor of economics, Yale University.

- Robert C. Ortner: lecturer on statistics, Wharton School, University of Pennsylvania.
 James P. Payne, Jr.: professor of economics, Louisiana State University.
 Joseph S. Peery: assistant professor of economics, University of Utah.
 A. A. Pepelasis: associate professor of economics, University of California, Davis.
 Boris P. Pesek: associate professor of economics, Michigan State University.
 Frank S. Pinet: associate professor of business administration, University of Kansas.
 Stanley Reiter: professor of economics, School of Industrial Management, Purdue University.
 V. W. Ruttan: professor, agricultural economics department, Purdue University.
 Wilson E. Schmidt: professor of economics, George Washington University.
 Leon M. Schur: associate professor of economics, Louisiana State University.
 J. T. Scott: associate professor of economics and sociology, Iowa State University, (Ames).
 Richard E. Shannon: associate professor of economics, Montana State University.
 Edgar T. Shaudys: associate professor, department of agricultural economics and rural sociology, Ohio State University.
 John W. Skinner: associate professor of economics, George Washington University.
 Gerald G. Somers: professor of economics, University of Wisconsin.
 Jack D. Steele: professor of business administration, University of Kansas.
 George Strauss: professor, school of business administration, department of industrial relations, University of Buffalo.
 Ronald L. Stucky: professor of industrial management and associate dean, School of Industrial Management, Purdue University.
 Milton C. Taylor: professor of economics, Michigan State University.
 D. W. Thomas: professor of agricultural economics, Purdue University.
 Erik Thorbecke: associate professor of economics and sociology, Iowa State University (Ames).
 Charles M. Tiebout: associate professor of economics, University of California, Los Angeles.
 Thomas A. Yancey: associate professor of economics, University of Illinois.
 Wesley J. Yordon: assistant professor of economics, University of Colorado.

Administrative Appointments

- D. E. Armstrong: professor and director of the School of Commerce, McGill University.
 Floyd Bond: dean, School of Business Administration, University of Michigan.
 Sol S. Buchalter: chairman, department of finance, San Fernando Valley State College.
 James D. Calderwood: chairman, department of business economics and international trade, School of Business Administration, University of Southern California.
 Robert G. Cox: vice dean, undergraduate division, Wharton School, University of Pennsylvania.
 Maurice E. Dance: chairman, department of economics, San Fernando Valley State College.
 John S. Day: associate dean, School of Industrial Management, Purdue University.
 Lowell E. Gallaway: director, Bureau of Business Services and Business Research, San Fernando Valley State College.
 William F. Hellmuth: dean, College of Arts and Sciences, Oberlin College.
 Donald D. Humphrey: director, William L. Clayton Center of International Economic Affairs, Fletcher School of Law and Diplomacy, Tufts University.
 C. Clyde Jones: chairman, department of business administration, Kansas State College.

E. W. Kierans, McGill University: president of the Montreal and Canadian Stock Exchanges.

William B. Keeling: director and professor, Bureau of Business Research, University of Georgia.

Hal B. Lary: associate director of research, National Bureau of Economic Research.

Stephen L. McDonald: head, department of economics, Louisiana State University.

John Nordin: acting head, department of economics, Iowa State University (Ames), 1960-61.

Raymond Pelissier: director, School of Business Administration, Georgetown University.

Edward S. Shaw: acting executive head, department of economics, Stanford University, 1960-61.

John W. Skinner: acting executive officer, department of economics, George Washington University.

Daniel L. Spencer, Southern Illinois University: professor of economics and head of department of economics, Howard University.

Sidney Weintraub: chairman, department of economics, Wharton School, University of Pennsylvania.

Richard M. Westebbe, Board of Governors, Federal Reserve System: executive director, Foreign Trade Administration, Ministry of Commerce, Greece.

Nathaniel Wollman: chairman, department of economics, University of New Mexico.

Appointments

Arthur T. Andersen: instructor, department of economics, Boston University.

Y. B. Awh: assistant professor of economics, Nichols State College, Louisiana.

Thomas R. Beard: assistant professor of economics, Louisiana State University.

Philip W. Bell, University of California, Berkeley: associate professor of economics, Haverford College.

David Bodenberg: instructor in economics, Princeton University.

Norman M. Bradburn: assistant professor of behavioral sciences, Graduate School of Business, University of Chicago.

George F. Break, University of California, Berkeley: member of senior staff, Brookings Institution, for special research.

Ayers Brinser: professor of economics, University of Colorado.

J. E. Brown, University of Florida: assistant professor of economics, University of Washington.

Conrad Caligaris: assistant professor of business and economics, University of Maine.

Robert W. Campbell: visiting associate professor, University of California, Berkeley, fall term; associate professor of economics, Indiana University, beginning February 1961.

George K. Chacko: analyst, operations research section, Atlas Powder Company.

Robert J. Connor: instructor in production management, Graduate School of Business, University of Chicago.

Ward S. Curran: instructor in economics, Trinity College.

Michael F. Dacey: assistant professor of regional science, Wharton School, University of Pennsylvania.

Ernest M. DeCicco: instructor, department of economics, Boston University.

Harold Demsetz: assistant professor, department of economics, University of California, Los Angeles.

W. Stanley Devino: assistant professor of business and economics, University of Maine.

Donald Dewey, Duke University: associate professor of economics, Columbia University.

Peter O. Dietz: instructor in finance, School of Business, Northwestern University.
Lev E. Dobriansky: professor of economics, Georgetown University.
Peter Drucker: visiting professor of industry, Wharton School, University of Pennsylvania.

I. M. Drummond: lecturer, department of political economy, University of Toronto.
James N. Duprey: staff of economics department, University of North Dakota.
Carl T. Eakin, University of Notre Dame: associate professor of marketing, University of Georgia.

Edward C. Fei, formerly of Swarthmore College: associate professor of economics, University of Wisconsin.

William J. Fellner: Ford research professor, Columbia University, 1960-61.

Lehman B. Fletcher, University of California, Los Angeles: assistant professor of economics, Iowa State University (Ames).

Hugh W. Folk: lecturer in economics, University of California, Berkeley, 1960-61.

D. F. Forster: lecturer, department of political economy, University of Toronto.

Karl A. Fox, Iowa State University (Ames): visiting professor of economics, Harvard University.

Harry L. Franklin: visiting lecturer, department of economics, Georgetown University.

Daniel R. Fusfeld, Michigan State University: appointment at University of Michigan.

Lloyd L. Gallardo, Michigan State University: appointment at St. Mary's College, California.

Jamshed Ghandhi: lecturer in finance, Wharton School, University of Pennsylvania.

Herbert A. Goertz: instructor in economics, Dartmouth College.

Raymond W. Goldsmith: professor of economics, Yale University.

H. A. J. Green: assistant professor, department of political economy, University of Toronto.

James L. Green, Wright-Patterson Air Force Base: professor of economics, College of Business Administration, University of Georgia.

Karl D. Gregory: assistant professor of economics, Wayne State University.

George F. Hadley: associate professor of business administration, Graduate School of Business, University of Chicago.

George D. Hanrahan, University of Minnesota: assistant professor of economics, Saint Louis University.

Alvin H. Hansen, Harvard University: fellow, Center for Advanced Studies, Wesleyan University, second semester, 1960-61.

Hugh G. Hansen: visiting associate professor of economics, Cornell University, 1960-61.

D. G. Hartle: assistant professor, department of political economy, University of Toronto.

Rex D. Helfinstine, formerly Agricultural Research Service, U.S. Department of Agriculture: staff of economics department, South Dakota State College.

W. J. Herman: instructor in business administration, University of South Florida.

Donald D. Hester: instructor in economics, Yale University.

Forest G. Hill, University of Buffalo: professor of economics, University of Texas.

Alan Hoffman: associate visiting professor, New School for Social Research.

Hendrik S. Houthakker, Stanford University: professor of economics, Harvard University.

Hans J. Jaksch, formerly University of Frankfurt/Main: assistant professor of economics, The Rice Institute.

C. Hayden Jamison, Beloit State Bank: lecturer in economics, Beloit College, 1960-1961.

Sidney L. Jones: assistant professor of business administration, School of Business, Northwestern University.

S. J. Kagan, Joint Council on Economic Education: faculty of School of Business Administration, University of Oregon.

Reuben A. Kessel: assistant professor of business economics, Graduate School of Business, University of Chicago.

Robert E. L. Knight, University of New Mexico: assistant professor of economics, University of Maryland.

Frederick E. Kottke: assistant professor of economics, University of Southern California.

Aranka E. Kovacs: lecturer in economics, McGill University.

Sigmund Krauthamer: assistant professor of economics, University of Utah.

Marian Krzyzaniak, Montana State University: lecturer 1960-61, The Johns Hopkins University.

Simon Kuznets, The Johns Hopkins University: professor of economics, Harvard University.

Richard A. LaBarge, Southern Methodist University: financial analyst, Ford Motor Company.

Alexis E. Lachman, International Cooperation Administration, Rome: financial advisor to Finance Minister of Laos.

William E. Laird, Jr.: assistant professor of economics, Florida State University.

Norman Levine: research economist, Systems Research Group, Mineola, L.I.

Fritz Machlup, The Johns Hopkins University: professor of economics and director, International Finance Section, Princeton University.

William L. McDaniel, Princeton University: assistant professor of economics, University of New Mexico.

Raymond H. McEvoy, Montana State University: economist, Bank of America National Trust and Savings Association, San Francisco.

David McFarland: lecturer in economics, Princeton University.

Jack Melitz: fellow at Thomas Jefferson Center for Studies in Political Economy, University of Virginia.

E. D. Milenev: assistant professor of economics, Hartwick College.

Richard A. Miller: instructor in economics, Wesleyan University.

Jacob Mincer: associate professor of economics, Columbia University.

John B. Miner, University of Pennsylvania: associate professor, School of Business Administration, University of Oregon.

Harry A. Miskimin, Jr.: instructor in economics, Yale University.

Aurelius Morgner: associate professor of economics, University of Southern California.

John G. Myers: assistant professor of economics, University of Colorado.

H. R. Neville: visiting professor of economics and management, Louisiana State University, first semester, 1960-61.

Alan T. Nichols: assistant professor of economics, School of Business, University of South Dakota.

Alfred Parker: instructor in economics, Oklahoma State University.

Jan Parker: instructor in economics, Wellesley College.

Hugh T. Patrick: assistant professor of economics, Yale University.

Edmund S. Phelps, Jr.: assistant professor of economics, Yale University.

- Gerald A. Pinsky: instructor in economics, Dartmouth College.
- Sher J. Rana, University of Alaska: assistant professor of economics, University of Puerto Rico.
- Gustav Ranis: assistant professor of economics, Yale University.
- James R. Ratliff: assistant professor of accounting, School of Business Administration, University of Pittsburgh.
- Ira J. Rees: assistant professor of business administration, University of Georgia.
- Dennis Reinmuth: staff of economics department, University of North Dakota.
- Richard A. Ridilla, University of Pittsburgh: position with the Pittsburgh National Bank.
- Gaston V. Rimlinger, formerly Princeton University: associate professor of economics, The Rice Institute.
- J. Thomas Romans: lecturer, department of economics, University of Buffalo.
- Stephen W. Rousseas: visiting associate professor of economics, University of Maryland, 1960-61.
- David J. Saposs: lecturer in international labor relations, School of International Service, The American University.
- Wolfgang Schoellkopf: instructor in economics, Cornell University, 1960-61.
- M. C. Schnitzer, University of Florida: staff of Virginia Polytechnic Institute.
- David Schwartzman: associate professor, department of economics, New School for Social Research.
- Martin Shubik: visiting professor of economics, Yale University.
- Ronald E. Simmons: assistant professor of economics, DePauw University.
- Rudolph Skandera: assistant professor of accounting, College of Business Administration, University of Georgia.
- John H. Smith: professorial lecturer in statistics, Graduate School of Business, University of Chicago.
- Eugene Smolensky, University of Pennsylvania: assistant professor of economics, Haverford College.
- Richard C. Spangler, Montana State University: research post, University of Hawaii.
- Case Sprenkle: assistant professor, department of economics, University of Illinois.
- Thirukodikaval N. Srinivasan: instructor in economics, Yale University.
- Barry Supple: associate professor of economic history, McGill University.
- Izumi Taniguchi, University of Texas: assistant professor of economics, University of Missouri.
- Daniel Thorner, Indian Statistical Institute, Bombay: associate professor, Ecole Pratique des Hautes Etudes, Sorbonne, Paris, 1960-61.
- S. G. Triantis: associate professor in the department of political economy, University of Toronto.
- Jan V. Tumlrir: instructor in economics, Yale University.
- Hirofumi Uzawa: assistant professor of economics and mathematics, department of economics, University of California, Berkeley.
- Stanley Vance, Kent University: H. T. Miner professor of business administration, University of Oregon.
- Otto von Fieandt: instructor in economics, Yale University.
- H. Jean Waldrop: instructor in economics, Wellesley College.
- Vivian C. Walsh: associate professor of economics, School of Business Administration, University of Buffalo.
- Rhea H. West, Jr., Wake Forest College: associate professor of economics, College of Business Administration, University of Georgia.

Robert O. Wheeler: instructor in economics, Montana State University.

C. Arthur Williams: visiting professor of insurance, Wharton School, University of Pennsylvania.

Robert J. Wolfson, Michigan State University: appointment at University of California, Los Angeles.

Arnold Zellner, formerly University of Washington: associate professor of economics, University of Wisconsin.

Leaves for Special Appointments and Assignments

Wallace N. Atherton, Michigan State University: director, Center for Studies on Economic Development, University of the Andes, Bogotá, Colombia, 1960-61.

Emile Benoit, Columbia University: Brookings Institution professorship 1960-61.

Max R. Bloom, Syracuse University: Fulbright lecturer, Technion-Israel Institute of Technology, 1960-61.

Russell P. Bowers, De Paul University, Chicago: University of Wisconsin-Ford Foundation project, Gadjah Mada University, Jogjakarta, Indonesia.

Karl A. Fox, Iowa State University (Ames): visiting professor of economics, Harvard University, 1960-61.

Henry W. Grayson, University of Maryland: professor of economics and head of economics department, University of Khartoum, Sudan.

Frank A. Hanna, Duke University: with United Nations as industrial statistics expert, National Statistical Services of Greece, Athens, 1960-61.

Earl O. Heady, Iowa State University (Ames): Center for Advanced Study in the Behavioral Sciences, Stanford, California.

James R. Hoath, formerly Kansas State University: University of Wisconsin-Ford Foundation project at Gadjah Mada University, Jogjakarta, Indonesia.

Patrick R. Huntley, University of Arizona: U. S. Bureau of the Census, Washington, D.C.

Alexandre Kafka, University of Virginia: United Nations secretariat, fall term 1960-61.

Michael Kaser, U.N. Economic Commission for Europe: research fellow, St. Antony's College, Oxford University, 1960-61.

B. S. Keirstead, University of Toronto: visiting professor, University College of the West Indies, 1960-61.

George Kleiner, University of Illinois: International Cooperation Administration, India, for two years.

Tjalling Koopmans, Yale University: Frank W. Taussig research professor of economics, Harvard University, 1960-61.

Abba Lerner, Michigan State University: Center for Advanced Study in the Behavioral Sciences, Stanford, California.

R. W. Lindholm, University of Oregon: in Korea for review of Economic Development Program of the Republic of Korea, summer 1960, request of International Cooperation Administration.

Bruce R. Morris, University of Massachusetts: University of Wisconsin-Ford Foundation project at Gadjah Mada University, Jogjakarta, Indonesia.

Howard W. Nicholson, Clark University: Transportation Study Group, Senate Committee on Interstate and Foreign Commerce, first semester 1960-61.

Forrest R. Pitts, University of Oregon: member, International Cooperation Administration advisory group to the Economic Development Council in Korea.

Edwin P. Reubens, The City College, New York: University College of West Indies, fall semester 1960.

Alek A. Rozental: tax adviser with the International Cooperation Administration in Laos.

Philip Sheinwold, Brooklyn College; Fulbright lecturer, Quito, Ecuador.

Paul B. Simpson, University of Oregon: member, International Cooperation Administration advisory group to the Economic Development Council in Korea.

Gerhard Tintner, Iowa State University (Ames): technical consultant, United Nations Technical Assistance Board, Indian Statistical Institute, Calcutta, India, summer 1960.

Rutledge Vining, University of Virginia: visiting professor of economics, Giannini Foundation, University of California, Berkeley, fall term 1960-61.

Resignations

Edward C. Acheson: George Washington University.

Edward C. Atwood, Jr.: Washington and Lee University.

Jack Hirshleifer: Graduate School of Business, University of Chicago.

John J. Klein: Oklahoma State University.

Miscellaneous

Agustín Benítez Heymann: admitted to partnership in public accounting firm Manrara y Pérez Daple, Havana, Cuba.

FIFTY-SEVENTH LIST OF DOCTORAL DISSERTATIONS IN POLITICAL ECONOMY IN AMERICAN UNIVERSITIES AND COLLEGES

The present list specifies doctoral degrees conferred during the academic year terminating June 1960, and theses undertaken in the same period.

General Economics; Methodology

Thesis in Preparation

JOAN E. ALLEN, B.S. Maryland 1958; M.A. Virginia 1959. Humanitarianism in classical economics. *Virginia*.

Price and Allocation Theory; Income and Employment Theory; History of Economic Thought

Degrees Conferred

ROBERT S. ADDEN, Ph.D. North Carolina 1960. The economic effects of governmental fiscal policy as viewed by certain English classical economists.

YOON B. AWH, Ph.D. Florida 1960. A study in the concept of balanced development with particular reference to the Nurksian doctrine of balanced growth.

IRENE H. BUTTER, Ph.D. Duke 1959. Academic economics in Holland, 1800-1870.

SAMUEL B. CHASE III, Ph.D. California (Berkeley) 1960. The theory of asset prices.

LESLIE D. FIXLER, Ph.D. New York 1960. The economics of price discrimination.

FREDERICK M. GOTTHEIL, Ph.D. Duke 1959. The economic predictions of Karl Marx: an examination of Marxian economic theory.

LAFAYETTE G. HARTER, JR., Ph.D. Stanford 1960. John R. Commons, institutional economist.

EUNICE KITCHELL, Ph.D. Texas 1959. Statistical investigation in the elasticities of demand for motor fuel.

ARNOLD B. LARSON, Ph.D. Stanford 1960. Evidence on the temporal distribution of price effects of new market information.

RONAN G. MACDONALD, Ph.D. Wisconsin 1960. A comparison of the theories of entrepreneurial expectations of Keynes and Schumpeter.

SANFORD L. MARGOSHES, Ph.D. New York 1960. Economic theory and entrepreneurial techniques of profit measurement.

BERNARD J. MARKS, Ph.D. Minnesota 1960. The prediction and analysis of demand for replacement parts.

BENTON F. MASSELL, Ph.D. Yale 1960. Determinants of productivity change in United States manufacturing.

JOHN W. NEVILE, Ph.D. California (Berkeley) 1960. Investment theory in some modern dynamic economic models.

JOHN C. S. PARK, Ph.D. Nebraska 1959. Value theory and oligopolistic manufacturing industries.

HERBERT R. RUNYON, Ph.D. Michigan 1960. Contributions of Irving Fisher to modern theory.

KAZUO SATO, Ph.D. Yale 1960. Price-cost structure and behavior of profit margins in manufacturing.

JOHN M. SCHEIDELL, Ph.D. Notre Dame 1960. The time elasticity of demand and productivity: a theoretical analysis.

CHARLES L. SCHULTZE, Ph.D. Maryland 1960. Recent inflation in the United States.

HOWARD D. SHARPE, JR., Ph.D. Harvard 1960. The effects of secular growth in factor

money income rates, productivity, and labor force on the secular level of, and growth of, employment.

HENRY SOLOMON, Ph.D. New York 1960. The significance of the concept of capacity to produce in economic theory.

TADIMI TACHINO, Ph.D. American 1960. Aspects of the theory of production and production functions.

JOHN W. L. WINDER, Ph.D. Chicago 1960. The demand for stocks: copper.

TENG-PIN YU, Ph.D. New York 1960. Variables of an aggregate consumption function.

Theses in Preparation

DONALD V. BEAR, B.A. Princeton 1954; B. Phil. Oxford 1956. The nature of prices in macro-dynamic economic processes. *Stanford*.

GARY BICKEL, B.A. Colorado 1955. Factor proportions and relative prices: an experiment in estimation of shadow prices. *Stanford*.

B. D. BIXLEY (earlier degrees not supplied). Welfare problems and implications of the theory of economic growth. *Toronto*.

STANLEY BOBER, B.A. New York 1953; M.A. 1956. A study of the cyclical behavior of retail inventories. *New York*.

WILLIAM BREIT, B.A. Texas 1955; M.A. 1956. The wages-fund theory: a historical analysis and restatement. *Michigan State*.

VARTKES BROUSSALIAN, B.S. London School of Econ. 1951. Inflation-caused redistribution of wealth: a test of a hypothesis. *California (Los Angeles)*.

DOUGLAS C. DACY, B.B.A. Texas 1950; M.A. 1954. A study of land prices in the United States. *Harvard*.

MARY HAMILTON (earlier degrees not supplied). Empirical implications of demand theory. *Pennsylvania*.

JOHN HENNING, B.S. Columbia 1955; M.A. Yale 1956. Trends in price flexibility: 1923-1959. *Cornell*.

NORMAN S. HUBBARD, B.A. Yale 1956; M.A. 1957. Cost reduction and the inducement to invest. *Yale*.

VELY M. LEROY, B.A. College of Notre Dame 1954; M. Comm., École des Hautes Études 1957. The Cambridge equation, the real balance effect and employment. *Michigan State*.

SELIG D. LESNOY, B.A. Michigan 1948; M.S. Columbia 1953. The cost of capital in the theory of investment. *Michigan*.

ABDULHADI MADJID, B.A. Harvard 1952. Flow of Funds. *Harvard*.

F. MARTIN, B.A. McGill 1952; M.A. 1958. Location theory. *McGill*.

WILLIAM M. PARKER, B.A. California (Berkeley) 1938; M.A. 1948. The use of accounting data to test and integrate theories of the firm. *Southern California*.

FRANK PETRELLA, JR., B.A. Notre Dame 1956; M.A. 1957. Edmund Burke as a conservative classical economist. *Notre Dame*.

WOLFGANG SCHOELLKOPF, B.A. California 1956. A microeconomic approach to consumption theory. *Cornell*.

KRISHNA PRASAD SHARMA, B.A. Calcutta 1951; M.A. 1954. Studies in some growth models with special reference to the U. S. economy. *Oregon*.

PHILIP E. SORENSEN, B.S. Utah State Agricultural 1954; M.S. 1957. The economics of Francis Ysidro Edgeworth. *California (Berkeley)*.

THIRUKODIKAVAI NILAKANTA SRINIVASAN, B.A. Madras 1953; M.A. Indian Statistical Inst. 1955; M.A. Yale 1958. Investment criteria and choice of techniques of production. *Yale*.

- DOMENICO A. TOSATO, B.A. Bowdoin 1956; M.A. Yale 1958. Uncertainty and the implementation of macroeconomic policy. *Yale*.
- IAN D. S. WARD, B.C., M.C. Melbourne. Productive and unproductive consumption in pre-classical and classical literature. *California (Berkeley)*.
- SIDNEY G. WINTER, JR., B.A. Swarthmore 1956; M.A. Yale 1957. Relations between research spending and corporate growth: a theoretical and statistical analysis. *Yale*.
- DE-MIN WU, B.A. National Taiwan Univ. 1956. A decision-unit model of household expenditures on durable goods. *Wisconsin*.

Economic History; Economic Development; National Economies

Degrees Conferred

- LORENZO M. BELOTTI, Ph.D. Texas 1960. The influence of Keynesian "Dirigisme" on Christian socialism in the postwar Italian economic policies.
- JAMES H. BLACKMAN, Ph.D. Columbia 1960. Soviet transport and the process of industrialization.
- BERNARD G. BROWN, Ph.D. Wisconsin 1959. The fiscal influence on economic development, with special reference to Iran.
- EDWIN CALDWELL, Ph.D. Harvard 1960. The development of manufacturing in Texas since 1919.
- YU-MIN CHOU, Ph.D. Illinois 1960. The role of international trade in the economic development of Southeast Asia, with particular reference to Malaya and Indonesia.
- EDWARD F. CRIM, JR., Ph.D. Illinois 1960. The effect of regional public expenditures on the level of regional income as illustrated by the State of Oklahoma.
- PADMA R. DESAI, Ph.D. Harvard 1960. A short term planning model for the Indian economy.
- GUIDO J. M. DI TELLA, Ph.D. Mass. Inst. of Tech. 1960. Economic history of Argentina: 1914-1933.
- JOHN M. FRIKART, Ph.D. Colorado 1959. Effects of the Peron regime on the Argentine economy.
- GEOFFREY B. HAINSWORTH, Ph.D. California (Berkeley) 1960. Classical theories of overseas development.
- JOSEPH M. HENNESSEY, Ph.D. Boston College 1960. New England manufacturing, 1947-1954; a postwar analysis of employment, output and productivity.
- JOHANNES HIRSCHMEIER, Ph.D. Harvard 1960. The genesis of modern entrepreneurs in Meiji, Japan.
- MOON H. KANG, Ph.D. Nebraska 1960. The monetary aspect of the economic development in Japan with special reference to monetary policies, 1868-1935.
- EDWARD S. LITTLE, Ph.D. American 1959. National resources of Spain as a basis for an industrial economy.
- PAUL MEDOW, Ph.D. Columbia 1960. Conceptual and methodological problems in applying Schumpeter's theory of economic development to nonmarket economies.
- JORA MINASIAN, Ph.D. Chicago 1960. The economics of research and development.
- HARRY A. MISKIMIN, Ph.D. Yale 1960. Specie debasement and price movements in France, 1295-1395.
- MAX MUELLER, Ph.D. Illinois 1960. Money, investment and economic development with special reference to India.
- HANEEF A. NASEEM, Ph.D. American 1960. Progress of economic growth in Pakistan (1947-57): a critical study in retrospect.
- TOYOKI OKABAYASHI, Ph.D. Oregon 1960. Measuring the contributions of natural resources to the national outputs of the United States and Japan.

- IZZID D. PAL, Ph.D. McGill 1960. Commercial policy and economic development with reference to Pakistan.
- THOMAS T. POLEMAN, Ph.D. Stanford 1960. The Papaloapan development project.
- ZORA PROCHAZKA, Ph.D. Harvard 1960. Foreign trade and economic development of Czechoslovakia.
- BEATRICE G. REUBENS, Ph.D. Columbia 1960. State financing of private enterprise in early New York.
- SAMUEL M. ROSENBLATT, Ph.D. Rutgers 1960. The House of John Norton & Sons: a study of the consignment method of marketing tobacco from Virginia to England.
- ROKNEDDIN SADAT-TEHRANI, Ph.D. New York 1960. The seven year plan organization for the economic development of Iran.
- JOGINDER S. SAHOTA, Ph.D. Oregon 1960. Economic criteria for determination of methods of production for Indian agriculture.
- MARTIN C. SCHNITZER, Ph.D. Florida 1960. The use of inducements by states and communities in the promotion of industrial development, with special reference to Mississippi.
- SAID A. SHAH, Ph.D. McGill 1960. Structural obstacles to economic development in India.
- ROBERT G. SPIEGELMAN, Ph.D. Columbia 1960. The production of the Indian steel industry in the inter-war period.
- HAZEL J. WALDROP, Ph.D. Southern California 1960. A study of the major theories of economic development and decline.
- IVOR P. WOLD, Ph.D. Texas 1960. Economic change in Canada, pre-war to recent, emphasizing aggregates.

Theses in Preparation

- JAMSHID ASHRAFI, B.A. California 1953; M.A. San Francisco State 1955. Criteria for investment decision in underdeveloped economies—with special reference to Iran. *South-ern California*.
- ELIEZER AYAL, M.A. Hebrew 1954. Thailand's economy and public policies under the constitutional government—a case study of an underdeveloped country. *Cornell*.
- VIRBEHAN G. BHATIA, B.Sc. Banara Hindu 1951; M.A. Panjab 1954; M.A. Harvard 1959. Locational approach to economic development. *Harvard*.
- BRIAN W. BROGAN, B.Comm. Melbourne. Location problems in economic development. *Johns Hopkins*.
- ANDRZEJ BRZESKI, M.A. Lediz (Poland). Inflation in post-war Poland. *California (Berkeley)*.
- YOUNGIOB CHUNG, B.S. California (Los Angeles) 1952; M.A. Columbia 1955. State and capital formation in Japan. *Columbia*.
- GEORGE A. DONELY, B.A. Williams 1956; M.A. Columbia 1958. The reaction of New Orleans and the lesser Gulf ports to the Erie Canal and other East-West transportation improvements in the 19th century. *Columbia*.
- DANIEL J. EDWARDS, B.A. Maryland 1956; M.A. 1958. The process of inflation in a neutral country: Sweden during World War II. *Virginia*.
- LATTEE A. FAHEM, B.A. California (Berkeley) 1957; M.A. 1958. Economic planning in Nigeria: 1950-1975. *Massachusetts Inst. Technology*.
- A. GERLOF HOMAN, B.A. Bethel 1949; M.S. Kansas State 1952. The role of agriculture in development in Latin America. *Oregon*.
- CONSTANTINA S. KALMAN, B.A. American (Cairo) 1956; M.A. Stanford 1957. The growth of the Greek economy, 1927-1959. *Johns Hopkins*.

- RICHARD H. KAUFMAN, B.A. Brandeis 1957; M.A. Harvard 1959. The impact of commercial measures on the economic development of Israel. *Harvard*.
- MICHAEL KAVANAGH, B.A. University College Dublin 1952; M.A. 1954. An application of growth model concepts to the programming of development in the Irish economy. *Fordham*.
- DAVID J. LOSCHKY, B.A. Missouri 1956; M.A. Harvard 1958. The development of productivity in the American distribution system. *Harvard*.
- K. S. MALI, B.A. Panjab 1941; M.A. 1944. Financing economic development of Burma since Independence. *Indiana*.
- CHARLES Y. MANSFIELD, B.A. Oberlin 1955; M.P.A. Princeton 1958. Development banks: their role in economic growth. *Princeton*.
- PLACIDO L. MAPA, B.A. Ateneo de Manila 1955; M.A. St. Louis 1957. Development financing in an underdeveloped country. *Harvard*.
- ROBERT MUSCAT, B.A. Columbia 1952; M.A. 1957. Foreign aid and economic development. *Columbia*.
- RAMPRASAD B. PANDIT, B.A. Ahmedabad 1948; M.B.A. Southern California 1960; M.A. 1960. Planning techniques and economic development: India and China, 1945-59. *Southern California*.
- ABHAYKUMAR C. PARIKH, B.A. Gujarat 1952; M.A. Bombay 1955. India banking and economic development since 1947. *American*.
- HOLLIS W. PETER, B.S. Wisconsin 1938; M.S. Michigan 1956. Technical knowledge and economic growth: a case study of the Philippines. *Michigan*.
- ABDUL QADIR, B.A. Delhi College of Commerce 1949; M.A. Karachi 1953. Take-off to economic growth: a case study of Pakistan. *Clark*.
- LAURA RANDALL, M.A. Massachusetts 1959. The effects of large supplies of agricultural labor on Mexican economic development—1940 to date. *Columbia*.
- A. ROTSTEIN (earlier degrees not supplied). Institutional foundations of Canadian industrial development. *Toronto*.
- HOWARD F. SMITH, B.A. Wayne 1940; M.B.A. Harvard 1942. An analysis of Ceylon's ten year plan of economic development. *American*.
- SNOH UNAKUL, B.Comm. Melbourne 1954; M.A. Columbia 1958. Formulation and economic appraisal of Thailand's public development projects. *Columbia*.
- JAMES R. WASON, B.A. American 1949. Labor in the federal city in the Jacksonian era. *American*.
- JOHN A. WEIR, B.S. St. Dunstan's, P.E.I. 1953; M.B.A. Univ. Western Ontario 1955. Rural reconstruction on Prince Edward Island: an evaluation. *Notre Dame*.
- GEORGE R. WINTER, B.S. Alberta 1955; M.S. Iowa (Ames) 1958. External economies in relation to underdeveloped areas. *Iowa (Ames)*.
- LUIS F. YEPEZ, B.S. Wisconsin 1959; M.S. Wisconsin 1959. Market structure considerations and the theory of economic development. *Wisconsin*.
- JUNE ZACCONE, M.A. North Carolina 1954. Comparative economic development of India and China. *North Carolina*.

Statistical Methods; Econometrics; Social Accounting

Degrees Conferred

- JOHN O. BLACKBURN, Ph.D. Florida 1959. A balance sheet for the nation: a study in concepts.
- FRANKLIN M. FISHER, Ph.D. Harvard 1960. A priori information and time series analysis.
- SEYMOUR GOODMAN, Ph.D. Johns Hopkins 1960. Patterns of income inequality in states.

JOHN F. HABERER, Ph.D. Duke 1960. Some conceptual problems in moneyflows accounting: United States and Canada.

PAUL B. HENDERSON, JR., Ph.D. Mass. Inst. of Tech. 1960. Theory of data systems for economic decisions.

JANET M. HOOKS, Ph.D. Illinois 1960. The contribution of women to national income.

RUBIN SAPOSNIK, Ph.D. Minnesota 1959. Models for the analysis of capital equipment purchase policies.

Theses in Preparation

DAVE BRAMHALL (earlier degrees not supplied). Social accounting for regions. *Pennsylvania*.

JAMES MCKENNEY, B.S. Purdue 1952; M.S. 1952. A conceptual framework for multi-programming activities. *California (Los Angeles)*.

NORMAN RUSEFORTH, B.Sc. Birmingham 1954. A procedure for testing the hypothesis that K sample correlation matrices are from the same population. *Cornell*.

Economic Systems; Planning and Reform; Cooperation

Degrees Conferred

JOHN S. HOYT, JR., Ph.D. American 1959. An investigation of the economics of Soviet locational doctrine, policy and practice: with special emphasis on heavy industry.

MOHAMMED IMADY, Ph.D. New York 1960. Economic programming in the Syrian region of the United Arab Republic.

Theses in Preparation

FUAD S. ABU-ZAYYAD, B.A. Berea 1957; M.A. Fletcher School 1958; M.A.L.D. 1959. Economic implications of Arab unity. *Fletcher School*.

MAGDI M. EL KAMMASH, B.Comm. Cairo 1952; D.A. 1956; M.P.H. North Carolina 1958. On the use of national accounting in planning models for underdeveloped countries. *Duke*.

HY SANG LEE, B.A. Elmhurst 1958; M.S. Wisconsin. Economic planning in India and Communist China: a comparison. *Wisconsin*.

DAVID MERMELSTEIN, B.A. Amherst 1955. Recent theories on American capitalism. *Columbia*.

RAYMOND J. MONSEN, JR., B.S. Utah 1953; M.A. Stanford 1954. Ideologies of modern American capitalism. *California (Berkeley)*.

Business Fluctuations

Degrees Conferred

YOSSEF ATTIEYEH, Ph.D. Chicago 1959. Wage-price spiral versus demand inflation: United States, 1949-1957.

PETER D. STERNLIGHT, Ph.D. Harvard 1960. United States credit policy in the 1954-57 period.

RONALD P. WILLETT, D.B.A. Indiana 1959. A model for forecasting economic activity in Indiana and its subregions based on the use of current economic indicators.

Theses in Preparation

VINCENT CANGELOSI, B.S. Louisiana State 1954; M.B.A. 1956. Forecasting methods for predicting gasoline consumption. *Arkansas*.

VICTOR GARLIN, B.A. California (Berkeley) 1956. Technological change and business cycles in the post-war period. *California (Berkeley)*.

- CLARENCE L. HAM, B.S. Wisconsin 1947. The recession of 1957-58. *California (Berkeley)*.
 MICHEL E. A. HERVÉ, Diplôme Hautes Études Commerciales 1950; M.A. Harvard 1955. French inflation 1952-1958. *Harvard*.
 JOHN H. NIEDERCORN, B.A. Yale 1956; M.A. Harvard 1959. An econometric study of inflation. *Harvard*.
 JOHN P. ONDRECHEN (earlier degrees not supplied). Business cycles in the period after World War II. *Pennsylvania*.
 RUDOLPH G. PENNER, B.Comm. Toronto 1958. International influences on the effectiveness of stabilization policy in Canada. *Johns Hopkins*.
 JACK RICHARDSON, B.Comm. Toronto 1957. International transmission of inflation: the Canadian case. *Johns Hopkins*.
 HENRY L. WOJTYLA, M.A. Chicago 1953. Cyclical interrelationships in the postwar period. *Chicago*.

Money, Credit and Banking; Monetary Policy; Consumer Finance; Mortgage Credit

Degrees Conferred

- ABDELHAK BELKORA, Ph.D. Colorado 1960. Monetary policy in two post-accord recessions, 1953-54, 1957-58.
 WILLIAM BERANEK, Ph.D. California (Los Angeles) 1959. A study on the cost of capital.
 ORESTES W. CANDILIS, Ph.D. Georgetown 1960. Monetary and credit policies of the Bank of Greece and their influence on the Greek post-war economy, 1944-1958.
 ROBERT R. EDMISTER, Ph.D. California (Berkeley) 1960. Money and credit in Mexico, 1920-1940.
 RACHEL FLOERSHEIM, Ph.D. Johns Hopkins 1960. Financial intermediaries in Israel, 1950-1954.
 PETER G. FOUSEK, Ph.D. Columbia 1960. Foreign central banking: the instruments of monetary policy.
 RICHARD W. GRAVES, D.B.A. Indiana 1960. Anti-inflationary techniques of selected European countries.
 HARRY P. GUENTHER, D.B.A. Indiana 1959. Commercial bank lending and investing behavior during a period of restrictive federal monetary policy.
 T. E. HOLLANDER, Ph.D. Pittsburgh 1960. Economic significance of measures of capital formation and capital consumption in the business sector of the economy.
 MARSHALL KAPLAN, Ph.D. Chicago 1960. Neoclassical monetary theory.
 ALBERT M. LEVENSON, Ph.D. Columbia 1960. Differentials in interest rates and the cost of commercial bank lending.
 DAVID B. MCCALMONT, Ph.D. Johns Hopkins 1960. Redistribution of gold reserves among Federal Reserve banks.
 DANILO ORESCANIN, D.B.A. Indiana 1960. Organization of savings and loan associations.
 HUGH T. PATRICK, Ph.D. Michigan 1960. The Bank of Japan: a case study in the effectiveness of central bank techniques of monetary control.
 JAMES P. QUIRK, Ph.D. Minnesota 1959. Default risk and the loan market.
 KARL W. ROSKAMP, Ph.D. Michigan 1960. Economic growth, capital formation, and public policy in West Germany, 1948-57.
 HIRENDRA N. ROY, Ph.D. Stanford 1960. The role of monetary policy in economic development: a study of the activities of the Reserve Bank of India, 1949-56.
 DONALD H. SAUER, D.B.A. Indiana 1959. The supply of and the demand for nonfarm residential mortgage funds, 1960-70.

- VINODCHANDRA C. SHAE, Ph.D. Columbia 1960. Monetary policy and economic development with reference to India.
- CASE M. SPRENKLE, Ph.D. Yale 1960. Warrant prices as indicators of expectations and preferences.
- RICHARD H. TIMBERLAKE, JR., Ph.D. Chicago 1959. Treasury monetary policies from Jackson to Lincoln.
- ROY E. TUTTLE, Ph.D. Minnesota 1959. Leaseholds—their financial consequences and disclosure.
- DONALD A. TYREE, Ph.D. Texas 1959. The small-loan industry in Texas.
- TALHA YAFFI, Ph.D. Wisconsin 1959. The monetary and banking system of Lebanon, with special reference to monetary reform.

Theses in Preparation

- DANIEL AHEARN, B.A. Columbia 1949. Aspects of Federal Reserve policy, 1951 to date. *Columbia*.
- CHARLES C. BAKER, JR., B.A. Duke 1955; M.A. Harvard 1958. Commercial bank policy and participation in regional growth. *Harvard*.
- ARTHUR BENAIE, B.A. Wayne 1954; M.A. Michigan 1955. A statistical analysis of the impact of the behavior of non-bank financial institutions on the effectiveness of monetary policy. *Michigan*.
- NORMAND R. V. BERNARD, B.A. Assumption 1955; M.A. Boston College 1957. The "Bills Only" technique of open market operations. *Boston College*.
- EUGENE A. BRADY, B.A. Washington 1952; M.A. 1954. A study in mortgage credit and monetary policy. *California (Berkeley)*.
- WILLIS K. BRAMWELL, JR., B.S. Arizona 1950; M.A. Columbia 1955. Commercial bank holdings of consumer credit and central bank monetary policy. *Columbia*.
- ALBERT BRETON, B.S. St. Boniface. The demand for money: recent Canadian experience. *Columbia*.
- CONRAD P. CALIGARIS, B.B.A. Clark 1955; M.A. Brown 1958. A multiple correlation analysis of factors affecting net earnings of member banks in the First Federal Reserve District. *Brown*.
- ORESTES W. CANDILIS (earlier degrees not supplied). Monetary and credit policies of the Bank of Greece and their influences on the Greek post-war economy, 1944-1958. *Georgetown*.
- KANG CHAO, B.A. Taiwan 1951; M.A. Michigan 1957. Demand for cash by manufacturing corporations in the U. S. *Michigan*.
- HAROLD L. CHEADLE, B.A. Miami (Ohio) 1940. A re-examination of monetary velocity analysis. *American*.
- HENRY J. CLAYCAMP, B.A. Washburn 1956; M.A. Illinois 1957. Structure of consumer savings. *Illinois*.
- ADEL ELGOWHARY, B.Comm. Cairo 1951. Financial structure and monetary policy of Egypt. *Syracuse*.
- ALBERT FISHLOW, B.A. Pennsylvania 1956. A study in American monetary history. *Harvard*.
- RICHARD FRIEDMAN, B.A. City College of New York 1957. Open-market operations and liquidity theory. *Johns Hopkins*.
- DONALD D. HESTER, B.A. Yale 1957; M.A. 1958. An empirical examination of a loan offer function by commercial banks. *Yale*.
- RAYMOND W. HOOKER, B.S. Missouri 1956; M.S. 1959. Bank financing of business. *Wisconsin*.

- HAROLD L. JACKSON, B.A. California (Berkeley) 1956. Development of financial institutions, 1919-1933. *California (Berkeley)*.
- ROBERT A. JOHNSTON, B.Comm. Toronto 1956. An analysis of the Canadian floating discount rate and its implications for monetary policy. *Yale*.
- BERISLAV KARCIC, B.A. Columbia 1955. Monetary policy of Yugoslavia, 1948-1960. *Columbia*.
- DIONYSIOS S. KOTSONIS, B.A. Bowdoin 1953. The devaluation of the Greek drachma, 1954. *Columbia*.
- LEONARD LAUDADIO, B.A. College of Puget Sound 1956; M.A. Washington 1957. The adequacy of bank earnings. *Washington*.
- EARL MARTINSON, B.A. San Diego State 1951; M.B.A. California (Los Angeles) 1952. Management organization in the savings and loan industry. *California (Los Angeles)*.
- BRUCE T. MCKIM, B.S.C. Iowa 1950; M.A. 1958. Monetary velocity in the United States; from the Accord through 1959. *Iowa*.
- JOHN N. MCKINNEY, B.A. Washington 1954. Some aspects of the term structure of interest rates in the United States. *California (Berkeley)*.
- JACOB P. MEERMAN, B.A. Chicago 1954; M.A. 1953. Nicholas Biddle on central banking. *Chicago*.
- NICOCLES MICHAS, B.A. City College of New York 1957. Qualitative credit control in underdeveloped countries. *Columbia*.
- GAIL E. MULLIN, B.A. Wabash 1952; M.B.A. Indiana 1953. Growth and structural change in the savings and loan business. *Indiana*.
- KOJI NAKAGAWA, B.A. Kyoto Imperial Univ. 1953; M.A. Syracuse 1960. A money-flow approach to the analysis of cost-push inflation. *Syracuse*.
- CHARLES F. PEAKE, B.S. East Tennessee State 1956; M.S. Tennessee 1958. Financial intermediaries in the United Kingdom, 1850-1960. *Maryland*.
- BENEDICT J. PEDROTTI, B.A. Michigan 1952; M.A. 1956. Commercial bank portfolio adjustments during the period 1955-57. *Michigan*.
- R. BRUCE RICKS, B.S. California 1956; M.B.A. 1957. Trend to equity real estate investment by institutions in the post-war period. *California (Berkeley)*.
- BYONG HYUN SHIN, M.A. American 1954. Monetary and fiscal policy in South Korea, 1954-1960. *Columbia*.
- EDWARD P. SNYDER, B.A. Oberlin 1948. The homogeneity of the member bank business. *Chicago*.
- JAMES E. SUTTON, B.S. Wisconsin 1955. The composition of liquid asset holdings. *Michigan*.
- JOSE VERGARA, B.A. Maryland 1956. An analysis of opposition to the quantity theory of money. *Virginia*.
- NORMAN E. WEIR, B.A. Oklahoma 1935; M.A. Colorado 1941. Consumer instalment credit as a variable in the quantity and velocity of money in California, 1952-1960. *Southern California*.
- HARVEY J. WHEELER, B.A. Maine 1957. An examination of the hypothesis that money is a "luxury" good. *Virginia*.

Public Finance; Fiscal Policy

Degrees Conferred

- SAMUEL BLITMAN, Ph.D. Columbia 1960. The invested capital standard in the United States excess profits tax.
- JOHN D. COUPE, Ph.D. Clark 1960. Municipal debt control in Massachusetts and its countercyclical implications.

- IRVING GOFFMAN, Ph.D. Duke 1959. Erosion of the personal income tax base in Canada and the United States.
- JOHN F. GRAHAM, Ph.D. Columbia 1960. Provincial-municipal fiscal relations and economic development in a low-income province: Nova Scotia.
- WALTER T. GREANEY, JR., Ph.D. Harvard 1960. Expenditures and the budget in Massachusetts.
- JOSEPH R. GUERIN, Ph.D. Pennsylvania 1960. The development of the theory of excise taxes.
- REED R. HANSEN, Ph.D. Wisconsin 1960. The tax treatment of family income.
- JAMES M. HEIDELL, Ph.D. New York 1960. The purchasing of tax-exempt bonds by individuals in the 1946-1956 decade.
- ROBERT KINSEY, Ph.D. Columbia 1960. The role of lotteries in public finance.
- HARRY H. LANDRETH, JR., Ph.D. Harvard 1960. The measurement of local fiscal capacity.
- JAMES H. MALOON, Ph.D. Indiana 1960. The Ohio death taxes.
- HAROLD F. MCCLELLAND, Ph.D. Harvard 1960. Tax aspects of the variable annuity.
- WILLIAM J. MCKINSTRY, Ph.D. Yale 1960. An estimate of the impact of increasing death tax rates on beneficiaries of federally taxable estates.
- ALEXANDER S. POW, Ph.D. New York 1960. The comptroller general and the General Accounting Office of the United States.
- ARNOLD H. RAPHAELSON, Ph.D. Clark 1960. Massachusetts unemployment compensation 1948-57; a study in countercyclical finance.
- CHUNG-HIEH (JOHN) RIEW, Ph.D. Wisconsin 1960. Forty years of property values in Wisconsin.
- RODERICK H. RILEY, Ph.D. Wisconsin 1959. The "bonding period" in federal taxation of distilled spirits.
- ROBERT L. SLIGHTON, Ph.D. Johns Hopkins 1960. Taxation and investment incentives.
- EDWARD K. SMITH, Ph.D. Harvard 1960. The Massachusetts income tax.
- JOHN D. STRASMA, Ph.D. Harvard 1960. State and local taxes paid by manufacturers: a new comparison.
- JOSEPH WACHTEL, Ph.D. New York 1959. The effect of litigated cases on the Internal Revenue Code.
- LAURENCE N. WOODWORTH, Ph.D. New York 1960. Taxation by the United States of income earned abroad.
- GEORGE S. ZARKOS, Ph.D. Indiana 1960. The sales tax system of Greece.

Theses in Preparation

- JOHN R. ALLAN, B.A. McMaster 1955. The effect of fiscal policy in redistributing incomes in Canada. *Princeton*.
- ROBIN BARLOW, B.A. Oxford 1954; M.B.A. Michigan 1958. The role of fiscal policy in the economic development of Egypt. *Michigan*.
- EARL S. BEECHER, B.A. Utah 1949; M.B.A. California (Los Angeles) 1956. Public finance in California. *California (Los Angeles)*.
- RICHARD M. BIRD, B.A. King's College (Canada) 1958. Initial allowances and investment allowances under the British income tax: countercycle policy. *Columbia*.
- ROBERT O. BOSTON, B.S. Alabama 1949; M.S. 1950. An economic analysis of the problem of controlling municipal indebtedness in Alabama. *Alabama*.
- EDITHA BRANNOCK, A program for equitable financing of the public welfare function in North Carolina. *North Carolina*.
- EDWARD W. BRENNAN (earlier degrees not supplied). The personal property tax in Pennsylvania. *Pennsylvania*.
- ELIZABETH J. DAVID, B.A. Swarthmore 1953; M.A. Michigan 1957. The economic implications of attitudes toward state and local finance. *Michigan*.

- ARAKKAL T. EAPEN, B.A. Madras 1945; M.B.A. Michigan 1954. A study of federal finance in selected countries: U.S., Australia, Canada, and India. *Michigan*.
- KARL D. GREGORY, B.A. Wayne State 1952; M.A. 1957. State and local expenditures for publicly supported institutions of higher education. *Michigan*.
- JAMES HEILBRUN, B.S. Harvard 1945; M.A. 1947. Real estate taxation and rehabilitation of urban dwellings. *Columbia*.
- LEON KOROBOW, B.A. Brooklyn 1954. Relevence of theory of risk to public expenditures for life-saving and accident-averting purposes. *Columbia*.
- WILLIAM E. LATRD, B.S. Stetson 1956; M.A. George Washington 1958. The theory of debt management. *Virginia*.
- ABU N. M. MAHMOOD, M.A. Dacca 1942. Deficit-financing for economic development-case study. *Harvard*.
- ERLING O. NAESETH, B.A. Luther 1947; M.S. Wisconsin 1949. Financial support for education in Iowa. *Wisconsin*.
- ABDEL RAHMAN, B.A. Cairo 1949; M.A. Kentucky 1957. The Egyptian income taxation of non-resident alien and foreign corporations and its effect on the Egyptian economic growth. *Indiana*.
- ROBERT W. RESEK, B.S. Illinois 1957; M.A. Harvard 1960. Fiscal policy and economic growth. *Harvard*.
- G. REZEK, B. of Bus. Admin. American Univ. (Beirut) 1951; M.A. McGill. Public finance with special reference to underdeveloped countries of the Middle East. *McGill*.
- MICHAEL D. TANZER, B.A. Harvard 1957; M.A. 1960. The income elasticity of state and local governmental revenues and expenditures. *Harvard*.
- ARAN THAMMANO, B.A. Thammasat (Thailand) 1953; M.A. Michigan 1958. An evaluation of public debt management policies, 1946-1958. *Oregon*.
- JAN V. TUMLIR, B.A. Yale 1953; M.A. 1955. Taxes, public expenditures and the balance of payments: Germany, 1954-58. *Yale*.
- LYLE L. TYNER, B.S. Kansas 1933; B.A. Denver 1948. Special assessments in local government finance. *Wisconsin*.

International Economics

Degrees Conferred

- DAVID J. ASHTON, Ph.D. Fletcher School 1959. The meaning of export origin.
- ROBERT F. BARLOW, Ph.D. Fletcher School 1960. United States exports and imports of cotton textiles.
- NATHAN BRODSKY, Ph.D. American 1959. Devaluation and the British dollar gap in the postwar period 1946-55.
- ARCHIBALD C. CALLAWAY, Ph.D. Harvard 1960. Adjustment problems of a primary exporting economy.
- MANUEL O. DIAZ, Ph.D. Pennsylvania 1960. The Spanish average principle as practiced in the trade between Spain and the Indies.
- ROBERT R. DINCE, Ph.D. Cornell 1960. The lending policies of the Export-Import Bank, 1945-52.
- LYMAN A. DREWRY, JR., Ph.D. Virginia 1960. Offsetting interventions in the international market: cotton and cotton textiles as a case study.
- HORST H. H. ESCHENBERG, Ph.D. Purdue 1960. German balance of payments problems since 1950.
- RONALD E. FINDLAY, Ph.D. Mass. Inst. of Tech. 1960. Essays on some theoretical aspects of economic growth.
- RICHARD L. GORDON, Ph.D. Mass. Inst. of Tech. 1960. Coal pricing and the energy problem in the European community.

- JACK D. GUENTHER, Ph.D. Harvard 1960. The Mexican balance of payments, 1950-58.
- CHARLES W. HULTMAN, Ph.D. Iowa 1960. Agricultural surplus disposal and foreign aid.
- STEPHEN H. HYMER, Ph.D. Mass. Inst. of Tech. 1960. International operations of national firms—a study of direct foreign investment.
- LELAND LANGBEIN, Ph.D. Pittsburgh 1960. International movement of petroleum and petroleum products.
- JUNG-CHAO LIU, Ph.D. Michigan 1960. An econometric model of the rice market in the Japanese Empire, 1910-1937.
- SARAH S. MONTGOMERY, Ph.D. Wisconsin 1960. The terms of trade of primary products and manufactured goods in international trade, 1872-1952.
- ROBERT S. OZAKI, Ph.D. Harvard 1960. Japan's postwar resurgence in international trade.
- MARCEL K. RICHTER, Ph.D. Mass. Inst. of Tech. 1959. Some economic problems in an uncertain world.
- HERBERT I. SCHILLER, Ph.D. New York 1960. The United States Congress and the American financial contribution to the United Nations Relief and Rehabilitation Administration.
- ROBERT G. SCHROEDER, Ph.D. California (Berkeley) 1959. British and the European common market.
- MARTIN W. WILMINGTON, Ph.D. New York 1960. Economic regionalism in the Middle East during World War II (the Middle East supply center).

Theses in Preparation

- ROBERT J. BALL (earlier degrees not supplied). Econometric analysis of the U. K. balance of payments. *Pennsylvania*.
- VLADIMIR N. BANDERA, B.A. Connecticut 1954. International capital movements in Eastern Europe between the World Wars. *California (Berkeley)*.
- RUTH A. BIRDZELL, M.S. Illinois 1952. Postwar investment in Australia. *Illinois*.
- DAVID W. BODENBERG, B.A. Yale 1955; M.A. Princeton 1958. The soft loan: studies in international trade theory and contemporary policy. *Princeton*.
- GEORGE H. BOSSY, M.A. Columbia 1956. Effects of commercial policy on economic development in Australia. *Columbia*.
- RICHARD N. COOPER, B.A. Oberlin 1956; M.Sc.Econ. London 1958. Inflation, growth and the U.S. balance of payments, 1956-59. *Harvard*.
- ANA N. EAPEN, B.A. Univ. of the Philippines 1950; M.A. Michigan 1958. Exchange controls in the Philippines. *Michigan*.
- A. GEORGE GOLS, B.A. Upsala 1954; M.A. Johns Hopkins 1957. United States foreign petroleum investments and public economic policy. *Oregon*.
- WARREN R. HARDEN, B.A. Iowa State Teachers 1950; M.A. Colorado 1951. Central American economic integration. *Indiana*.
- MOHAMMED P. HASAN, B.A. Panjab 1950; M.A. 1952. Inflation and balance of payments: the experience of Pakistan, 1950-59. *Yale*.
- CHARLES W. HULTMAN, B.A. Upper Iowa 1952; M.A. Drake 1957. Agricultural surplus disposal and foreign aid. *Iowa*.
- OMESH KHANNA, B.A. Albion 1953; M.B.A. Michigan 1955. The balance of payments difficulties in semi-planned economies: the Indian experience. *Michigan*.
- HEINZ KOHLER, Vordiplom Free Univ. of Berlin 1956; M.A. Michigan 1958. International economic relations within the Communist bloc. *Michigan*.
- HERMES LEMONIAS, B.A. American Univ. (Cairo) 1954; M.A. Brown 1956. European economic integration and Greece. *Brown*.
- ROBERT LIPSEY, B.A. Columbia 1944. Study of trends in U.S. international trade: export and import volume and price indexes for U.S. foreign trade since 19th century. *Columbia*.

- RICHARD D. MALLON, B.A. Princeton 1949. Industrialization and import replacement. *Harvard*.
- JOSEPH A. MARTELLARO, B.A. Notre Dame 1956; M.A. 1958. World bank loans to Italy and the effects on post-war economic growth. *Notre Dame*.
- MICHAEL MISCHAIKOW, M.A. Indiana 1957. Postwar effects of variations in German fuel requirements upon United States coal exports. *Indiana*.
- RUSSELL MORAN, B.A. California 1954. Trade nationalization in the Philippines: an instrument of national economic policy. *Cornell*.
- GORDON R. MUNRO, B.A. British Columbia 1956; M.A. Harvard 1959. Evolution of the British foreign exchange control system in the postwar era. *Harvard*.
- AHMAD A. MURAD, B.A. Washington State 1956; M.A. Wisconsin 1960. Problems of capital accumulation in Egypt. *Wisconsin*.
- JAMES M. MURRAY, B.S. North Dakota 1956; M.A. 1958. Taxation and private foreign investment. *Oregon*.
- SAID NABULSI (earlier degrees not supplied). The theory and the practice of economic integration as applied to the monetary aspects of the Syrian-Egyptian union. *Georgetown*.
- JAMES K. NETTLES, B.S.C. Spring Hill 1957. The interregional balance of payments of the United States. *California (Berkeley)*.
- JOSEPH G. POLACH, LL.D. Masaryk (Czechoslovakia). Euratom: a study in the European economic integration. *American*.
- FRANK W. SCHIFF, B.A. Columbia 1942. Economic impact of U.S. aid on selected Asian countries. *Columbia*.
- CHARLES SIEGMAN, B.A. City College of New York 1957. Uses of export controls in international trade of primary producing economies. *Columbia*.
- KENJI TAKEUCHI, B.A. Kwansee Gakauin (Japan) 1956. The special features of foreign investment in Japan, 1950-1959, and its effects upon her economic growth. *Duke*.
- LEO TANSKY, B.S. Syracuse 1950; M.S. Columbia 1951. Comparative analysis of the impact of U.S. and U.S.S.R. economic aid to underdeveloped countries with special reference to India, Turkey and the United Arab Republic. *American*.
- ORVEL L. TRAINER, B.A. Colorado 1950; M.A. 1955. The Scandinavian approach to the common market. *Colorado*.
- DONALD A. WELLS, B.A. DePauw 1953; M.A. Virginia 1958. Servicing U.S. direct foreign investment. *Oregon*.
- MARINA WHITMAN, B.A. Radcliffe 1956; M.A. Columbia 1959. Government participation in U.S. private foreign investment. *Columbia*.
- JEFFREY G. WILLIAMSON, B.A. Wesleyan 1957; M.A. Stanford 1959. Long swings and United States balance of payments, 1800-1914. *Stanford*.
- RICHARD B. WIRTELIN, B.S. Utah 1956; M.A. 1957. The growth dynamics of linkages in the foreign sector. *California (Berkeley)*.

Business Finance; Investment and Security Markets; Insurance

Degrees Conferred

- WILLIAM H. L. ANDERSON, Ph.D. Harvard 1960. An econometric study of capital investment in the United States manufacturing industry, 1948-57.
- HASKELL BENISHAY, Ph.D. Chicago 1960. Determinants of variability in earnings price ratios of corporate equity.
- NAI-RUENN CHEN, Ph.D. Illinois 1960. Factors influencing investment expenditures.

- ALAN B. COLEMAN, Ph.D. Stanford 1960. Financial management of foreign operations.
- EDWIN B. COX, Ph.D. Pennsylvania 1960. Trends in the distribution of stock ownership.
- LYNN E. DELLENBARGER, JR., Ph.D. Florida 1960. A study of relative common equity value in fifty mergers of listed industrial corporations, 1950-1957.
- ROBERT S. FELTON, D.B.A. Indiana 1960. Selected problems faced by fire and casualty insurers under state and local taxation.
- JOHN HALL, Ph.D. Pennsylvania 1960. The regulation of commercial health insurance for the individual.
- CHARLES W. HOWE, Ph.D. Stanford 1959. A theoretical and empirical investigation of internal financing.
- LAWRENCE D. JONES, JR., Ph.D. Harvard 1960. Portfolio objectives, external constraints, and the postwar investment behavior of life insurance companies.
- MELVILLE PETERSON, Ph.D. Illinois 1960. A comparative study in debenture and mortgage bond financing.
- NESTER R. ROOS, D.B.A. Indiana 1959. Government regulations of fire insurance.
- STUART SCHWARTZCHILD, Ph.D. Pennsylvania 1960. The rights of creditors in life insurance policies.
- DAVID A. SNELL, Ph.D. Texas 1960. Financial problems and the availability and adequacy of external financial resources for small firms.
- FUAD H. TELLEW, Ph.D. Southern California 1959. Private foreign investment as a possible aid for the economic growth of Iraq.
- DAN USHER, Ph.D. Chicago 1960. The debt-equity ratio.
- SHENG W. WANG, Ph.D. Wisconsin 1959. Some fundamental problems of equipment investment planning.
- WALTER WILLIAMS, Ph.D. Indiana 1960. Determining the actual cash value of commercial real property for insurance purposes.

Theses in Preparation

- JOSEPH BELTH (earlier degrees not supplied). An analysis of participating ordinary life insurance issued by stock life insurance companies in the U.S. and Canada. *Pennsylvania*.
- WILLARD T. CARLETON, B.A. Dartmouth 1956. Economic implications of the financial decisions of corporations. *Wisconsin*.
- ROBERT CROWE (earlier degrees not supplied). Underwriting experience in automobile physical damage insurance. *Pennsylvania*.
- CHARLES D'AMBROSIO, B.S. Loyola 1955; M.S. Illinois 1958. Structural changes in railroad financial plans: 1928-1958. *Illinois*.
- WALTER S. FRANK, B.A. Harvard 1949; B.A. Oxford 1952. Portfolio behavior of the investment trusts. *Harvard*.
- PAUL HAGGIPAVLOU, B.Sc. Ohio State 1957; M.A. 1958. Determinants of business investment—an empirical investigation in the steel industry. *Columbia*.
- BOB E. HALL, B.S. Arkansas 1952; M.B.A. 1958. The variable annuity—the payout period. *Arkansas*.
- VICTOR HALLMAN (earlier degrees not supplied). Comparison of compulsory automobile insurance with unsatisfied judgment funds. *Pennsylvania*.
- ROBERT HUNGATE, B.A. Washington 1951. Inter-business financing. *California (Los Angeles)*.
- E. W. KIERANS, B.A. Loyola 1935. Financing corporate activity in Canada, 1936-58. *McGill*.
- HERMAN I. LEIBLING, B.A. City (New York) 1935; M.A. American 1945. Financing of the postwar expansion of business: an industry analysis. *American*.

- ROLAND T. MULLINS, B.A. Arkansas State 1956; M.B.A. Arkansas 1958. An investigation into the effects of inflation on the market for bonds. *Arkansas*.
- JAMES T. MURPHY, B.Sc. Iowa 1954; M.A. 1957. An empirical study of the investment of three industries (1954-1958). *Iowa*.
- J. RUSSELL NELSON, B.A. Pacific Union 1952; M.B.A. California (Los Angeles) 1957. The role of stock rights in corporate financial policy. *California (Los Angeles)*.
- MARSHALL PUCKETT (earlier degrees not supplied). Dividend policy; cost of capital; and stock price maximization. *Pennsylvania*.
- JOHN C. RITCHIE, JR. (earlier degrees not supplied). Trends in internal financing 1915-1955, selected large manufacturing corporations. *Pennsylvania*.
- ALEXANDER A. ROBICHEK, B.S. California 1956. Allocation of funds—mutual life insurance companies. *California (Berkeley)*.
- HOWARD J. SHERMAN, B.A. California (Los Angeles) 1950; J.D. Chicago 1953; M.A. Southern California 1957. Profit rates: relation to cyclical variations and corporate size, United States, 1931-1958. *California (Berkeley)*.
- PETER E. SLOANE, B.A. Yale 1947; M.A. 1958. Determinants of bond yield differentials, 1954-1959. *Yale*.
- CECIL E. WALTON (earlier degrees not supplied). A study of some of the aspects of corporate refunding operations. *Arkansas*.
- ROYALL WHITAKER (earlier degrees not supplied). Investigation of probability aspects of life rating and selection. *Pennsylvania*.
- HOWARD WIDDOWSON (earlier degrees not supplied). Exchange of individual contracts in the area of life insurance and annuities. *Pennsylvania*.

Business Organization; Managerial Economics; Marketing; Accounting *Degrees Conferred*

- PETER C. BRIANT, Ph.D. Michigan 1960. The corporation income tax: its incidence and effects.
- JOHN E. CHAMPION, Ph.D. Michigan 1960. Effectiveness of LIFO in the textile industry.
- ELWYN K. DE VORE, D.B.A. Indiana 1960. A study of selected factors which may influence the movement of selected types of retailers away from the downtown shopping area of small cities.
- HARALD EINSMANN, Ph.D. Florida 1960. The effects of the formation of the European economic community on managerial decisions of entrepreneurs in member countries.
- PERSIS R. EMMETT, Ph.D. Stanford 1960. The development and location of shopping centers; and common location characteristics in selected areas in California.
- WALTER D. GAINER, Ph.D. Mass. Inst. of Tech. 1959. Regional income leakages and the multiplier: Alberta, 1948-58.
- JUDITH A. GROUSE, Ph.D. Harvard 1960. Executive compensation and how it is determined: the theory of mutual backpatting.
- JOHN W. HENDERSON, JR., Ph.D. Wisconsin 1960. Import competition in manufacturing industries.
- JIMMIE L. HESKETT, Ph.D. Stanford 1960. Industrial logistics: a movement system concept.
- CHARLES H. HINDERSMAN, D.B.A. Indiana 1960. The changing balance of retail trade between downtown and outlying stores in metropolitan areas.
- IRA HOROWITZ, Ph.D. Mass. Inst. of Tech. 1959. Economics of industrial research.
- DONALD F. ISTVAN, D.B.A. Indiana 1960. The capital-expenditure decision-making process in forty-eight large corporations.
- THOMAS F. KELLER, Ph.D. Michigan 1960. The interperiod allocation of corporate income tax.

- TOSINOBU G. F. KITANO, Ph.D. Saint Louis 1960. A genetic study of German managerial policy of works community.
- WALTER H. KRAMER, D.B.A. Indiana 1960. The role of the travel agent in the marketing of domestic air travel.
- J. D. LANDES, Ph.D. North Carolina 1960. An analysis of classified advertising in newspapers.
- CHI (DAVID) LUAN, Ph.D. Texas 1959. Statistical quality control in cotton marketing.
- PAUL W. MACAVOY, Ph.D. Yale 1960. Price formation in natural gas fields.
- GEORGE G. MILLER, Ph.D. Texas 1959. A critical analysis and appraisal of the theory and techniques of the management audit.
- CHARLES V. MOORE, Ph.D. Ohio State 1959. An evaluation of farm accounting systems as aids to the management of commercial farms.
- WALTER O'DONNELL, Ph.D. Columbia 1960. The value structure of corporate decisions.
- TSVI OPEIR, Ph.D. Mass. Inst. of Tech. 1960. Intracompany pricing in the decentralized firm.
- BEDROSE P. PASHIOIAN, Ph.D. Mass. Inst. of Tech. 1960. Automobile distribution: an economic analysis of the franchise system.
- DONALD L. RICHARD, Ph.D. American 1959. The economic effects of the reducing charge methods of depreciation.
- GEORGE SCHWARTZ, Ph.D. Pennsylvania 1960. Development of marketing theory (an evaluation of certain approaches being used in the development of marketing theory).
- MAURICE SELDIN, D.B.A. Indiana 1960. An analysis of the impact of the firm on urban plant problems.
- WILLIAM A. SHEPPARD, Ph.D. Mass. Inst. of Tech. 1960. Social aspects of operations research.
- ALLAN T. STEELE, Ph.D. Texas 1960. A history of auditing in the United States, 1914-1957.
- JAMES D. TAYLOR, Ph.D. Iowa 1960. A description, analysis, and partial explanation of changes in areal distribution of retail sales for selected groups in the central cities of ninety-five standard metropolitan areas from 1948 through 1954.
- KENNETH P. UHL, Ph.D. Iowa 1960. Stockowners as customers for their corporations' products.
- SHERWOOD G. WALTERS, Ph.D. New York 1960. Marketing in Brazil tested according to recent economic theory.
- MARTIN R. WARSHAW, Ph.D. Michigan 1960. Manufacturer-wholesaler relations and their influence on pattern of distribution in selected consumer goods industries.
- HILDA C. WASSON, D.B.A. Indiana 1959. Retail pricing policies and practices.
- GERALD O. WENTWORTH, Ph.D. Stanford 1959. An evaluation of direct costing.
- DELBERT E. WILLIAMSON, Ph.D. Stanford 1960. The concept of full disclosure in current accounting practice.

Theses in Preparation

- DONALD K. ABE, B.S. Pennsylvania 1950; M.B.A. California 1955. Data for managerial control. *California (Berkeley)*.
- R. CLIFTON ANDERSEN, B.S. Indiana 1955; M.B.A. 1958. An analysis of the marketing wholesalers in the State of Indiana. *Indiana*.
- ARTHUR F. BELOTE, B.S. Ohio State 1952; M.B.A. 1953. A study leading to the development of a more quantitative and systematic approach to hospital job evaluation. *Florida*.
- ROBERT H. BOCK, B.S. Purdue 1954; M.S. 1955. An analysis of the long-range planning process in a select group of business firms. *Purdue*.

- RALPH DAY, M.S. Georgia Institute of Technology 1955. Applications of linear programming to marketing. *North Carolina*.
- JOHN H. DROGE, B.S. Kansas State 1953; M.S. North Carolina State 1959. Interregional competition in marketing Wisconsin commercial Irish potatoes. *Wisconsin*.
- JOE R. FRITZMEYER, B.B.A. Baylor 1956; M.B.A. Indiana 1957. Management and financial control of automobile dealer agencies. *Indiana*.
- LEONARD J. GARRETT (earlier degrees not supplied). Information systems and managerial decision making and control. *Pennsylvania*.
- JAC L. GOLDSTUCKER, B.B.A. Oklahoma 1940; M.B.A. Southern Methodist 1952. The dynamics of wholesale trading areas. *Minnesota*.
- IVAN L. HALL (earlier degrees not supplied). A study of the problems of cost determination in the production of oil. *Arkansas*.
- HERBERT G. HICKS, B.C.E. Georgia Inst. of Tech. 1954; M.B.A. Alabama 1958. The quantification of managerial decision problems. *Alabama*.
- FRED J. HOFFER, B.S.A. Florida 1953; M.A. 1955. The cost of packing and distribution of Florida honey. *Florida*.
- ARTHUR HOVERLAND, B.S. Miami 1951; M.S. Illinois 1954. A critical study of certain accounting principles and their possible incompatibility with management decision making. *Michigan*.
- JOHN G. HUTCHINSON, B.S. Rhode Island 1951; M.S. 1953. Administration of production standards. *Michigan*.
- MICHAEL INGRAHAM, B.S. California (Los Angeles) 1952; M.B.A. 1953. The management of franchising organizations with particular reference to planning and control principals and practices. *California (Los Angeles)*.
- HAROLD KATZ, B.A. Brandeis 1956. The effects of electronic data processing innovations on diseconomies of management and optimum size of firms. *Columbia*.
- ROBERT G. KOKAT, B.S. Pennsylvania State 1956; M.S. 1957. The impact of disarmament on the composition of industrial output. *Indiana*.
- CRAIG LUNDBERG, B.A. Washington 1954; M.B.A. 1957. Decisioning in industrial organization. *Cornell*.
- AVRUM W. W. MARKS (earlier degrees not supplied). An analysis of motivations for purchase and ownership characteristics of imported automobile (retailing at less than \$2,000) owners residing in Phila., Pa. *Pennsylvania*.
- ROBERT C. MEIER, B.S. Indiana 1952; M.A. Minnesota 1955. Administration of industrial operations research. *Minnesota*.
- ROBERT J. MEYER, B.A. Massachusetts 1956; M.A. Connecticut 1951. An investigation of the economic consequences of the use of more capital intensive production. *Harvard*.
- GERHARD G. MUELLER, B.S. California 1956; M.B.A. 1957. The accounting standards and practices of several foreign countries and their implications for financial analysis in the U.S. *California (Berkeley)*.
- WILLIAM NELSON (earlier degrees not supplied). The development of the concept of the firm. *Pittsburgh*.
- FRANCESCO M. NICOSIA, Dott. Economia e Commercio Rome 1949. Decision making: toward a paradigm of consumer decision making. *California (Berkeley)*.
- JAMES OMPS (earlier degrees not supplied). A regional case study of economic methods and motivation of small business proprietors. *Pittsburgh*.
- GEORGE B. SIMMONS, B.A. Louisville 1953; M.B.A. Indiana 1957. A theoretical framework for the evaluation of market potential in underdeveloped countries. *Indiana*.
- JOHN F. STOLLSTEIMER, B.S. Michigan State 1953; M.S. 1957. The impact of the introduction of bulk handling techniques on the marketing of deciduous fruits. *California (Berkeley)*.

- LAWRENCE X. TARPEY, M.B.A. Indiana 1955. Some economic and administrative aspects of food chain advertising: a case study. *Cornell*.
- RUSSELL A. TAUSSIG, B.S. California 1941; M.B.A. 1947. Use of accounting and economic costs in operations research models. *California (Berkeley)*.
- WILLIAM W. THOMPSON, B.S. Clemson 1953; M.B.A. Alabama 1958. A managerial history of a textile manufacturing company. *Alabama*.
- HARVEY TSCHIRGI, M.B.A. Chicago 1949. An experimental study of interpersonal influence in executive decision making. *California (Los Angeles)*.
- C. EUGENE VINCENT, B.S. Indiana 1953; M.B.A. 1957. The application of control procedures in retail hardware stores. *Indiana*.
- RICHARD J. WHITING, B.A. Washington 1947; M.B.A. Stanford 1949. An analysis of management functions as influenced by organized labor. *Southern California*.
- FLOYD W. WILLIAMS, B.S. Virginia Polytechnic Inst. 1953; M.S. 1958. An evaluation of consumer preference and the effects of price variations and selected fruit characteristics on retail sales of Florida avocados. *Florida*.

Industrial Organization; Government and Business; Industry Studies

Degrees Conferred

- ALLEN E. ABRAHAMS, Ph.D. New York 1960. An economic analysis of the American chemical nitrogen industry.
- BEN ALVORD, Ph.D. Illinois 1960. A study of the financing of the U.S. trunk airlines, 1946-1955.
- DANIEL BASTLE, Ph.D. Cornell 1959. Development and critical analysis of the food program of the military forces of the United States.
- ALBERT BUCKBERG, Ph.D. Michigan 1960. The interindustry structure among manufacturing industries in Michigan and Detroit, 1929-1955.
- JAMES P. CAIRNS, Ph.D. Johns Hopkins 1960. Integration in the food retailing industry.
- ALFRED B. CARLIP, Ph.D. Columbia 1960. The slide-fastener industry: a study of market structure innovations.
- ELEANOR CRAIG, Ph.D. Duke 1959. Recent history of the North Carolina furniture manufacturing industry with special attention to locational factors.
- HERSCHEL CUTLER, Ph.D. Syracuse 1960. Government transportation: the reduced rate controversy.
- OTTO A. DAVIS, Ph.D. Virginia 1960. The economics of municipal zoning.
- CHARLES R. DEAN, Ph.D. Columbia 1960. Industrial organization and monopoly behavior.
- NORTON T. DODGE, Ph.D. Harvard 1960. Trends in labor productivity in the Soviet tractor industry.
- FRED DZIADZEK, Ph.D. Johns Hopkins 1960. The productivity of the U.S. Post Office: an intertemporal and cross-sectional study of post office labor productivity.
- DANIEL O. FLETCHER, Ph.D. Michigan 1960. A study of package freight carriers on the Great Lakes.
- MILTON S. GOLDBERG, D.B.A. Indiana 1960. Consent decrees in Sherman Act cases: an analysis of antitrust enforcement by negotiation.
- F. J. HAYES, Ph.D. McGill 1960. The pulp and paper industry in Canada: an analysis of the restrictions on competition.
- EUGENE C. HOLSHOUSER, Ph.D. Kentucky 1960. The effect of the Lexington, Kentucky, northern belt line on land values and land use: a case study.
- WILLIAM R. HUGHES, Ph.D. Harvard 1960. The efficient organization of the privately owned electric utility industry in the United States.
- RICHARD A. LA BARGE, Ph.D. Duke 1959. A study of United Fruit Company operations in Isthmian America, 1946-1956.

- DONALD A. MARKWALDER, Ph.D. Northwestern 1960. The flour milling industry—an economic study of excess capacity.
- CHARLES O. MEIBURG, Ph.D. Virginia 1960. The “free” public road and government highway policy decisions.
- NORMAN A. MERCER, Ph.D. Harvard 1960. Growth and financing of the major firms in the electrical manufacturing industry.
- EDWARD D. PETERSON, D.B.A. Indiana 1960. Selected issues arising from government regulation of the public utility industries.
- CHARLES F. PHILLIPS, JR., Ph.D. Harvard 1960. Competition in the synthetic rubber industry.
- STUART M. RICH, D.B.A. Indiana 1960. Electric home heating in the United States: its growth, industry, structure, equipment, promotion, marketing and outlook.
- ROBERT A. ROBERTSON, Ph.D. Illinois 1960. An analysis of the motor vehicle industry as a factor in the economic life of Canada.
- ROBERT M. ROESTI, Ph.D. Southern California 1960. Economic analysis of factors underlying pricing in the southern California tuna canning industry.
- RASUL SALMAN, Ph.D. Columbia 1960. A distribution of gains from advancing productivity: in some concentrated and non-concentrated industries.
- WILLIAM A. SANDRIDGE, Ph.D. Virginia 1960. The effects of fair trade on retail prices of electric housewares in Washington, Baltimore, and Richmond, 1952-1959.
- WILLIAM D. SHIPMAN, Ph.D. Columbia 1960. An inquiry into the high cost of electricity in New England.
- EARL A. SPILLER, Ph.D. Michigan 1960. Significance and influence of accounting data adjusted for price level changes on the problems and policies of public utilities.
- MAX D. STEWART, Ph.D. Michigan State 1960. Some economic aspects of the Canadian wooden match industry and public policy.
- FREDRIC STUART, Ph.D. Columbia 1960. The effects of television on the motion picture and radio industries.
- HAROLD WEIN, Ph.D. Pittsburgh 1959. An integration of economic and legal approaches to growth in the steel industry.
- PAUL WEINER, Ph.D. Clark 1960. Highway planning and pricing.
- WESLEY J. YORDON, JR., Ph.D. Harvard 1960. Industrial concentration and prior flexibility in inflation: price response rates in fourteen industries, 1947-58.
- ZENON S. ZANNETOS, Ph.D. Mass. Inst. of Tech. 1959. Theory of oil tankship rates.

Theses in Preparation

- WILLIAM C. ADAMS, B.A. Arkansas State Teachers 1951; M.A. Arkansas 1953. The probable impact of inflation upon electric power company operations in Arkansas. *Arkansas*.
- LEWIS N. AMIS, B.A. Peabody 1951; M.A. 1952. An economic history of gasoline: a case study in commodity economics. *Arkansas*.
- MCDONALD P. BENJAMIN, D.I.C.T.A. Imperial College of Trop. Agric. (B.W.I.) 1945; M.A. California (Davis) 1956. California's fruit and vegetable canning industry, a study of the changing relationships of input in the industry. *California (Los Angeles)*.
- MICHAEL BORETSKY, Diplom-Volkswirt Erlangen 1949; Dr. rerum politicarum Free Univ. 1949. The cost and economies of scale in soviet-machine-tool building industry. *Columbia*.
- JOHN CARLSON, B.S. Denison 1955. Investment decisions in the face of rapid technological progress: a case study of telephone exchanges. *Johns Hopkins*.
- ARTHUR L. COBB, B.S. Florida 1950; M.A. 1952. The concept of the market in antitrust cases: a study in applied economics. *Indiana*.

- BERT M. EVANS, B.S. Nebraska 1953; M.A. Texas 1954. Internal-external efficiencies and structural change in the perishable bakery products industry. *Harvard*.
- IVAN HALL, B.S. Arkansas State Teachers 1956; M.B.A. Arkansas 1958. A study of the problems of cost determination in the production of oil. *Arkansas*.
- R. B. HARSHBARGER, B.S. Manchester 1956; M.A. Indiana 1958. An analysis of TVA coal buying program. *Indiana*.
- ROBERT S. HIMES, B.S. American 1951; M.B.A. 1955. The problem of stability in the machine tool industry. *American*.
- DAVID HORLACHER (earlier degrees not supplied). Measurement of industrial capacity. *Pennsylvania*.
- GEORGE R. HORTON, JR., B.S. Auburn 1952; M.S. 1953. An examination of the economic consequences of the consolidation of the southeastern railroads. *Virginia*.
- EDWARD B. JAKUBAUSKAS, B.A. Connecticut 1952; M.A. 1954. Impact of technological change in the railroad industry—1947-1957. *Wisconsin*.
- WALTER P. KLEIN, B.A. Wisconsin 1956; M.A. 1957. Some relationships between industrial concentration and market growth. *Wisconsin*.
- EUGENE KOZIK (earlier degrees not supplied). Economic analysis utilized by the Supreme Court in its anti-trust decisions with special emphasis on the years 1946-59. *Pittsburgh*.
- GERALD KRAFT, B.A. Wayne 1955; M.A. Harvard 1957. Methods of statistical costing in the transportation industry. *Harvard*.
- R. F. LINDSAY (earlier degrees not supplied). A history of the natural gas industry in Canada. *Toronto*.
- SUILLIN LING, B.S. Michigan 1952; M.S. 1953. Economies of scale in the generation of electricity by steam. *Columbia*.
- CHARLES MEYER, B.A. Illinois 1954; M.A. 1955. The cost function for the telephone industry: increasing or decreasing? *Johns Hopkins*.
- RONALD E. MILLER, B.A. Harvard 1955; M.A. Washington 1957. The efficiency of the domestic air passenger transportation industry. *Princeton*.
- NORMAN P. MONSON, B.S. Utah 1954; M.S. Columbia 1955. Incipient monopoly in industries competitively organized: lessons from U.S. vs. Brown shoe. *Columbia*.
- THOMAS G. MOORE, B.A. Washington (St. Louis) 1957; M.A. Chicago 1959. Restrictions on entry and regulation of prices in the state of Illinois and the city of Chicago. *Chicago*.
- RUSSELL C. PARKER, B.A. Washington State. Vertical integration in the retail food industry. *Wisconsin*.
- WARREN PILLSBURY, B.A. New Hampshire 1953; M.S. Florida State 1958. The location of public highways: an economic analysis. *Virginia*.
- RICHARD L. POLLOCK, B.A. Washington 1955; M.A. 1956. Profits in the defense industry. *Wisconsin*.
- GEORGE D. QUITIN, B.A. Alberta 1952; M.A. 1958. The regulation of field prices for natural gas under the Natural Gas Act. *Princeton*.
- DONALD RICHMAN (earlier degrees not supplied). The small business administration. *Pennsylvania*.
- PHILIP ROBBINS (earlier degrees not supplied). An analysis of economic problems of big business as reflected in and related to the writings of Ida M. Tarbell. *Pittsburgh*.
- WILLIAM A. SANDRIDGE, B.A. Richmond 1943; M.A. Virginia 1956. The effects of fair trade on prices in Baltimore, Washington, and Richmond. *Virginia*.
- WADE P. SEWELL, B.S. Illinois 1947; M.S. 1949. A stochastic production function of aircraft operation. *Chicago*.
- ALBERT J. SIMONE, B.A. Tufts 1957. An economic analysis of public policy in the aluminum industry. *Massachusetts Inst. Technology*.
- DONALD SOLAR, B.A. Wisconsin 1951. Federal Air Lines. *Columbia*.

GEORGE T. WEINER, B.A. Grinnell 1957. Piggyback and the Interstate Commerce Commission. *Massachusetts Inst. Technology*.

CLINTON H. WHITEHURST, B.S. Florida State 1957; M.A. 1958. The case for competition in American ocean shipping: a critique of government subsidy policy. *Virginia*.

Land Economics; Agricultural Economics; Economic Geography; Housing *Degrees Conferred*

RENE BENALCAZAR, Ph.D. Wisconsin 1952. Toward a program for agricultural development in Ecuador.

MAX R. BLOOM, Ph.D. American 1959. Economic criteria and the use of land in subsidized urban redevelopment areas.

THEODORE BOREK, Ph.D. Pittsburgh 1959. Factors contributing to the growth and development of the arid southwest, with particular reference to the Phoenix metropolitan area.

HENRY BRAMER, Ph.D. Pittsburgh 1960. An analysis of the economic aspects of the water pollution abatement case in the Ohio River valley.

WILLIAM H. CONKLE, Ph.D. Cornell 1960. The status of and trends in produce prepackaging in the northeast.

GILBERT DEMENTIS, Ph.D. Chicago 1960. Resource allocation in the lower Rio Grande River valley (1940-1956).

JOSEPH R. EWERS, D.B.A. Indiana 1959. Federal housing programs, 1960-70.

B. DELWORTH GARDNER, Ph.D. Chicago 1960. Misallocation of resources on federal range lands.

GEORGE R. HALL, Ph.D. Harvard 1960. The lumber industry and forest policy; a study in the economics of natural resources.

CURTIS C. HARRIS, JR., Ph.D. Harvard 1960. Direct production payments in agriculture.

JOHN R. HILDEBRAND, Ph.D. Chicago 1959. Geographical differentials in the earning power of input categories on Kansas farms.

PAUL H. HOEPNER, Ph.D. Minnesota 1960. An economic analysis of risk and uncertainty in dairy and hog production.

ANDREAS A. HOLMSEN, Ph.D. Cornell 1960. Variability in income and in factors affecting income on commercial dairy farms in the north country and central plain regions of New York state.

HARRISON HSIA, Ph.D. Wisconsin 1960. Economic decision-making in hog feeding—a new approach.

PHIMOL JITEMANA, Ph.D. Wisconsin 1960. Agriculture in a developing economy—a mid-century appraisal of Thailand's agriculture.

PAUL R. JOHNSON, Ph.D. Chicago 1959. Land substitutes and changes in corn yields.

ARMAND L. LACASSE, Ph.D. Cornell 1959. Costs and efficiency in the operation of milk manufacturing plants in the New York-New Jersey milkshed.

ROBERT E. LAUBIS, Ph.D. Ohio State 1959. An analysis of the financial structure of agricultural cooperative business organizations in Ohio and suggestions for improvement.

JERRY M. LAW, Ph.D. Minnesota 1959. The development of a classification of market structures for agriculture.

GREGOR LAZARCIK, Ph.D. Columbia 1960. Production and productivity in Czechoslovak agriculture: 1934-38, 1946-59.

QUENTIN W. LINDSEY, Ph.D. Harvard 1960. The problem of periodic reorganization in agriculture.

ALLYN O. LOCKNER, Ph.D. Colorado 1950. A proposal for the taxation of molybdenum, uranium and vanadium mining in Colorado.

IRA S. LOWRY, Ph.D. California (Berkeley) 1959. Residential location in urban areas.

ALAN R. PLOTNICK, Ph.D. Pennsylvania 1960. Economic and commercial policy aspects of marketing western Canadian petroleum in Canada and the United States.

- RONALD H. POLLOCK, Ph.D. Ohio State 1959. An analysis of changes in consumer milk purchases in two Ohio metropolitan areas.
- STANLEY R. SCHULTZ, Ph.D. Ohio State 1960. An economic analysis of manufacturing milk production in Ohio.
- RAM R. SINGH, Ph.D. Ohio State 1959. Reorganization of contiguous small farms for the maximum economic returns.
- FOUAD TAWFIK, Ph.D. Texas 1959. Development and influence of the United States raw cotton policy on its economy.
- GUSTAVO A. TEJADA, Ph.D. Ohio State 1959. Comparative returns to resources used on different types and classes of farms by major types of farming areas in Ohio and neighboring areas.
- SISTER MARY E. THOMAS, B.V.M., Ph.D. Notre Dame 1960. A study of the causes and consequences of the economic status of migratory farm workers in Illinois, Indiana, Michigan, and Wisconsin from 1940-1958.
- EDWIN R. WESTCOTT, Ph.D. Ohio State 1960. Optimum combinations of resources for dairy farms in west-central Ohio.
- JOHN R. WILLIAMS, Ph.D. Columbia 1960. The premium price plan for copper, lead, and zinc.

Theses in Preparation

- MARTIN E. ABEL, B.S. Cornell 1956. Programs for expanding the demand for farm food products in the United States. *Minnesota*.
- DAVID J. ALLEE, B.S. Cornell 1953; M.S. 1954. Institutional arrangements for irrigation in New York. *Cornell*.
- DAVID L. ARMSTRONG, B.S. Ohio State 1957; M.S. 1958. An analysis and comparison of the economics of forage management systems. *Ohio State*.
- ABDEL-MAWLA M. BASHEER, B.S. Alexandria 1951; M.S. Cornell 1958. The land settlement approach to economic development—a comparative study. *Wisconsin*.
- H. H. F. BINHAMMER, B.A. Western Ontario 1948; M.A. Queen's 1957. Study of the Canadian housing sector. *McGill*.
- OSWALD P. BLAICH, B.S. Manitoba 1946; M.S. Minnesota 1955. The theory of vertical integration with an application to the feed processor-hog producer-meat packer complex. *Minnesota*.
- CARROLL G. BRUNTHAVER, B.S. Ohio State 1954; M.S. 1958. An economic analysis of live and carcass lamb marketing structure in Ohio. *Ohio State*.
- GIOVANNI CODA-NUNZIANTE, Dr. in Agri. Sc. Naples 1952. International trade in lemons and lemon products. *California (Berkeley)*.
- DAVID G. DAVIES. Agriculture response to urban industrial development. *North Carolina*.
- MARCEL F. E. DE BACKER, B.A. Royal Lycée (Antwerp) 1942. An economic appraisal of forest-policy structures in the mountainous savanna regions of eastern Central Africa. *California (Berkeley)*.
- VINCENT F. DUNFEY, B.A. Boston College 1937; M.A. 1947. An analysis of the demand for haddock. *Boston College*.
- MARY P. DURBIN, B.S. Reading (England) 1955; M.S. California (Davis) 1957. The economics of water importation, with special reference to problems of drainage. *California (Berkeley)*.
- SALOMON R. ECKSTEIN, Lic. en Economie, Mexican 1957. Collective farming in Mexico. *Harvard*.
- CARL K. EICHER, B.S. Michigan State 1952; M.S. 1956. Economic development policy and planning for American Indian rehabilitation programs. *Harvard*.
- AUSTIN B. EZZELL, B.S. Alabama Polytechnic Inst. 1948; M.S. Michigan State 1956. Some economic impacts of selected types of legislation on food distribution. *Ohio State*.

- HOMER FAVOR (earlier degrees not supplied). The effects of racial changes in occupancy patterns upon property values in Baltimore. *Pittsburgh*.
- LEHMAN B. FLETCHER, B.S. Florida 1954. Economic organization and growth in the Los Angeles milkshed. *California (Berkeley)*.
- HELMUT J. FRANK, B.S. Columbia 1948; M.A. 1950. The pricing of Middle East oil. *Columbia*.
- MARTIN J. GERRA, B.S. Georgetown 1949; M.A. Catholic 1954. Microeconomic forecasting—a case study on the price of eggs. *Johns Hopkins*.
- PETER G. HELMBERGER, B.S. Minnesota 1955; M.S. 1957. The economic impact of bargaining cooperatives in the production and distribution of fruits and vegetables. *California (Berkeley)*.
- STEPHEN J. HIEMSTRA, B.S. Iowa (Ames) 1953; M.S. 1957. Structural changes in California food retailing. *California (Berkeley)*.
- JAMES G. HILTON, B.S. North Carolina State 1954. An application of inventory theory to farm equipment repair parts. *Iowa (Ames)*.
- IRVING E. JOHNSON, D.I.C.T.A. Imperial College of Trop. Agri. (B.W.I.) 1942; M.S. Cornell 1950. Land tenure in Jamaica. *Cornell*.
- EDITH M. JONES, B.Sc. London 1940; M.S. California 1959. An economic analysis of agricultural market controls under government regulation—a comparative international survey. *California (Berkeley)*.
- JOHN F. KAIN, B.A. Bowling Green State 1957. A theory of residential location. *California (Berkeley)*.
- ROBERT KARG (earlier degrees not supplied). Theory and practice of the firm applied to the crude petroleum industry. *Pittsburgh*.
- DONALD G. LARSON, B.S. Iowa (Ames) 1956. Analysis of enumerator variance in agricultural censuses. *California (Berkeley)*.
- TONG H. LEE, B.S. Chosun Christian 1955; B.A. North Eastern Missouri 1956; M.A. Wisconsin 1958. A decision unit model of housing expenditure in the United States. *Wisconsin*.
- CHARLES Y. LIU, B.A. National Taiwan 1954; M.S. Montana State 1957. Short-term distribution of meat production by regions. *Iowa (Ames)*.
- WILLEAM E. MARTIN, B.S. California (Davis) 1954. An interindustry analysis of California emphasizing agriculture. *California (Berkeley)*.
- LUIS A. MEJIA-MATTEI, B.A. Puerto Rico 1949; M.S. Wisconsin 1958. Integration in agricultural marketing with special emphasis on cooperative marketing. *Wisconsin*.
- ELMER L. MENZIE, B.S. British Columbia 1952; M.S. 1955. Section 22 of the Agricultural Adjustment Act of 1933: a study in public policy formation and administration. *California (Berkeley)*.
- GLENN H. MILLER, JR., B.A. Kansas 1952; M.A. 1954. American agriculture in the 19th century. *Harvard*.
- DON B. MILLIKEN, B.S. Purdue 1952; M.S. 1953. Enhancing the contribution of accounting information to the effective management of fluid milk plans. *Purdue*.
- WILLIAM C. MOTES, B.S. Kansas State 1954; M.S. 1958. Effects of transportation costs on the location of the meat packing industry. *Iowa (Ames)*.
- RALPH E. NELSON, B.S. Minnesota 1949; M.S. 1952. The nature of competition among dairy processing plants in South Dakota. *Minnesota*.
- HUGH O. NOURSE, B.A. Washington (St. Louis) 1955; M.A. Chicago 1959. The effect of public housing on property values in St. Louis. *Chicago*.
- JOSE A. OLIVIERI-RODRIGUEZ, B.S. Puerto Rico 1950; M.S. Wisconsin 1955. An evaluation and analysis of research in agricultural economics in the north central region of the U.S. *Wisconsin*.

- MANCUR L. OLSON, B.C. North Dakota Agri. 1954; B.A. Oxford 1956. The American Farm Bureau Federation: an illustration of a concept of collective action. *Harvard*.
- SURYAMANI PATHAK, B.Sc. State College of Agri. (India) 1951; M.Sc. 1953. Relationship between various input-output factors in agriculture, western U. P. India. *Cornell*.
- RUTH RASCH, B.A. Bryn Mawr 1957. Effects of monetary and public lending policies on residential construction. *Johns Hopkins*.
- ROBERT E. RIECK, B.S. Wisconsin 1950; M.S. 1957. An empirical measure of decision-making ability among Wisconsin farmers. *Wisconsin*.
- R. STEPHEN RODD, B.A. British Columbia 1950; M.Sc. London School of Econ. The economic structure of metropolitan agglomeration. *Massachusetts Inst. Technology*.
- ORLANDO J. SACAY, B.S. Philippines 1954; M.S. Cornell 1956. An analysis of the crop loan operation of the agricultural credit and cooperative financing administration. *Cornell*.
- WILLIAM N. SCHALLER, B.A. Princeton 1951. An aggregative programming analysis of regional production response. *California (Berkeley)*.
- E. C. SIEVWRIGHT, B.Sc. London 1950; M.A. McGill. Impact of the oil industry on the economy of Alberta. *McGill*.
- ABDUL H. H. TAHER, B.Comm. Ain-Shams 1955. Allocation of costs between crude oil and natural gas. *California (Berkeley)*.
- RICHARD W. TRESTRAIL, B.S. Minnesota 1950. Valuation of forests in the U.S. for taxation purposes. *Washington*.
- HENRY A. WADSWORTH, JR., B.S. Cornell 1956; M.S. 1959. Economics of large dairy farms in New York State. *Cornell*.
- JAMES C. WILLIAMSON, JR., B.S. North Carolina State 1943; M.S. 1950. Grade price relations in flue-cured tobacco. *Chicago*.
- ELMER E. ZANK, B.S. Wisconsin 1954; M.S. 1956. Structure of the grain marketing industry. *Wisconsin*.
- PINHAS ZUSMAN, M.S. Hebrew (Israel) 1956. Econometric analysis of California early potato market. *California (Berkeley)*.

Labor Economics

Degrees Conferred

- ABRAHAM H. BELITSKY, Ph.D. Harvard 1960. Hiring problems in the building trades with special reference to the Boston area.
- ELLIOT J. BERG, Ph.D. Harvard 1960. Recruitment of a labor force in sub-Saharan Africa.
- EDWIN W. BISHOP, Ph.D. Wisconsin 1959. The Guatemalan labor movement, 1944-1959.
- DONALD T. BUTLER, Ph.D. Wisconsin 1959. Trade union organizing in Iowa, 1945 to 1956.
- JOHN H. G. CRISP, Ph.D. Mass. Inst. of Tech. 1960. Collective bargaining in the public service: a study of union-management relations in Ontario Hydro and TVA.
- J. KENNETH DAVIES, Ph.D. Southern California 1959. A study of the labor philosophy developed with the Church of Jesus Christ of Latter Day Saints.
- WILLIAM S. DEVINO, Ph.D. Michigan State 1960. Unemployment compensation exhautees during a recession.
- ABDUL S. N. EL-SHAHIN, Ph.D. Southern California 1960. Minimizing Iraq's disguised unemployment through industrial development.
- EDMOND L. ESCOLAS, Ph.D. Clark 1960. A study of Wyoming's workmen's compensation system, 1915-56.
- ROBERT EVANS, JR., Ph.D. Chicago 1959. The economics of American Negro slavery, 1830-1860.
- THOMAS A. FINEGAN, Ph.D. Chicago 1960. Hours of work in the United States—a cross sectional analysis.

- HOWARD M. GITELMAN, Ph.D. Wisconsin 1960. Attempts to unify the American labor movement, 1865-1900.
- ARANKA KOVACS, Ph.D. Bryn Mawr 1960. The theory of wages in the Ricardian environment.
- ARTHUR M. KRUGER, Ph.D. Mass. Inst. of Tech 1959. Labour organization and collective bargaining in the Canadian basic steel industry.
- BEVARS D. MABRY, Ph.D. Tulane 1959. An analysis of state labor laws regulating trade union activities.
- GARTH L. MANGUM, Ph.D. Harvard 1960. Wildcat strikes and union pressure tactics in American industry: a case and general study.
- HAZEL MCCALLEY, Ph.D. Pennsylvania 1959. Collective bargaining among professional workers employed by selected nonprofit organizations.
- CHARLES R. MILTON, Ph.D. North Carolina 1960. The development of philosophies of personnel administration.
- MARTIN P. OETTINGER, Ph.D. Harvard 1960. The industrial relations system in the Netherlands with special emphasis on wage policy since the end of World War II.
- THOMAS J. REYNOLDS, Ph.D. Columbia 1960. Factory employment in New Jersey 1899-1956: growth and structural change.
- SUMNER M. ROSEN, Ph.D. Mass. Inst. of Tech. 1959. Labor in Turkey's economic development.
- KARL RUPPENTHAL, Ph.D. Stanford 1959. Revolution in the Air Line Pilots Association.
- HENRY SANBORN, Ph.D. Chicago 1960. Income differences between men and women in the United States.
- GEORGE S. SAUNDERS, Ph.D. Wisconsin 1959. The movement of union and nonunion wage rates in the Ontario iron and steel products industries, 1946-1954.
- KENNETH T. STRAND, Ph.D. Wisconsin 1959. Jurisdictional disputes among the building trades unions.
- S. HERBERT UNTERBERGER, Ph.D. Pennsylvania 1960. The evolution of wage incentive systems.
- WARREN C. WATERHOUSE, Ph.D. Northwestern 1960. Causal factors underlying the relationship between wage rates and job content in the industrial firm.

Theses in Preparation

- PEL-YANG CHANG, Soochow 1940; LL.M. New York (Law) 1953. Picketing. *New York*.
- ALBERT CLARK (earlier degrees not supplied). An analysis of the development and operations of the employee retirement system operated by the city of Philadelphia. *Pennsylvania*.
- ROSANNE COLE, B.A. Miami 1955. Discrimination as an explanation of unemployment differentials. *Columbia*.
- MARY O. CONLON, B.A. Queen's 1954; M.A. 1956. Secular and cyclical movements of skilled/unskilled wages in Canada. *Columbia*.
- ANDREW J. COOPER III, B.S. Georgia Inst. Technology 1953; M.S. 1954; M.A. Princeton 1959. Air Line Pilots Association. *Princeton*.
- N. F. DAVIS, B.S. Lincoln 1949; M.B.A. Washington (St. Louis) 1952. Trade unions' practices and the Negro worker—the establishment of AFL-CIO antidiscrimination policy. *Indiana*.
- JULIA DE VINCENTI, M.S. New York 1940. Developmental history of organized labor in Puerto Rico. *Cornell*.
- DORIS DRURY, B.A. Western Ontario 1949; M.A. Indiana 1950. The concept of bargaining in the theory of unions. *Indiana*.

- J. D. DUNN, B.A. Texas 1950; M.B.A. 1955. White-collar unionization in the deep South. *Alabama*.
- ELDON J. DVORAK, B.S. South Dakota State 1953. Effects of changes in the labor-force structure upon labor-management relations. *Washington*.
- G. E. EATON, B.A. McGill 1957. Developments of trade unions in Jamaica. *McGill*.
- JOHN FERGUSON, B.A. Stanford 1933; M.B.A. 1935. Job satisfaction and productivity in a university faculty. *Cornell*.
- GERALD E. FITZGERALD, B.A. Brooklyn 1945; M.P.A. City (New York) 1956. Current practices in wage administration and position classification administration in selected American jurisdictions. *New York*.
- HARRY J. GILMAN, M.A. Chicago 1958. The role of discrimination in unemployment. *Chicago*.
- CHARLES P. HALL (earlier degrees not supplied). Analysis of special compensation paid to salaried employees of life insurance companies. *Pennsylvania*.
- ROBERT I. HISLOP, LL.B. St. Lawrence 1938; LL.M. 1939. The international labor organization. *Colorado*.
- MARY D. HOUSKA, B.S. Simmons 1954. The substitution of engineers for men with less technical education. *Massachusetts Inst. Technology*.
- MAXIMILIAN B. JONES, M.S. North Carolina 1955. Wage structure theory and its implications for job evaluation. *North Carolina*.
- ARNOLD M. KATZ, B.A. Hamilton 1952; M.A. Yale 1953. Cyclical changes in family labor force participation. *Yale*.
- JAY B. KENNEDY, B.A. Indiana 1954; M.A. 1957. Protective labor legislation in Indiana. *Indiana*.
- JOSEPH J. MELONE (earlier degrees not supplied). Impact of collective bargaining on private pension plans. *Pennsylvania*.
- MURRAY B. NESBITT, B.A. New York 1948; LL.B. 1950. Administration and UE process in the National Labor Relations Board. *New York*.
- JOHN C. O'BRIEN, B.Comm. London 1952; M.A. Notre Dame 1959. An analysis of standards in the administration of welfare and pension plans. *Notre Dame*.
- LEONARD A. RAPPING, B.A. California (Los Angeles) 1956; M.A. Chicago 1959. The impact of unionism and government subsidies on the relative wages of seamen. *Chicago*.
- RAY C. ROBERTS, M.S. North Carolina 1957. Collective bargaining in the trucking industry. *North Carolina*.
- RICHARD L. ROWAN. A study of recent labor reform bills. *North Carolina*.
- KARL SAUBER, B.S. Kent State 1956. An economic analysis of the 1959 steel strike. *Illinois*.
- WARREN P. SAUNDERS, JR., B.A. Penn State 1956; M.A. Illinois 1957. The political dimension of labor-management relations in Massachusetts: its economic impact. *Massachusetts Inst. Technology*.
- HAROLD SIMMONS, B.A. Western Michigan 1946; M.A. Michigan State 1953. The UAW-AFL: a study of an AFL enclave in the auto industry, 1939-1960. *Michigan State*.
- NAM WON SUH, B.S. Seoul 1954; M.S. San Francisco State 1958. An analysis of organized labor movements: the concept of limited-purpose organization. *California (Berkeley)*.
- JOHN P. SUSKO, B.S. Pittsburgh 1932; M.A. 1933. An analysis of the impact on prices of the escalator clause in collective bargaining agreements. *Notre Dame*.
- RITA F. TAUBENFELD, B.A. New York 1946. Labor readaptation and industry rationalization: a study in factor mobility. *California (Berkeley)*.
- WILLIAM J. WASMUTH, B.S. Jefferson 1944; M.B.A. Washington (St. Louis) 1955. The administrative feasibility of flexible retirement programs in selected companies. *Indiana*.
- C. GLYN WILLIAMS, B.A. University (Wales) 1956; M.A. Manchester 1958. A comparative study of labor practices in railroading in the United States and Great Britain. *Virginia*.

FRANK WIRIG (earlier degrees not supplied). Employee benefit plans of American religious organizations. *Pennsylvania*.

HAROLD WOOL, B.S. Brooklyn 1936; M.S. New School 1939. Skilled manpower needs and resources of the armed services—trends and implications. *American*.

DONALD C. WRIGHT, B.S. Iowa 1940; M.A. 1949. The unionization of the registered nurse. *Iowa*.

Population; Welfare Programs; Standards of Living

Degrees Conferred

MARCUS ALEXIS, Ph.D. Minnesota 1959. Racial differences in consumption and automobile ownership.

MARTIN H. DAVID, Ph.D. Michigan 1960. Family composition and consumption.

HUGH W. FOLK, Ph.D. Duke 1960. The income and employment of the aged.

JACK MINKOFF, Ph.D. Columbia 1960. The Soviet social insurance system since 1921.

ELLA J. POLINSKY, Ph.D. American 1959. Some implications of the employment patterns of women under social security.

ROBERT L. ROBERTSON, JR., Ph.D. Wisconsin 1960. The economics of patient care at University of Wisconsin hospitals.

JACK R. WENTWORTH, D.B.A. Indiana 1959. An analysis of possible consumer income and expenditure patterns from 1960 through 1970 with special emphasis on housing expenditures.

RAY O. WERNER, Ph.D. Nebraska 1960. Economic aspects of major legislative proposals for federal support of primary and secondary education in the U.S., 1946-1958.

Theses in Preparation

DAVID H. CLARK, B.A. Oklahoma 1954; M.S. Wisconsin 1960. The national health bill: its history and future. *Wisconsin*.

JOHN A. DELEHANTY, B.A. Miami 1949; M.A. 1956. Financing unemployment benefits in Indiana: as assessment of past experience and outlook for the immediate future. *Indiana*.

PAUL G. DEMENY, M.A. Princeton 1959. Investment allocation and population growth. *Princeton*.

ROBERT D. EILERS (earlier degrees not supplied). Regulation of volunteer non-profit medical care plans. *Pennsylvania*.

BELTON M. FLEISHER, B.A. Stanford 1957; M.A. 1959. Development of a migration function for the Puerto Rican economy. *Stanford*.

JOSEPH GARTNER, A.A.S. L.I. Agri. & Tech. Inst. 1951; B.S. Connecticut 1954; M.S. New Hampshire 1956. An application of economic models to consumer marketing education. *Iowa (Ames)*.

LOIS S. GRAY, B.A. Park 1943; M.A. Buffalo. The net gains from migration—Puerto Rico to New York. *Columbia*.

KHONDKAR T. HOSAIN, B.A. Dacca (Pakistan) 1951; M.A. 1952. The role of demographic variables in economic development as considered in the literature of the United Nations. *Duke*.

KAROL J. KROTKI, B.A. Cambridge 1948; M.A. 1952. The demography of Sudan. *Princeton*.

ALVARO LOPEZ, C.M.E. Colombia 1949. Some problems in the theory of a stable population. *Princeton*.

- ADOLPH MARK, B.A. DePaul 1950; M.A. Michigan 1954. Some economic and social relationships between changing household technology and family labor force behavior—1920-60. *Illinois*.
- HERBERT NEIL, B.A. Michigan 1952; M.B.A. 1953. Effects of inflation upon the incomes and asset values of consumer spending units, 1949-58. *Michigan*.
- S. E. ROTENBERG (earlier degrees not supplied). Some aspects of the economics of retirement. *Toronto*.
- ELEANOR M. SNYDER, B.A. Connecticut College for Women 1936; M.A. Columbia 1938. Freedom from want. *Columbia*.
- PAUL T. THERKILDSEN, B.S. Bradley 1951; M.S. Colorado 1957. Public welfare; a positive micro institutional and normative macro institutional approach. *Colorado*.
- YASUKICHI YASUBA, B.A. Tokyo 1953; M.A. 1956. Economics of birth rates in the U.S., 1800-1960. *Johns Hopkins*.

VACANCIES AND APPLICATIONS

The Association is glad to render service to applicants who wish to make known their availability for positions in the field of economics and to administrative officers of colleges and universities and to others who are seeking to fill vacancies.

The officers of the Association take no responsibility for making a selection among the applicants or following up the results. The Secretary's Office will merely afford a central point for clearing inquiries; and the *Review* will publish in this section brief description of vacancies announced and of applications submitted (with necessary editorial changes). Since the Association has no other way of knowing whether or not this section is performing a real service, the Secretary would appreciate receiving notification of appointments made as a result of these announcements. It is optional with those submitting such announcements to publish name and address or to use a key number. Deadlines for the four issues of the *Review* are February 1, May 1, August 1, and November 1.

Communications should be addressed to: The Secretary, American Economic Association, Northwestern University, Evanston, Illinois.

Vacancies

Economist, Central Intelligence Agency: Openings in the United States government are available for qualified economists interested in economic research concerning foreign areas. Positions involve the measurement of aggregative economic performance as well as detailed research on major industries and on agriculture, transportation, communications, and international trade. Prefer applicants with an advanced degree or substantial amount of graduate work in economics. Salary scales and other benefits comparable to civil service. Starting salary depends on experience and training. A minimum of 5 years U.S. citizenship is required. For application form and interview arrangements, write to: Office of Personnel, Central Intelligence Agency, 2430 E Street, N.W., Washington 25, D.C. Please give personal data and résumé of education and experience.

Business administration: A four-year, coeducational college, located in a metropolitan center in the Southwest is searching for a man to serve as head of its department of business administration. A Ph.D. degree in economics or business administration is required. Salary at attractive level. P229

International Cooperation Administration: This Administration has several openings at the moment and anticipates additional vacancies from time to time for well-qualified economists interested in positions overseas. The positions are of two types. Some are for advisors in special areas, such as finance, taxation, and industry. Others are for generalists to make continuing analyses of a country's economy to help guide the planning and execution of the foreign aid program for the country. Both types of positions require economists with sound theoretical background and experience in research or applied practice. The minimum tour is for two years at the foreign post, but employees who make good are encouraged to make a career of the service. Candidates must have been citizens of the United States for at least five years. The candidate and all dependents who will reside at the foreign post must pass complete physical examinations. Preferred ages are between 32 and 55. Base salaries for the vacancies that most frequently arise range from about \$8,000 to \$14,000. There are also a number of fringe benefits, including housing, medical care, educational allowances for children, differentials ranging to 25 per cent for some hardship posts, and home leave between tours. If interested—whether or not immediately available—send a résumé or preferably the standard U. S. Government Employment Application Form (Form 57) to: Office of Personnel, International Cooperation Administration, Box ER-2, Washington 25, D.C. Applications will be held confidential.

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SIMULATION OF ECONOMIC SYSTEMS

By GUY H. ORCUTT*

Simulation is a general approach to the study and use of models. As such it furnishes an alternative approach to that offered by conventional mathematical techniques. In using conventional mathematical techniques to solve a model the objective is to determine, deductively and with generality, the way in which the model implicitly relates endogenous variables to initial conditions, parameters, and time paths of exogenous variables.¹

Simulation techniques also are used to solve models, but in any single simulation run the solution obtained is highly specific. Given completely specified initial conditions, parameters, and exogenous variables, a single simulation run yields only a single set of time paths of the endogenous variables. To determine how the behavior of the endogenous variables is more generally dependent on initial conditions, parameters, and exogenous variables may require a very large number of simulation runs; and even then induction from specific results to general solutions will be required.

An individual simulation run may be thought of as an experiment performed upon a model. A given experiment involves operating a model after first completely specifying a set of initial conditions appropriate to the model, a set of values of the parameters used in specifying relations contained in the model, and the time paths of those variables used in the model and treated as exogenous. Additional experiments would involve operating the model after respecifying the initial conditions, the parameters, and/or the exogenous variables. The problem of inferring general relationships from specific results obtained in

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¹For purposes of this paper variables generated by a model will be regarded as endogenous, while variables treated as given and fed into a model will be regarded as exogenous. No distinction is made in this paper between constants and parameters.

individual experiments performed on a model is the same as that of inferring general relationships from specific experimental results in any of the inductive sciences. The scientist studying natural phenomena has no alternative. The research worker, studying or using a model, could conceivably use a purely deductive approach, but this alternative may not be attractive or it may not prove feasible with known mathematical methods.

In practice, the word simulation is used in a variety of related but somewhat different senses. For example, if someone refers to the simulation of an economy, or of some other set of real phenomena, he probably is referring to the combined activity of building a model of that set of phenomena and also of studying and/or using the model by means of the kind of simulation approach defined above. While building a model it is, of course, important to give some thought to its eventual solution and use. However, any particular model might be expressed in any one of various languages and might be solved and used by means of more than one approach. This being the case, it does not seem overly useful for most purposes to classify models either according to the intended techniques of solution alone or even simply according to the language in which they happen to be expressed.²

Not only is the term simulation used in different senses, but also a number of other words are used to identify activities or methods which might properly be regarded as specialized uses of a simulation approach. Both gaming and Monte Carlo methods are cases in point. The former may be characterized by the fact that contestants are used as components in models studied by simulation techniques. The latter may be characterized by the fact that probability models are studied by simulation techniques.

Various uses of simulation are described in Section I of this paper. Models and model building are discussed in Section II, and Section III presents various means by which complex, large-scale systems, such as economies, can be simulated.³ Section IV describes a concrete example of simulation of a demographic model of the United States.

I. *Uses of Simulation*

Pre-1950 Uses. Simulation is an ancient approach to the study and use of models and probably antedates recorded history. However, its

² Anyone who has read the excellent article [6] by Cohen and Cyert will recognize that this paragraph takes some issue with the manner in which they classify models. Nevertheless, at a more substantive level the author of this paper is in nearly complete agreement with the views they express about the implications of modern computers for model building.

³ The literature on simulation and related subjects is extensive. A bibliography of this literature has been prepared by Martin Shubik [17].

use by economists or by other social scientists has been quite limited. Early uses of a simulation approach primarily involved experimenting with models given a physical embodiment. Perhaps the limited use of simulation techniques by early economists may be accounted for on the basis that appropriate models of social phenomena are not easily given simple physical embodiments.

Irving Fisher's celebrated hydraulic model [8] furnishes one example of an early use of simulation as a device for teaching and assisting understanding. At a conceptual level, at least, Wicksell's rocking-horse analogy [23] and Aftalion's furnace analogy [2] are two other similar examples.

Yule in his classic work on spurious correlation between time series [25] used a simulation approach to determine the features of a sampling distribution of correlations between generated time series. In doing this he was using simulation techniques in a way that would now be referred to as a Monte Carlo approach. Slutsky's use of simulation techniques in his highly suggestive study on the quasicyclical behavior of economic time series [18] furnishes a somewhat similar example. Orcutt, James, and Cochrane [5] [14] [15] also made extensive use of this approach to determine sampling distributions of estimators of parameters in models containing autocorrelated time series, autocorrelated errors, and simultaneous equations.

Edward Chamberlin's classroom simulation of a market [3] is an example of use of simulation techniques in an application that now, perhaps, would be referred to as gaming.

Use in Training. The use of simulation in training drivers, pilots, and rocket passengers is well known. War games and business games also are used as training devices. In each case the trainee is able to get some feel of what he would experience in the real situation and some indication of the likely outcome of various actions and responses on his part. In each of these uses the value lies in being able to carry out training in a less expensive or less dangerous way. Perhaps economic policy-makers also should first try their skill on a model of the economy.

Design. Experimentation with models of aircraft in a wind tunnel illustrates the case in which simulation may be useful in choosing between alternative designs. In this case, experimentation on a given model may consist of varying the air stream impinging on the model; introduction of turbulence into the air stream; operation of the rudder and other controls on the model; and so forth. The behavior of the model airplane with respect to drag, stability, and response are observed. It is hoped that the model responds to the experimentation in the same way that the plane modeled would respond. If such is the

case, then this sort of simulation indicates how various designs would work in practice. Such simulation doesn't eliminate the need for test pilots and experimental aircraft, but it does reduce the number of false tries with real aircraft and pilots.

System Control. Models of electric power systems are sometimes used for similar purposes. Alternative policies, with regard to where power is to be generated at various times, can be tried out on a model. The effect of such policies on the model are then observed. Differences in expected costs, as between producing power at one generating station or another or with some combination, might be computed and this information might be used as a guide in choosing between alternative policies in dealing with a real electrical system. The value of such simulation studies lies partly in the fact that it is considerably cheaper to experiment on a model than on a real electric power system. Also, such simulation may be of value in indicating that certain stabilization policies would wreck part of the system while other policies would successfully stabilize it. It is, of course, more satisfactory to find this out while experimenting with a model than when experimenting with the real thing.

If it is desirable to carry out simulation studies relating to the operations of an electric generating system, it is even more desirable first to employ a model to try out alternative policies aimed at modification, stabilization, or growth of an economy. Governmental policies should be chosen on the basis of their fruit, but it would be more prudent to sample their fruit before trying them out on a real economy. Experimenting on a model of an economy is a way of doing this.

A model which embodies the essential features of an economic system, whether developed in terms of micro-components or developed in terms of aggregative components, could be used for a variety of policy-making purposes. If government policy-makers are considering alternative tax laws, alternative open-market policies, alternative spending policies, or alternative stabilization policies, they ought to know what kind of consequences would follow from each of these policies. Thus, if an adequate representation of the economic system were available, alternative tax policies could be tried out upon it. By using such a model to generate the behavior that would follow from each tax policy, a base would be provided for inferences concerning the yields and relative effects of applying each of these different policies to the real economy. If the model of the economic system used were sufficiently accurate, this would be of enormous value. Obviously, however, if the representation of the way in which the components of the real economy respond and interact were incorrect, such simulation studies

would in all probability yield incorrect predictions about the consequences of the tax laws under consideration.

Forecasting. In the above example predictions were being made about the consequences of alternative policies. In forecasting, unconditional predictions of what is going to happen next month, or six months from now, or next year, are sought. For purposes of policy formulation, the objective is to discover what kind of a situation to prepare for. Is there likely to be more unemployment or less unemployment six months from now? Are prices going to continue to rise or fall? A model could be used for this purpose by specifying the appropriate initial conditions and the values of exogenous variables, running the model, and observing the values it predicts for the relevant output variables.

II. *Models and Model Building*

A model of something is a representation of it designed to incorporate those features deemed to be significant for one or more specific purposes. In some cases such features are directly observable. Thus maps are models of geographic areas and they incorporate spatial, topological features of the areas represented. In other cases models incorporate more subtle features such as how the thing modeled responds to stimuli or otherwise behaves. Models of atoms, of airplanes used in wind tunnels, of rocketship cockpits and controls, and of electric generating and distributing systems are examples of this second type. For most purposes, it is unimportant whether models superficially look like the thing modeled. For example, no one cares if a model of an electric generating and distributing system looks like a sized-down physical replica. What is important is that the responses of the model to changes in load, failure of components, and so on, be similar to those of the real system. Whether a model is a physical representation, or expressed in ordinary prose, or described by pictorial geometry, or set forth in the language of formal mathematics, or presented as a computer program is, again, mainly a matter of feasibility and convenience. Some concepts may be described and worked with more easily in one language than in another and some languages may be comprehensible to different or to wider audiences than others. Simulation studies often involve extensive computations and these may call for use of high-speed computers. If it is known in advance that a model is to be used in simulation studies, it should if possible be developed in terms of a computer language, thereby avoiding a later need to encounter the difficult task of translation. However, there are times when, to facilitate taking advantage of available multivariate statistical techniques used in estimation and testing, it may be more convenient first to develop the model to be

simulated in the language of formal mathematics. Then the model may be translated into computer language to take advantage of high-speed computers in carrying out the simulation studies.

Elements of Models. Components, variables, and relations are the elements of models of economic systems, and the alternative choices open to model-builders have to do with these elements.

Major types of components used in models of economic systems include major sectors such as the household, business, and government sectors; major categories of goods such as consumer durables; industries; and micro-units such as individuals, families, firms, plants, specific markets, and specific goods. Examples of models using major sectors as components would include those constructed by Tinbergen [21], Colin Clark [4], Klein [9] [10] and Klein and Goldberger [11], Duesenberry, Eckstein, and Fromm [7], and Smithies [19], to name only a few. Several of these models have been at least partially analyzed by use of simulation techniques. The study by Duesenberry, Eckstein, and Fromm and one by I. Adelman and F. Adelman [1] are two good examples of analyses based on simulation studies. Models of economic systems using industries as basic components are associated primarily with the work of Leontief [12] [13] and his former students and associates. Models of economies built in terms of micro-components are still in a formative stage and for more information about such models the reader is referred to a forthcoming book by Orcutt, Greenberger, Korb, and Rivlin [16].

Simulation techniques either have been or could be used in studying and using any of the above types of models. With the simpler aggregative national income type of model, a simulation approach may be less appropriate than a purely deductive mathematical approach. In the case of static Leontief models the use of large-scale computers is called for, but matrix inversion methods have been extensively used rather than simulation techniques. More sophisticated aggregative models, large dynamic Leontief models, and micro-analytic models are probably all more amenable to handling by simulation techniques than by any other known method. In fact it is only since the revolution in computing technology that micro-analytic models can be considered practical.

Variables in a model relate in one way or another to the components of the model. It is convenient to classify variables into three broad categories: output variables, status variables, and input variables.

An output of a component is anything which issues from or is generated by the component. Possible outputs of an individual would include entering the labor force, marriage, and death. Married couples

may have children and get divorced. Outputs of families would include expenditures and acquisition of debts and assets. Firms may hire labor, place orders, set prices, and generate various other outputs. Output variables are variables which describe outputs.

A component's status variables are variables which describe the state of the component. The values of these variables at the beginning of a time period are available for use in generating the component's behavior during the period, and the output of the component during the period may alter the values of the status variables that describe the state of the component at the start of the following period. For example, the status variables of an individual might include his age, sex, marital status, education, and previous incomes; the status variables of a firm might include its inventories, previous sales, balance sheet variables, and even anticipated sales.

A component's input variables as well as its status variables are used in generating its behavior. But whereas status variables are internal to the component and describe its state at a point of time, input variables arise outside of the component and must be fed into it. It is these variables which are used to describe the unfolding impact of the external environment on the component. Inputs may thus include the weather, time, and outputs of other components such as orders and income payments.

In addition to components, and the variables relating to the components, a model of an economy also must contain relationships if it is to generate behavior. Relationships specify how the values of different variables in the model are related to each other or how they are otherwise generated. Relationships are of two broad types: identities and operating characteristics. Identities are accounting or tautological statements which may be introduced for convenience. Thus, total sales of a product are set equal to total purchases of the product, total assets of a firm are set equal to total liabilities and equity, and so on.

An operating characteristic is a relationship specific to a given component which specifies either an hypothesis or an assumption about how output variables of the component are related to its status and input variables. One example of an operating characteristic of a family might be a demand relationship. In this case the output variable might be the monthly expenditure on clothing. Status variables that might be used in the operating characteristic generating this variable would include age of head, marital status of head, number of children, asset and debt position of the family, and previous incomes. Input variables that could be used in determining the demand for clothing of this component would include its current income, price information, and weather information.

By specifying how components in a model are to respond to input variables and to status variables, operating characteristics embody much of whatever knowledge is made use of in a model. It is primarily upon them, and the initial distribution assigned to status variables, that any predictive use of a model must depend. It also is worth observing that, whereas components and variables may be directly observed in a real economy, operating characteristics must be inferred from inductive studies.

Modern Tools. Some of the knowledge needed to specify operating characteristics appropriately may be derived from planned experimentation. However, in view of obvious difficulties, it seems clear that extensive use must be made of the innumerable unplanned "experiments" conducted by actual economies. Hypotheses must be formulated; data appropriate for testing must be collected and evaluated; hypotheses must then be modified or new hypotheses introduced; new data must be collected and so on. To facilitate this continual process of hypothesis formulation and testing, which typifies all scientific research, requires data collection instruments which are highly responsive to changing research needs. Sample surveys have become an indispensable part of this process in economic research. The rapid development and implementation of sample-survey methods complements the revolution in computer technology and has far-reaching consequences for model building.

Multivariate statistical tools of analysis are a third modern development of great importance to successful model building but will be passed over without further comment in this paper.

Role of Simulation. Simulation techniques and studies may be useful in the specification of operating characteristics, in the following ways:

1. Simulation techniques make possible the effective study of models containing large numbers of components, variables and relationships of almost any desired form. They more completely free research workers to be guided by considerations relating to adequacy of representation.

2. Simulation techniques make it feasible to carry out sensitivity analyses on a model. The model can be run many times with the value of one or more parameters being altered between runs. The resulting variations in time paths of endogenous variables can be observed, and related to the corresponding alterations of parameters. After finding out how sensitive these results are to specific differences in the size of parameters, the investigator is in a much better position to decide where to apply additional research effort in parameter estimation.

3. Simulation techniques permit specific implications of models to

be determined. By so doing they make it possible to carry out testing at various levels of aggregation, ranging from the level of individual components up to the level of highly aggregative phenomena such as national income. This is extremely important because achievement of more adequate testing is one of the most serious problems facing model builders.

4. Simulation, or closely related Monte Carlo studies, can be useful in supplementing or extending modern multivariate statistical techniques of estimating parameters in operating characteristics. In effect these techniques enable operating characteristics to be fitted to bodies of data by systematic trial-and-error procedures. This may become of greatest importance when dealing with various kinds of nonlinear relationships for which other methods of estimation are either unknown or too costly.

Simulation studies can also be used to improve our knowledge about how existing statistical techniques work in the face of specification errors of various sorts. The very interesting studies of Summers [20] illustrates this type of use.

III. *Computer Simulation of Complex, Large-Scale Systems*

Economic systems are complex organizations involving the behavior of hundreds of millions of complicated decision units and their interaction. Models involving hundreds of millions of components can be conceived of but their construction and use pose problems that appear insuperable both from the standpoint of completely specifying such models and from that of studying and using them if they could be constructed. There are, however, various means by which satisfactory computer simulation of large-scale systems can be made feasible. Many of these are discussed in detail by Orcutt, Greenberger, Korbel and Rivlin [16], but some of the more important means are as follows:

1. *Building-Block Approach.* One rather simple but essential procedure is that of using a building-block approach. Construction of a large-scale model and of a large-scale computer program requires an extended effort over time by many individuals with many skills. Extensive testing of individual pieces must be carried out before the pieces are assembled, and even after they have been assembled, it frequently may be necessary to modify some pieces. Also in finding and in eliminating the errors in a large and complex computer program it is important to be able to do it piece by piece. And even after it is assembled, it frequently may be desirable to alter a particular operating characteristic, or parameter, or the initial composition of components and their status variables. For these reasons it is highly useful to take the

individual components of a model as building blocks and construct them and the over-all model so that they are like the fully plugable components of a modern piece of electronic equipment.

2. *Use of Block-Recursive Models.* Given that a building-block approach is to be used in constructing and simulating a large-scale model on a computer, it is of considerable practical importance that the model be block-recursive, at least in its broad outlines. If it is assumed for purposes of illustration that individual families are treated as blocks, then in order for a model to be block-recursive it is necessary, for example, that a family's expenditures on clothing during a month should not have such an immediate chain of consequences as both to alter income payments to the family and also thereby alter the family's expenditure on clothing during the same month. This is thus a much less restrictive requirement than that of full recursiveness. Wold makes an impressive case for the use of recursive models [24] and all of Tinbergen's models have this property. Models of Klein, however, must be put in a reduced form before they have this property. Whether or not a real economy can be adequately represented by a block-recursive model depends upon the choice of blocks and how short time lags can be without being represented as zero time lags. Given flexibility in choice of blocks and the use of very short time lags where appropriate, it is difficult to see how the requirement of block-recursiveness places any serious limitations on the model builder. The advantage of working with models which are block-recursive is that digital computers now available all perform their operations in a sequential fashion. This doesn't prevent solving sets of reasonably small numbers of linear simultaneous equations. It does make extremely difficult or impossible the handling of large numbers of nonlinear simultaneous equations. Use of block-recursive models does limit any given set of simultaneous equations to those needed in generating the output of a single block and thus greatly facilitates computer simulation with computing equipment that is or will be available within the next few years.

3. *Replication of Components.* Given a building-block approach, another major method of facilitating the design and computer simulation of large-scale complex models is to replicate components. This means that while large numbers of components may be used in a model these components are restricted to a relatively small number of major types. Components of the same major type will have identical operating characteristics but may, of course, have different series of values for their status variables and different series of values for their input variables. The advantages of this kind of replication in terms of saving labor in developing and programing a model are obvious and important. There are, however, deeper and more subtle advantages to such repli-

cation that relate to estimation and testing of the operating characteristics of the basic components. A full discussion of these is beyond the scope of this paper, but it may be pointed out that replication has essentially the same importance for learning about the behavior and response mechanism of components that it has for experimental design in general.

4. *Treatment of Model as a Probability Sample.* A fourth basic method for achieving effective computer simulation of models that, conceptually at least, involve millions of micro-components, is to regard the components used in computer simulations as probability samples of the components in more extensive conceptual models of economic systems. This can reduce the number of components actually handled in a computer simulation to something less than the hundred thousand or so that are manageable with computers now available. The results obtained from the computer models may then be appropriately blown up in the same way that results obtained by sample surveys are blown up to yield figures appropriate for the populations sampled.

IV. *Computer Simulation of a Demographic Model—An Example*

As one step in demonstrating the feasibility and potential usefulness of simulation techniques in connection with the development and use of models of economies built in terms of micro-components, Orcutt, Greenberger, Korbel, and Rivlin [16] constructed and carried out computer simulations of a demographic model of the United States household sector.

The basic components of the model are individuals and combinations of individuals such as married couples and families. The family units form, grow, diminish, and dissolve as married couples have children and get divorced and individuals age, marry, and die. These outputs of the basic components in a given month depend on the status variables that characterize each component as of the beginning of the month and on the inputs into each component during the month. The operating characteristics, which serve to relate outputs of components to input and status variables, are stochastic in nature; i.e., it is the probabilities of occurrence of certain outputs rather than the outputs themselves which are regarded as functions of input and status variables. For example, the probability that a woman will give birth to a child during a given month was estimated, on the basis of available data, to depend on a nonlinear, multivariate function of her marital status, age, number of previous births, interval since previous birth or since marriage, month, and year. The model is recursive in that the probabilities determined for the possible outputs of each component in any period depend only on previously determined input and status variables.

Solution of the model was achieved by simulation on a large electronic computer. An initial population of over 10,000 individuals was made to be representative of the United States population as of April 1950 by appropriate assignment to these individuals or to groupings of these individuals of sex, race, age, marital status, parity, interval since marriage or previous birth, and family composition. Assignment of these status variables was based on information contained in tabulations of the Bureau of the Census and in various sample surveys carried out by the Survey Research Center of the University of Michigan for the Federal Reserve Board and for a family-planning study by Ronald Freedman, Pascal Whelpton and Arthur Campbell.

The simulations carried out proceeded in one-month steps. Data relating to specific individuals and families were stored on magnetic tape. Operating characteristics, summary information and variables nonspecific to micro-components were stored in high-speed core storage. In each "month" the members of each family were considered in turn. For each possible output of each component a probability of occurrence was specified by use of the relevant operating characteristic and the appropriate status and input variables. Whether the output occurred or not was then determined by a random drawing from this probability distribution. For example, if the probability that a male with a particular initial status will die during the month was calculated to be 0.0002, then, in essence, a random drawing was made from a bag containing two black balls for every 9,998 white balls. The man either died and was eliminated from the population (and from his family) or he lived through the month depending on the outcome of the draw. In practice, however, random numbers generated by the computing machine provided a less cumbersome method of making the random drawings than balls in a bag.

When each possible output for each unit had been considered in this way, the first "pass" or month was complete. The whole procedure was then repeated for the second month and as many more as desired. Each succeeding month was begun with a population of components which was slightly different both in size and composition from the preceding one, since some individuals had died or married, some couples had divorced, some babies had been born and all surviving individuals were one month older.

Several simulation runs covering part or all of the interval from April 1950 to April 1960 were carried out using operating characteristics estimated on the basis of data from available sources. Simulation runs, involving systematic variations in critical operating characteristics, also were carried out.

As each simulation run was carried out the computer was used to

summarize the micro-changes taking place and to feed out aggregative monthly information about births, deaths, marriages and divorces. At selected intervals cross-sectional tabulations of the developing populations also were produced by the computer. The aggregative time series and frequency distributions produced in this way were then blown up to provide estimates relating to the real population and where appropriate these estimates were compared with available aggregative data about the United States population and its composition.

The use of simulation techniques by the authors of this demographic study does not, of course, offer any guarantee in itself that they have produced an acceptable and useful population model. However, by producing a feasible means of solution it permitted them to introduce a variety of interactions, variables, nonlinearities and stochastic considerations into their model which they otherwise would have been forced to leave out despite strong evidence of their importance. In addition, by providing a means of solution it made possible comparison of generated results with observed time series and cross sectional data and thus permitted testing of a sort that would not otherwise have been possible.

V. Concluding Remarks

A simulation approach to the study and use of models is more easily understood and mastered by nonmathematicians than more conventional mathematical techniques. This aspect of a simulation approach has made it attractive to many research workers and might be expected to have a particular significance for social scientists as well as for business men and government officials.

From the revolution in computing technology of the last one or two decades, the simulation approach has emerged, as a practical means of studying and using more nearly realistic models of economic systems. In fact it is the only known approach to the satisfactory study and use of any of the existing dynamic models of economic systems for which any pretense of realism can be claimed. No one can say with certainty that purely deductive mathematical techniques could not be developed to do the required job. However, given the rapid development of survey research techniques and other improved means of data collection about micro-units, and given the rapid development of multivariate statistical techniques of estimating nonlinear relationships, it seems obvious that future models of economic systems will be much less amenable to conventional mathematical analysis than models previously developed. Therefore, a simulation approach to the study and use of models of economic systems has become essential and probably will continue to be for a long time.

It seems reasonably clear to this author that, during the next decade at least, simulation studies of large complex economic systems will be carried out most effectively on extremely fast digital computers. However, analogue computers have some advantages and some adherents, and the interested reader would benefit by studying the work of Tustin [22].

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SIMULATION OF THE INDUSTRY AND THE FIRM

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The high-speed digital computer¹ promises to provide the economist with means for constructing both the instruments for observation and the equipment for experimentation that have been the earmarks of the traditional sciences. Used in one way the computer supplies a viewing equipment to the economist in a manner analogous to the microscope for biologists (however, a great amount of work goes into setting up the "specimens" to be observed). Beyond its use as a viewing instrument, it provides a possibility for the construction and running of experiments. It has a use as laboratory apparatus. The various uses of the computer are not substitutes for economic analysis or observation. They are nevertheless supplements of considerable power.

Speaking broadly, there are five major areas of new development of interest to economists which depend heavily upon the advent of the computer. They are: (1) data-processing; (2) analytical methods; (3) simulation; (4) gaming; and (5) artificial intelligence. This paper deals primarily with the third of these, simulation, although there is a considerable interlinkage between the techniques in all categories; and the last two are sometimes treated as specialized types of simulation.

Data-Processing. In data-processing the important features of the new technology are the speed of operation and the ability to record, store and analyze quantities of raw data whose mere volume in most forms presents not only processing but even storage problems. The current generation of large computers performs an addition in under 5 millionths of a second.² New methods of storing information make it possible to keep a million "bits"³ on a piece of plastic an inch square. It has become possible to examine mass census data in many different

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¹ Possibly there will be a growing use for analogue computers; however, to date almost all the new applications of computer technology to economic problems have utilized digital computers.

² For instance, the IBM 7090 performs an addition in 4.8 micro-seconds.

³ A bit is the unit in which information is measured. It is the amount of information required to distinguish between two possibilities. For example, yes or no, or equivalently 1 or 0, contain one bit of information.

aggregations and disaggregations. Furthermore, the investigation of economic models which previously called for many months of labor in carrying out statistical computations such as correlation can be carried out in a matter of days, even if all of the work in setting up the material for computation is included. Only a few hours may be needed for the actual computation. "Canned programs"⁴ are already sufficiently numerous and available that technological unemployment is in sight for those wishing to write the type of master's thesis depending primarily upon tedious data-gathering and lengthy routine computations. More economic thought may be required as the data-processing times and difficulties decrease.

Analytical Methods. One of the main criticisms of economists using mathematical models has been that their models have tended to be "unrealistic." In many instances considerations of the economics involved have taxed the mathematics to such an extent that the problems have had either to be simplified in an undesirable manner or have not been mathematically tractable. The advent of the large computer is removing many of the limitations of mathematical analysis. Much of the work in the utilization of linear and dynamic programming would not be feasible without computing-machine assistance. Many problems in inventory, production or investment theory, for example, can be concisely stated but are not solvable by analytical methods. It is now possible to utilize numerical methods to obtain answers to many of these problems to any degree of accuracy.

Simulation. In the past few years the word "simulation" has begun to make its way into economic literature [24]. The word is old, especially in reference to other areas where analogue simulation has been used for some time. For example, the air frame industry has utilized physical models in wind-tunnel simulations; and for teaching purposes an hydraulic model at the London School of Economics has provided an analogue simulation of a macroeconomic system for many years. This particular model has been excellently illustrated by the English cartoonist Emmet in *Punch* of 1953.

A simulation of a system or an organism is the operation of a model or simulator which is a representation of the system or organism. The model is amenable to manipulations which would be impossible, too expensive or impracticable to perform on the entity it portrays. The operation of the model can be studied and, from it, properties concerning the behavior of the actual system or its subsystems can be inferred.

The close interrelationship between simulation, gaming and artifi-

⁴A canned program is a program written to perform a task which may be used many times in different problems; for example, a program for multiple correlation. Under such arrangements as SHARE whole libraries of programs are becoming available upon request.

cial intelligence will be considered in the next section. In the succeeding section, contemporary applications of simulation to the industry and the firm will be reviewed.

I. *Simulation and Allied Techniques*

Gaming and simulation are often classed together. It is important to make clear the distinction between these different methods designed for different purposes.

A. *Gaming.*

Gaming has been used as an experimental and training device by the military for many years [27]. Its use as an experimental tool for studying microeconomic behavior is recent. In a gaming experiment a model of an environment is provided. In other words, a simulated universe is constructed. In the military the simulated universe may be a sand table or a maneuver area; in a business game it may be a simulated oligopolistic market structure; in a small group behavior experiment it may be a specialized communications network. Players or decision-makers act within the simulated environment, and the experimenter, by observing them, may be able to test hypotheses concerning the behavior of the individuals and/or the decision system as a whole.

A distinction of interest to economists may be made between business games or "environment rich games" and more strictly controlled experimental games. For example, the recent work of Siegel and Fouraker [26] is of the latter variety. The many business games, almost all of them using a simulated structure of an oligopolistic market, provide examples of the former [14] [16].

B. *Simulation.*

In a simulation, either the behavior of a system or the behavior of individual components is taken as given. Information concerning the behavior of one or the other is inferred as a result of the simulation. Hence one distinction between gaming and simulation is that, although it is usually likely that a simulation will not include any human decision-makers (but merely the decision rules for their behavior), a gaming exercise or experiment will always include decision-makers whose behavior is to be influenced or studied.

The variety of problems explored by means of simulation has called for the development of specialized techniques. Most simulations employ only a high-speed digital or analogue computer. Those of interest to economists belong almost completely to the former type. There is, however, important work involving simulation utilizing both men and

computing machines. Furthermore, problems in physics and engineering have utilized a statistical device known as the "Monte Carlo method." There is a highly specialized technical literature on this [20] [21]. As this is not of prime concern to the economist no further discussion of the Monte Carlo method is given here.

Man-Machine Simulations. This category consists of simulations which use both men and computing machines simultaneously, but are not necessarily concerned with the problems involved in gaming. The RAND Logistics Laboratory provides an example of this type of work [11]. Here a complete logistics system is being simulated in order to study the effects of different changes in policies and initial conditions on the over-all performance of the system. Many individuals participate in the actual simulation, not primarily to be trained or experimented with as individuals, but because the cheapest effective simulator of the individual who is in control of the ordering of spare parts for an air defense system may be the individual who is in control of ordering spare parts for an air defense system.

With respect to purpose, a distinction can be made between two kinds of simulation of interest to economists, although the two categories are not altogether well defined. Suggestive names are operations research or tactical simulation and exploratory or strategic simulation. In addition, a third technique which concerns the simulation of cognitive processes deals with areas which Marvin Minsky [22] and John McCarthy have called artificial intelligence. This topic is developed by Clarkson and Simon in a companion article. A brief further reference is, however, made below in this Section.

The distinction between the first two is between simulation used as a device to compare the results of alternative decision rules within relatively well-defined structures and simulation used primarily as a device to aid in the exploration of the behavioral properties and the validity of relatively ill-defined models. This is not a totally happy distinction; however, examples may serve to clarify.

Tactical Simulation. Under this category much of the work in the general area of applied microeconomics, known as operations research, has taken place. This has included traffic scheduling [17], waiting-line problems [4] and investigations of production scheduling [15]. In these instances it has been relatively easy to describe the physical model to be simulated. The validation problem, which is at the heart of the use of simulation as an aid to exploring the relatively ill-defined models of the economic system, does not present a critical barrier to the more "tactical" operations-research utilization of simulation.

Exploratory or Strategic Simulation. This category has to date included the work of Orcutt [23] and collaborators on some aspects of

the whole economy, Hoggatt and Balderston [13], Kalman Cohen [5], Joseph Yance [28], Cyert, Feigenbaum and March [7], Jay Forrester [10], Benjamin Bryton and Martin Shubik [25], and others working at the levels of the industry or the firm and part of its immediate environment. There has also been military work, typified by that of RAND, which has already been noted, and there apparently exists a classified literature whose worth cannot be ascertained.

C. *Artificial Intelligence.*

The advent of the theory of games⁵ helped to clarify much of the vagueness obscuring the analysis of decision processes. In particular it provided a unifying mechanism with which to view the "reaction curves" and conjectured behavior assumptions which had been used by economists as an aid in the study of oligopoly. One immediate result of the game theoretic formulation of a decision process in extensive form [18] was that it highlighted the paradox concerning rational behavior and complete information; thus theories of rational behavior often assume complete information (such as detailed knowledge of preferences portrayed by an indifference map; or knowledge of many production alternatives). Even if information concerning all alternatives were available, in general no "rational" decision-maker has a long enough life to look at all of them. A theory of rational decision-making must show how the decision-maker avoids having to deal with too much information.

There are too many possibilities in even the simple game of chess to permit a complete exploration of the alternatives by the most powerful computer of the present day working full time for many millions of years. Yet many people play a relatively good game of chess without this search through all possibilities. The growth of work in learning theory [3] combined with the realization of the impossibility of total search through all the alternatives implied by a game theory model has led to the investigation of "heuristic programming" and artificial intelligence models. These, in turn, bear relation to the study of industries and oligopolistic market structures if the manifestations of the behavior of the firm are considered in terms of "game learning" processes under conditions of uncertainty.

II. *Simulation of an Industry or Firm*

The actual and potential contributions of work on the simulation of the firm can be categorized as fourfold: (1) Simulation provides a new econometric device to produce models based on empirical investi-

⁵ For a lucid exposition of the basic ideas of game theory see [18].

gation. (2) It serves as a computational aid and alternative to analysis in theory construction. (3) It may be used as a data-organizing device. (4) It may serve as a tool for anticipation and planning. The first two categories are of more interest to the theorist or academic economist, while the last two are more the concern of the economist or operations researcher in industry, government or other applied areas.

The mass of institutional detail which can be handled by a computer makes it possible to construct detailed micro-models of large systems, such as an industry. The time-series output of the simulation of these models provides alternative predictions to those derived by utilizing a set of structural equations estimated by econometric methods. Joint investigation of economic systems employing both methods offers a potential interlink between macro- and microeconomics.

Relatively casual empiricism (casual in a nonpejorative sense) has been the earmark of the work of many economic theorists. This is consistent with the use of intuition and the need for broad general searching and "scanning" prior to the construction of models whose testing may lead to new theory. Especially with conjectures and speculations concerning the behavior of the firm or a market, the work involved in tracing through the implications of a conjecture may be long and tedious or even not possible analytically. Quick small simulations enable the theorist to increase the number of casual conjectures that are worth further investigation. Simulation provides an aid to the intuition as a preliminary to doing the more extensive work of preparing for formal testing of new theory.

Possibly one of the most valuable contributions of simulation to date has been the discipline imposed by the necessity of precisely defining for the computer both the problems and questions to be answered. In spite of the current work on artificial intelligence the computer operates on most programs today in a docile and literal manner with very little exhibition of imagination. If it is fed nonsense it usually returns nonsense, or if the material is inconsistent or insufficiently defined it stops. These features force any utilizer of simulation techniques to have a well-defined problem. The discipline called for in making a program that will work is rigorous. The value of this is twofold, both in requiring the checking of models for consistency and completeness and in the designing of consistent data-organization processes.

As an applied tool for the firm the immediate value of simulation is probably more in providing a consistent data-organizing medium than in providing answers to policy questions for the next few years. Large economic organizations are, in general, not short of statistics, documents and paperwork. They are, however, invariably short of the statistics, documents or paperwork needed by the economist or other re-

searcher in the form that will make them of use within his model.

It is premature at this time to discuss in very much more than hortatory terms the power of current simulators to decide policy questions. The first stages towards this possibility, however, are well under way. They are the transformations being wrought by new methods of data processing, in general, and simulation as a data-organizing device for economic models, in particular.

The advent of the computer has extended the possibilities for experimentation and for investigating the behavior of over-all systems by means of simulation. The application of empirical and experimental methods is slow and tedious. It may take several years before enough work can be done and validated for any new, large generalizations to be obtained. Even at this time, however, the current investigations indicate the value of the method as an exploratory tool, especially in its use in approaching situations, whose complexity had hitherto made analysis intractable, if not impossible.

Technological Considerations. The computer provides a tool of a degree of sophistication and complexity that makes it necessary for the economist to consider explicitly some of the technical problems entailed in its use. It is quite probable that well within ten years a basic knowledge of computers will have joined mathematics and statistics as "language requirements" for advanced degrees in many areas of economics. A worth-while discussion of many of the technical aspects of simulation is given by R. W. Conway, B. M. Johnson and W. L. Maxwell [6].

Although this is not the occasion to go deeply into technical problems, there are several of basic interest to those wishing to have even a casual acquaintanceship with the use of simulation in specific and of computers in general. To go from a verbal description of an economic process through to a computer simulation, in most instances, may entail the use of as many as five languages. The verbal description may be translated into a mathematical model which, in turn, will give rise to flow diagrams from which a computer program will be written in a special language which, in turn, is translated by the machine into the actual machine language. It is hypothetically feasible to proceed directly from a written description to the machine language, but in practice this is generally inadvisable.

Flow diagramming and special languages, such as FORTRAN [29] are relatively easy to learn and are in themselves of considerable value in defining models accurately. They are more precise than English and more flexible than mathematical notation. One or two days is sufficient to obtain a working acquaintanceship with flow diagramming and one or two weeks with a special language.

These techniques may be the most important contribution to the

methodology of economics that the computer age has produced. They have enlarged the scope of modeling of complex systems by several orders of magnitude. Limitations of space do not permit an adequate exposition of this important point at this time.

Fundamentally, a simulation is a type of experimental investigation. A simulator can be regarded as laboratory equipment. As such, it can be assembled in many ways. Given unlimited time and money, it is no doubt true that just about anything can be simulated. However, when the economic restraints are considered attention must be paid to the costs of flexibility, for the latter is important to the economist interested in exploring different decision rules and changes in market structure. It is possible to design simulations in a modular manner; that is, so that without having to rewrite a complete program, the investigator will be able, for example, to substitute a new pricing or forecasting rule by merely rewriting a specialized subroutine. Otherwise, the change could easily cause lengthy, time-consuming and expensive revisions.

The past three or four years have seen dramatic changes in computer languages, speeds and flexibility of operations. For example, the existence of special languages has cut down the amount of time required to program any problem by a factor conservatively estimated at no less than five as compared with two or three years ago. Many of the difficulties of today probably will not exist within another three to four years.

Current Work. With respect to the firm as the central element of a simulation, current work is of three kinds: (1) simulations concerned primarily with the internal mechanisms of a firm; (2) simulations dealing with the firm and its relationship to its environment; and (3) simulations dealing with industries or aggregations of firms.

Most of the work has been done within the last two to three years and little of it has been published in finished form. Cyert and March [8] have begun to investigate the internal organization of a firm, specifically a department store, simulating the internal resource allocation and pricing behavior. The modeling problems for this type of simulation are great. Most of the other work at this level of micro-investigation has been gaming for research or teaching purposes on the small group-organization problems which underlie much of the internal structure of the firm.

Unpublished work by the writer deals primarily with a single firm and its relationship to its environment, taking into account some aspects of the forecasting, inventory and pricing behavior of the firm, its distributors and retailers. The major aims of the program are to serve as a data-organizing device by means of which information can be gathered and structured in a manner to provide models of sufficient

worth to be of value in sensitivity analyses of changes in policy, decision rules and market structure. A further goal of this particular program is to carry out an investigation of different degrees of aggregation for the same economic models and to provide data for testing the simultaneous equation estimation procedure developed by R. L. Basmann [2]. The econometric estimation processes as applied to an industry and the simulation of an industry provide alternative approaches, the relative merits of which must still be investigated.

Of the work on the firm and its environment and on models of industry as a whole perhaps the "industrial dynamics" project of J. W. Forrester [10] is the best known. The published works of this project to date, however, appear to be primarily expository and hortatory. The prime contribution appears to have been the construction of a special computer language, DYNAMO, for the writing of simulation and difference-equation models. The value of this contribution can only be judged on technical modeling, programming and computing time considerations. Depending upon the previous training of the individual, the availability of courses and computer time, a case can be made in either direction. With the exception of the work of Yance [23] none of the simulations noted in this article have utilized DYNAMO. There does not appear to have been any specifically new contribution to economic thought or methodology in the industrial dynamics project's published works.

Both Kalman J. Cohen [5] and Joseph V. Yance [28] have produced simulations of the aspects of the shoe, leather and hide industries. The choice of these industries was due to the excellent information provided by the study of Ruth P. Mack [19]. In each of these studies an attempt has been made to compare the model with the actual performance of the industries over several years.

In the Cohen model a difference-equation system is set down to represent the aggregate consumer expenditure on shoes, retailers' behavior including sales anticipations, manufacturers' behavior, tanners' behavior and hide dealers' selling prices [5]. The Yance model has been described previously in this journal [28].

Cohen [5] stresses the difference between "one-period change models" and "process models." He claims that "most econometric models have been estimated and tested in such a way that they can be considered to be one-period change models." He contrasts this with the process model in which the lagged endogenous variables, after initialization, are produced by the system rather than brought in at each successive stage at their observed values. In other words, the process model lives with any errors made in previous periods. Cohen examines both of these types of model.

The work of Hoggatt [12] and Balderston and Hoggatt [13] is of a somewhat different nature. The interest appears to be more in the investigation of the sensitivity of market models as an aid to the construction of theory. In the Hoggatt paper the concern is with the birth and death processes of firms under various conditions of supply, demand, cost, price, exit and entry conditions. As has been noted by Hoggatt, "the most striking feature of these results is the richness in detail of the several dynamic models as related to the model of comparative statistics whose equilibrium point they share" [12]. The number of cases and conditions worked out by Hoggatt would have been unfeasible without a simulation.

The joint work of Hoggatt and Balderston has been directed towards studying the effect of changes in the structure of the information system and costs of information in a simulation of the West Coast lumber industry. This work grew out of the initial investigations by Balderston [1] of communication networks in intermediate markets. The results of interest at this stage are not so much the empirical accuracy of the model of a particular lumber industry, as the effects of changes in information and other conditions which otherwise could only have been obtained by copious and tedious computation, if they could have been obtained at all. In their model supplier firms sell to wholesale intermediaries who resell to customer firms. Taken into account are information, commodity shipments, cash payment flow, and birth and death processes for firms at all levels. Messages are paid for and are sent for search, ordering or confirmation of orders. The institutional rules for trading are specified. These include the decision rules for setting price, quantity and search patterns for trading partners. The study examines the relationship between the number of messages and costs and explicitly introduces a measure of market segmentation which arises from search costs. This serves as a valuable linking of concepts of economics and sociology as well as an important attempt to further the application of economic analysis to marketing.

III. *Concluding Remarks*

The advent of computers in general and the techniques of simulation in particular open up possibilities for the growth of a new scientific institutional economics. The next few years should see a considerable change in data organization and information processes within firms. The growth of operations research and management science, much of which is applied microeconomics, is bringing this about. Further simulation studies of the type discussed here promise to provide the way to add the richness (in terms of explicit consideration of information costs, marketing variables, organizational structure and so forth)

needed to obtain adequate theories of the firm, pricing and market structure. The promise is twofold. The new methodology is beginning to offer the opportunity both to construct more complex theories and to validate them.

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SIMULATION OF INDIVIDUAL AND GROUP BEHAVIOR

By GEOFFREY P. E. CLARKSON AND HERBERT A. SIMON*

Simulation is a technique for building theories that reproduce part or all of the output of a behaving system. The system can be an aggregate of behaving units, an entire economy, or a particular unit, a human decision-maker. The output can be one aggregated element, e.g., that interest rate which clears the money market, or the whole host of thoughts, associations, and actions employed by a man while he solves a specified problem.

This paper, with its companion papers, outline some methods for using simulation to study various aspects of an economic system. In our paper, we shall place special emphasis on microeconomic simulation—especially the simulation of individual economic actors and individual firms—leaving to Orcutt and Shubik the discussion of larger units. In an earlier article, one of us has discussed at length the reasons why economists might be interested in analysis of behavior at the level of the decision-making process [23], and we shall not repeat these reasons here.

The process of simulation involves constructing a theory, or model, of a system that prescribes the system's processes. These processes can refer to macro as well as micro elements and the prescriptive detail reflects the researcher's knowledge of and interest in particular parts of the system. By carrying out the processes postulated in the theory, a hypothetical stream of behavior is generated that can be compared with the stream of behavior of the original system.

I. *Simulation and Classical Econometric Analysis*

What is new and what is "classical" in simulation can be illustrated by a simple market theory of the familiar cobweb type. Consider a dynamic model consisting of two equations:

$$\begin{aligned}(1) \quad & p_t = D(q_t) \\ (2) \quad & q_t = S(p_{t-1})\end{aligned}$$

where p_t is the price during time-period t , q_t the quantity sold during time-period t , D the demand function, and S the supply function.

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Henry Schultz dealt extensively with models like this in *The Theory and Measurement of Demand* [22]. What did he do with them? (a) He postulated that the economic relations were of this form. (b) He estimated the parameters of the functions D and S from empirical data for particular commodities (by regression methods). (c) He tested the goodness of fit of the regressions. (d) He compared the qualitative properties of the estimated parameters with certain predictions derived from utility theory.

The last point, (d), requires a word of explanation. As is well known, under certain additional assumptions, the postulate of consumer utility maximization implies that the elasticity of demand is negative. Hence, if the empirically estimated coefficient of the demand function was negative, this would be regarded as confirming the utility theory; if positive, as disconfirming. The method implicit in this analysis has been systematically developed by Samuelson [21, Ch. 9], who calls it the method of comparative statics.

The methodology of steps (a) through (c) is widely employed in econometrics. Cohen and Cyert [8] speak of classical econometrics as using "one-period change models," which they distinguish from the "process models" used in simulation. Since the difference lies not in the models themselves, but in the ways they are manipulated and tested, we prefer to speak of "one-period change analysis," which we shall distinguish from "simulation."

Now consider how simulation techniques may be applied to the simple market theory we are using for illustration:

(a) We postulate that the economic relations are of the form given by the equations. (b) We estimate the parameters of the functions D and S from empirical data for particular commodities (say, by regression methods). (c) Taking the given initial quantity, q_0 , by alternate applications of equations (1) and (2) we generate time series for p and q for the system. We compare these hypothetical time series with the actual time series.

Since the equations of the theory certainly do not hold exactly in the real world, it might be supposed that the predicted values of the time series would necessarily diverge more and more from the actual values. Thus, one-period change analysis, since it sets the estimates back on the track each period by comparing them with the actual values, might be expected to give better fits than simulation. This would probably be the case with our simple illustrative model. It need not hold for more sophisticated models—in particular, if the model itself contains "feedback" mechanisms. For example, if quantity produced in a given time-period is a function *both* of last period's price and of inventory at the beginning of the period, errors in the model's supply prediction will

tend to be self-correcting for the same reason that suppliers' actual errors are self-correcting—the errors reflect themselves in disequilibrium inventories which affect the supply for the next period. In a particular case—a model of the shoe industry—Cohen [7] has shown that annual averages of the time series yielded by simulation were generally closer to the actual series than were the series yielded by one-period change analysis.

Our purpose here, however, is not to argue for the superiority of simulation over one-period change analysis. We wish simply to explain in what respects they are the same, and in what respects different. Research over the next decade will undoubtedly teach us a great deal about their respective spheres of greatest usefulness.

II. *Some Forms of Simulation*

Simulation has come into economics through at least three routes: (1) dynamic macroeconomics, particularly business cycle theory and "cobweb" theory; (2) operations research and management science; and (3) theory of economic decision-making—including theory of the firm and oligopoly theory.

1. *Dynamic Macroeconomics.* This kind of simulation is exemplified by the models usually found in theories of the business cycle and market behavior.¹ In the past, mathematical models of the business cycle were handled by standard analytic techniques—e.g., solving the differential or difference equations of the model; applying the method of comparative statics to draw qualitative conclusions; and so on. As the models grew in complexity, and as various kinds of nonlinearities were introduced that were relatively impervious to standard analytic methods, greater use was made of numerical analysis—especially one-period change analysis—to study system behavior. In this realm, simulation is simply an additional technique for numerical analysis. Combined with the computer's speed, it permits a study of a system's behavior when initial conditions and parameters are varied. The computer allows much larger systems to be formulated and studied than could be analyzed numerically without this tool.

2. *Management Science.* The models used by the operations researcher or management scientist are normative in intent. They incorporate certain decision variables that the management of the firm can manipulate, and the problem is to find rules for setting these decision variables so as to maximize, minimize, or satisfice with respect to some criterion—e.g., to maximize profits, minimize costs, or achieve a

¹ The classical models of business cycle and market behavior are surveyed in Henderson and Quandt [13]; Klein [15].

desired share of the market. A number of powerful mathematical techniques have been developed for approximating complex business decision-making situations and finding optimal decision rules by straightforward computational algorithms. Linear programming and dynamic programming are among the best known of these techniques [2] [4] [10] [26]

In this as in other realms, the riches of nature often embarrass art, for the complexities of the situations to be handled make inapplicable even such flexible techniques as linear programming. In these cases, simulation provides an alternative route to finding good decision rules. The system can be modeled with little regard to the applicability of known computational schemes, and the behavior of the model studied "empirically," that is, numerically with various decision rules over a range of hypothesized environmental histories. Decision rules can then be chosen that yield satisfactory system behavior under the conditions studied. Of course these techniques—the analytic techniques as well as simulation—can only be employed after adequate estimates have been made of the structural parameters of the system [1].

3. *Economic Decision-Making.* Almost all economic models refer at least implicitly to the decision-making of economic actors—a demand curve, for instance, can be interpreted as a set of hypothetical statements about what buyers would do under a range of circumstances. However, as we move from macroscopic to microscopic models, and as we move into the area of normative economics, the decision-making elements in the models becomes more explicit. In normative economics, our aim is to find rules for making "good" decisions. In positive microeconomics, our aim is to explain the decision-making behavior of economic agents.

Apart from its normative uses, simulation is a peculiarly attractive method for describing and explaining the decision-making processes at a microeconomic level. Its first, and most obvious, advantage is the same one that has led to its wide use in management science—it allows a degree of complexity to be handled that would be unthinkable if inferences could be drawn from the model only by standard analytical techniques. We shall provide, below, an example of an oligopoly theory that attains considerable realism and plausibility by exploiting this tolerance for complexity.

Simulation has a second advantage that may turn out, in the long run, to be even more important. We are beginning to learn that models do not have to be stated in mathematical or numerical form to permit us to simulate their behavior with electronic computers. With computers, we can simulate the behavior of symbol-manipulating and information-

processing systems, regardless of whether the symbols these systems handle are numbers, or are English words, phrases and sentences [16] [17].

It has always seemed quite natural to express macroeconomic models in mathematical form. After all, the phenomena with which they are concerned are quantities of goods and services and prices. The further we move, however, toward the detail of microeconomic analysis, the more dubious becomes the assumption that the important factors in the situation can all be represented as real numbers. We try manfully to encompass the phenomena. With great ingenuity, we axiomatize utility as a cardinal quantity; we represent risks by probability distributions. Having done this, we are left with the question of whether we have constructed a theory of how economic man makes his decisions, or instead a theory of how he *would* make his decisions if he could reason only in terms of numbers and had no qualitative or verbal concepts.

The question is still an open one, but the answer need no longer be imposed by the limitations of our analytic techniques. Research in so-called heuristic programming² has given us new ways of simulating systems that incorporate some of the kinds of nonnumerical symbol manipulation that, in humans, we call thinking and problem-solving. These technical advances open up new opportunities for building and studying realistic models of economic decision-making at the level of the individual firm and the individual decision-maker. Before we go on to some concrete examples, which will occupy the final part of this paper, we need briefly to discuss these new techniques of nonnumerical simulation.

III. *Nonnumerical Computation*

To appreciate the scope of opportunity for computer simulation of decision-making processes, we must be quite clear as to what a computer is, and what it is capable of doing.

1. Computers are general-purpose devices capable of employing various operations for manipulating symbols. They can read symbols (e.g., sense patterns of holes on punched cards), write symbols (e.g., create magnetic patterns on coated tape), erase symbols (change such patterns), and store symbols (retain patterns in magnetic cores or other kinds of internal memories). They can copy symbols (write patterns identical with patterns that are presented), and compare symbols (determine whether two patterns are identical or different). Finally, and most important, they can behave differentially depending

²Heuristics are important because they often lead us quickly to solutions which we would otherwise reach much more expensively by algorithmic and analytic techniques. For a more extensive discussion of heuristic programs see [18] [24].

on whether a pair of patterns, when compared, turn out to be identical or different (in computer terminology, they can transfer or branch conditionally). By virtue of this last capacity, they can follow strategies—make decisions that are conditional upon any kind of symbolic information.

2. The symbols, or patterns, that computers can read, write, compare, and process can be interpreted as numbers, as words, as English sentences, or even as geometric diagrams. How they are interpreted depends on the programs that process them. Historically, computers were specifically designed to process symbols as numbers—to perform arithmetical operations on them. The computer “hardware” of standard computers in use today does not incorporate this limitation. If computers are used mainly to do rapid arithmetic, that is because people want to use them in this way and not because this is the only way they can be used.

3. A number of computer programs have been written that process nonnumerical symbols. At least one of these [19] is capable of applying means-end analysis to the solution of a fairly wide range of types of problems. It proceeds somewhat as follows: I am given situation a , and am asked to arrive at b . What is the difference between a and b ? One difference I note is d . Do I know of any ways of dealing with differences like d ? Yes, using q sometimes removes such differences. Let me, then, apply q to a , and see whether the resulting situation, a' , brings me any closed to b . There is a growing body of evidence that this computer program simulates some of the main processes that humans use in solving problems.

4. A number of computer programs have been written that learn—that modify their own programs in an adaptive direction on the basis of experience [20]. The existence of such self-adaptive programs takes most of the meaning out of such statements as: “A computer can only do what you program it to do.” The statement becomes exactly parallel to a statement like: “A human being can only do what his genes program him to do.”

By reason of these capabilities of computers, we are today in a position to use simulation not only as a means of handling systems of great complexity, but also as a means of incorporating in our theories of human decision-making qualitative aspects of human symbol manipulation that have eluded our attempts at mathematical translation. To write a heuristic program of a decision-making process, we do not first have to construct a mathematical model, and then write a program to simulate the behavior of the model. We can directly write a program that manipulates meaningful symbols in the same ways that (we hypothesize) the human decision-maker manipulates them [11].

IV. *Examples of Microeconomic Simulation*

We shall illustrate our general discussion with three examples of computer simulations of microeconomic systems. Even though the use of simulation for building and testing theories is a recent phenomenon, a number of such models have already been reported in the literature. The particular examples we cite are not intended to be "representative," but are chosen to emphasize some important specific points. The first, a mathematical model of a duopoly situation, illustrates the richness in description of the decision-making process that is attainable with simulation techniques. The second, a normative program for solving an assembly-line balancing problem, shows how heuristic methods grossly modeled on the processes used by skilled human schedulers provide a usable simulation of a situation too complex to be handled by existing mathematical algorithms. The third, a direct simulation of the decision-making processes of an economic agent, shows how simulation can be used to describe and explain decision-making that involves non-numerical as well as numerical symbols and that is dependent in important ways upon the structure of human memory.

A. *A Theory of Duopoly*

Although there are numerous theories of duopoly, the model of Cyert, Feigenbaum and March [9] postulates in much greater detail than earlier ones the decision processes of the firms. In rough outline, each firm is assumed to: (1) forecast the reactions of its competitor, (2) revise its estimate of the demand curve, (3) revise its estimates of its own cost curve, (4) specify its profit goal (on the basis of its profit achievements in the past), (5) evaluate the alternatives available to it. If no alternatives which meet its goal are available, the firm (6) searches for opportunities for cost reduction, (7) re-examines its estimates of demand, and (8) lowers its profit goal to a more modest level. Finally, the firm (9) sets its output for the coming period.

The authors point to a number of important ways in which this theory differs from conventional oligopoly models [9, pp. 93-94]:

- (1) The models are built on a description of the decision-making process . . .
- (2) The models depend on a theory of search as well as a theory of choice. They specify under what conditions search will be intensified. . . . They also specify the direction in which search will be undertaken. . . .
- (3) The models describe organizations in which objectives change over time as a result of experience. . . .
- (4) Similarly, the models describe organizations that adjust forecasts on the basis of experience. Organizational learning occurs. . . .
- (5) The models introduce organizational biases in making estimates. . . .
- (6) The models all introduce features of "organizational slack."

By specifying particular values for the parameters of the model, the authors were able to generate behavior over time that simulated some of the striking general features of the history of the tin can industry from the time of entry of Continental Can Company as a competitor of the American Can Company.

B. Balancing an Assembly Line

F. M. Tonge [25] has built a heuristic model that balances an assembly line. The problem of balancing an assembly line is a member of the class of combinatorial problems in which the elements of a set are ordered or grouped on the basis of some criterion. This class includes such problems as job-shop scheduling, the traveling salesman problem, and the personnel and equipment assignment problems.³ Although these problems fall within the domain of operations research, they are too large to be solved by the usual algorithmic approaches. As an example of their size, consider the problem of assigning n men to n distinct jobs. If we let $n = 100$ (a relatively small number when you consider the size of the work force in a large company), the number of possible assignments is $n! = 100! = 9.3 \times 10^{157}$. Now if 10^6 orderings are examined in one second (which is the approximate manipulatory speed of current computers) it will take 3×10^{114} years to examine all possible orderings and select the optimal one. Thus, blind search techniques are simply not feasible methods for solving combinatorial problems of any size. Unfortunately, for these problems efficient algorithms that would reduce the search to practicable dimensions are not known.

The assembly-line balancing problem is essentially a task of assigning the elemental tasks making up the assembly operation to work stations along the line. In his model, Tonge assumes a fixed rate of production, e.g., a fixed conveyor-belt speed, and takes as given the time required to complete each elemental task. The goal of the program is to find the minimum number of workmen consistent with the given rate of production and the partial ordering constraints on the assembling of the product.

Tonge uses heuristics to simplify the problem sufficiently so that it can be solved by simple, straightforward methods. Although the heuristics were devised by observing industrial assembly-line balancers, the program does not attempt to reproduce in detail actual decision-making behavior. The program consists of three main phases. Phase one simplifies the line-balancing problem by constructing a hierarchy of aggregated elements. Thus, the elemental tasks are grouped into large "sub-assemblies" each of which has partial orderings between the elements and requires a given amount of operating time. The second phase solves

³These problems are described in detail in Churchman, Ackoff, Arnoff [4].

the simplified problem by assigning groups of available workmen to the subassemblies generated by phase one. It then treats each subassembly as a simplified line balancing problem and assigns the groups of workmen to the components of each subassembly. Phase three is a "smoothing" operation which transfers the tasks among work stations until the distribution of assigned time is as even as possible.

The program was tested by having it balance 11-, 21-, and 70-element problems. Even though the program does not produce optimal solutions the results of the 70-element test compared very favorably with an industrial engineer's balance of a roughly similar problem. Although the comparison is not too precise, since the industrial engineer had to deal with a few additional constraints, the program assigned 23 men to the assembly task as against the 26 men required by the industrial engineer's plan.

C. The Investment Decision Process

Although economists have constructed normative mathematical and statistical theories of investment decision-making, the investment model of G. P. Clarkson [5] is the first that attempts to describe and explain the actual processes of an individual making investment decisions. The goal of this model is two-fold: (a) to develop a model of individual trust investment behavior which incorporates and reproduces observable and inferable human problem-solving processes, and (b) to use the structure of the decision process itself as a method of predicting aggregate trust investment behavior.

The investment decision process is divided into three parts: (a) the analysis and selection of a list of stocks available for investment, (b) the formation of an investment policy, and (c) the selection of a portfolio. Although the selection process (c) contains rules on diversification and on how to select the number of shares to buy, the essence of the whole investment process lies in carrying out the prior analysis—steps (a) and (b). Thus, a major part [step (a)] of the model's function is to convert the information on the economy, industries, and companies as found in financial journals and reports into a list of stocks suitable for current investment. The next part [step (b)] consists of formulating an investment policy from the information on the beneficiary or the trust fund involved, while the final section (c) of the model performs the task of actually selecting the required portfolio.

The model, built by observing the decision processes of individual trust investors, is a theory of decision-making under uncertainty. The major hypotheses of this theory are structural and make strong assertions about the content of decisions as well as the order in which they take place. For example, the model asserts that the formation of expect-

tations is essentially a process of "pattern-recognition." While this is neither an entirely new idea nor the first time it has been asserted as an hypothesis⁴ the model asserts in detail how expectations are formulated, adjusted over time, and employed in the investment decision process.

The model, like the human investor, stores information in its memory in lists which contain closely associated pieces of information. These lists are arranged in a hierarchical order, e.g., for each industry there is a list of associated companies, and for each company there is a list of attributes that contain relevant financial information. Detailed information, given to the model from the outside, is processed to form new lists which summarize the basic data. In some cases the summary contains quantitative information, e.g., the average rate of growth of sales or earnings, and in other cases qualitative information, e.g., that the F.R.B. index of industrial production is expected to be above last year's, and last year's was below the index of the year before. These lists are the backbone of the decision-making process, since they provide the information necessary for the concept formation and pattern-recognition processes.

In the course of his work, an investor forms definite concepts of what different industries are like and how they can be expected to perform. Investors also associate different industries with different investment goals. These concepts change with time, but in order to alter them new information must appear that is sufficiently out of keeping with the current concept to force a reappraisal. Thus, small changes are unlikely to affect general concepts and what was a good buy yesterday will probably remain a good buy today and for some time to come. The model simulates these processes by scanner-selector mechanisms which search the summarized lists and match their values against sets of desired criteria. Since new information on the economy, industries, and companies is fed in at regular intervals the scanner-selector mechanisms allow the model to adapt both the criteria by which it selects and the portfolio selections it makes to current economic and market conditions.

Since there can be no theory of human problem-solving unless invariances exist among problem solvers, one cannot develop an aggregate investment model, along these lines, unless one first isolates the structure of the investment decision process that is invariant among investors. Although a general investment model has not been developed, the author suggests that his investment model contains that part of the investment decision process which is invariant among trust investors, and hence provides a basis for an aggregate model of trust investment

⁴ See Bruner, Goodnow, Austin [3]; Feldman [12].

behavior. Thus, the program may be viewed as a theory of the "representative investor," analogous to the Marshallian representative firm.

In order to test both assertions—that is, the ability of the model to predict both individual and aggregate trust investment behavior, a series of tests are performed. The first part of these tests consists in having the model select a series of portfolios for a particular set of actual trust accounts. These selections are compared with the actual portfolios chosen by the investment officers of several national banks. The second part of the testing program requires the model to predict the set of stocks purchased by trust funds in the state of Pennsylvania during 1960. The model makes its selections on a quarterly basis and the results are compared with the purchases of trust funds as reported by the Pennsylvania Bankers Association. As yet this testing program has not been completed and the reader can only be referred to the results obtained from an earlier and less complete model [6].

If space permitted, we could describe other instructive examples of microeconomic theories developed by means of simulation. Cohen, for example [7], has studied by simulation techniques an elaborate model of the shoe, leather, and hide industries; while Hoggatt [14] has simulated a perfectly competitive industry allowing entry and exit of firms. These and other examples are described in the papers by Orcutt and Shubik.

V. Directions of Simulation Research

We have seen that one advantage of simulation models lies in the complexity they permit. This does not mean that this new technique will relieve us of the task of making careful selection of our variables. The real world is still orders of magnitude more complex than the simulations we can handle on present or prospective computers. We still will need to think, and think hard, about what part of reality needs to be incorporated in the model if it is to provide reasonable answers to the questions we wish to ask of it.

Another advantage of simulation derives from our new-found ability to construct directly computer programs describing human problem-solving and decision-making processes without first going through the intermediate step of constructing mathematical models. We can look forward to theories that will handle the qualitative aspects of human decision-making as readily as the quantitative, and we can already find examples of such theories. These theories incorporate adaptive and learning behavior and include one or more aspects of heuristic reasoning. Since expectations play a central role in economic theory, and since all the evidence suggests that expectations are formed by a

process of pattern recognition, this process, incorporated in heuristic programs, will be the object of much research.

Finally, simulation appears to offer new approaches to the aggregation problem. To the extent that whole classes of individual decision units in the economy share relatively invariant structures, aggregation can be attempted by writing programs for individual units, and treating these as representative units, *à la* Marshall, in the aggregate model. Much work will have to be done before we will be in a position to evaluate the potentialities of this approach. One of its attractions lies in the new opportunities it affords for direct confrontation of the theory with concrete behavior. It does not restrict us to viewing the economic system through the wrong end of a telescope—limiting ourselves to census data and similar kinds of statistics. It permits us to see whether the decision-making processes we observe in the executive and the individual business firm correspond to the postulates about process that we incorporate in our models, and—if we are even moderately successful in finding satisfactory aggregation techniques—to work back and forth in our theory testing between micro-observations and aggregative data.

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DUOPOLY: COLLUSION OR WARFARE?

By ROBERT L. BISHOP*

It has long been recognized that oligopolists may achieve monopolistic results by means of an explicit agreement [4, p. 80], and that they may well behave in an essentially monopolistic way even without any explicit collusion [2, esp. pp. 46-48 and 100-1]. It is also widely appreciated, however, that some form of economic warfare is an alternatively possible outcome, at least temporarily, and that the possibility of such warfare is likely to play some sort of role in conditioning the oligopolists' collusion or mutual self-restraint even when actual warfare never occurs. There has been a notable scarcity, however, of specific theories of the nature of oligopolistic warfare, whether actual or potential, and of specific solutions of the problem of the precise terms on which warfare may be avoided.¹

We are often told that, when side payments are ruled out, rational and knowledgeable duopolists will at least end up somewhere on their Edgeworthian contract curve, or that, when side payments are allowed, they will maximize their joint profit and then divide it somehow. Now these "somewhere" and "somehow" solutions are not really solutions at all. If a pair of duopolists cannot agree on a *unique* solution, including a specific profit division, they cannot end up on their collusion locus at all. That is, if the duopolists persist in mutually incompatible demands upon one another, this must express itself in warfare, which carries them away from their collusion locus. This raises the question, which has been so surprisingly neglected, of the alternatively possible types of warfare, whether limited or not, that may then prevail. And

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¹ Chamberlin, without reference to the possibility of warfare, seems to imply that any oligopoly situation has a unique mutual-dependence-recognized solution. Actually, however, he has identified that solution only in cases involving perfectly symmetrical demand and cost data. Mathematical game theorists have also suggested several alternative solutions, so far limited to just duopoly but free of the symmetry restriction. Of these, that proposed by John Nash [10] is best known. Significantly, Nash gets his results only in conjunction with an equally specific theory of potential warfare (or threats).

Among economists, Fellner has given perhaps the richest discussion of the possible relationships between collusion and warfare, within the framework of his "four factors pertaining to the outcome within the bargaining range" [5, esp. pp. 24-33]. Yet these are rather vaguely qualitative factors, quite incapable of yielding a really specific solution even in situations that are deliberately simplified for purely theoretical purposes. Another richly suggestive discussion in much the same spirit is Rothschild's [11].

it likewise raises the question of the duration and course of such warfare, and the further question of the terms on which it may be terminated, if at all.

The present paper has several objectives. First, in the case of duopoly, an analysis will be presented of the way in which each duopolist might rationally convey, by means of a reaction function and without the necessity of any verbal communication, both his willingness to accept some contract-curve solution and also his readiness to back up that demand, if necessary, with a warring response. It will be stressed that a wide variety of such collusive offers are possible, and that their acceptability to the other duopolist is, in general, an open question. As this implies, it will be argued—in contrast to Chamberlin's recently reiterated view [3, p. 216]—that the mere fact that duopolists fully recognize their mutual dependence is not sufficient for a unique solution. Second, when a collusive agreement is not immediately established, a framework for analyzing the ensuing warfare will be presented. A central warfare hypothesis will be suggested, together with an analysis of its implications; and the significance of either more or less aggressive warfare will also be indicated. Finally, the question will be briefly considered whether an existing or threatening state of war contains any suggestive clues as to the terms on which it might reasonably be settled or avoided.

I. The Case of Constant and Equal Costs

The basic analysis may be illustrated with reference to a simple and familiar example: a homogeneous product subject to a linear demand is produced by a pair of duopolists at constant and equal average (and marginal) costs. For numerical convenience but without any further loss of generality, the quantity and price units may be selected so that each duopolist's average cost is $c_i = a$ and the demand-price function is $p = a + 24 - q = a + 24 - q_1 - q_2$ (where p is price, q is aggregate quantity, and q_1 and q_2 are the individual quantities of the respective duopolists).

Each duopolist's profit as a function of his own output (q_i) and the other man's (q_j) is then:

$$\pi_i = 24q_i - q_i^2 - q_iq_j.$$

When plotted for selected profit magnitudes as in Figure 1, these functions yield the duopolists' isoprofit curves. These three selected curves for each duopolist, corresponding to profits of 36, 72, and 108, illustrate the tangency condition that identifies the contract curve—which is defined as the locus where, for any given attainable profit of one man, the other man's profit is a maximum. In the present very

special case, that locus is the linear $q_1 + q_2 = 12$, implying everywhere a jointly maximized profit or $\pi_1 + \pi_2 = 144$. Were still other isoprofit curves drawn, those signifying successively higher profits for each duopolist would lie closer and closer to his monopoly point (where $q_1 = 12$, $q_2 = 0$, and $\pi_1 = 144$), and those signifying successively lower positive profits would be ever closer to the other duopolist's axis (where $\pi_i = 0$ because $q_i = 0$) and the line $q_1 + q_2 = 24$ (where $\pi_1 = \pi_2 = 0$).²

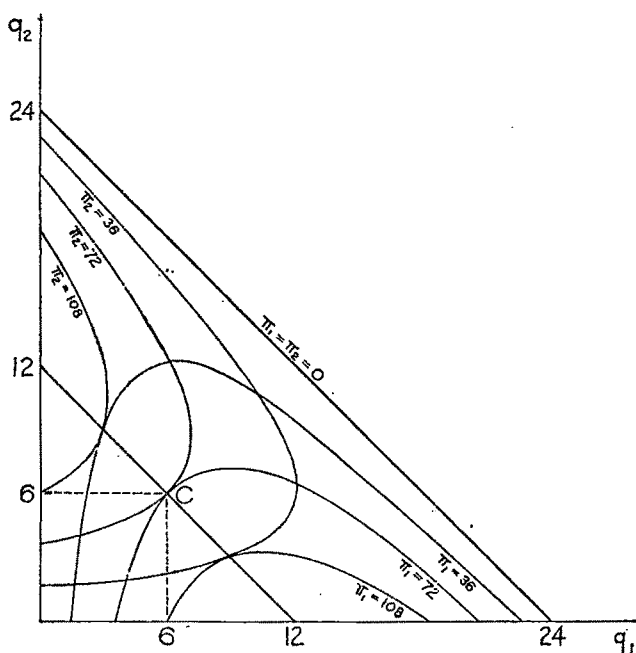


FIGURE 1

Of special interest in Figure 1 is the point of symmetrical collusion, at the midpoint of the duopolists' contract locus (where $q_1 = q_2 = 6$

² The various traditional solutions of Cournot, Bertrand, and Stackelberg will not concern me, because these correspond neither to successful collusion nor (save by coincidence) to "rational" warfare; but these may also be identified with reference to Figure 1. Each man's Cournot-reaction locus is a straight line between axis-intercepts of $q_i = 12$ and $q_j = 24$ —such that, for any given value of q_j , q_i is selected to maximize π_i . The Cournot solution, where these reaction lines intersect, then implies $q_1 = q_2 = 8$ and $\pi_1 = \pi_2 = 64$. Similarly, each duopolist's preferred Stackelberg equilibrium, occurring where one of his isoprofit curves is tangent to the other man's Cournot-reaction schedule, implies that $q_i = 12$, $q_j = 6$, $\pi_i = 72$, and $\pi_j = 36$. Accordingly, "Stackelberg warfare"—whereby each duopolist tries to push the other to the Stackelberg point preferred by the first—implies $q_1 = q_2 = 12$ and $\pi_1 = \pi_2 = 0$. By coincidence, this is also the profit implication of a Bertrand equilibrium, anywhere on the locus $q_1 + q_2 = 24$.

and $\pi_1 = \pi_2 = 72$). This point has been labeled C, since it corresponds to the solution that Chamberlin holds will be automatically established by the mere fact of "mutual dependence recognized."

When the duopolists' situations are indeed perfectly symmetrical and a symmetrically collusive solution therefore exists, I am inclined to agree that this outcome is rather more likely to emerge than any single other one. This outcome cannot be established as somehow inevitable, however, even when the duopolists are assumed to be eminently intelligent, knowledgeable, and "rational." Even in the present primitively symmetrical case, I shall argue, an *asymmetrical* collusion is also possible, with or without a preliminary interlude of warfare or (when verbal communication is avoided) an interval in which the duopolists improvise some form of nonverbal communication. Secondly, I shall also argue that even prolonged warfare is possible, notwithstanding the high degree of "rationality" that both duopolists will be assumed to possess.

In order to explore the possible patterns of behavior that may emerge with the indicated demand and cost data, I shall assume that both duopolists know, and are known to know, the objective facts of their indefinitely recurrent market situation. For convenience and simplicity, it is also assumed that the only "dynamics" in the analysis are in the memories and anticipations of the duopolists themselves. Specifically, the demand and cost data, in remaining unchanged from one period to the next, are also assumed to be independent as among the various separate periods. The duopolists are also assumed to be quantity-setters rather than price-quoters, since the mere quoting of equal prices for perfectly homogeneous products would leave quantities indeterminate. Thus, in each period, both duopolists simultaneously determine their quantities, which are then sold at a price determined in accordance with the demand function.

Because of its greater theoretical interest, I shall also employ for the most part the assumption that the duopolists scrupulously refrain from any verbal communication with one another. In the present severely simplified situation, however, it would not make much difference if verbal communication were allowed; for this would only allow the duopolists to put into words the demands and threats that they must in any event convey to one another by other means when verbal communication is effectively prohibited. Furthermore, as long as the strategy of each duopolist is confined to the single dimension of the quantity of his output, communication solely by means of reciprocal actions in the market is likely to prove fully adequate, provided only that both duopolists have at least a moderate degree of social ingenuity. In the real world, of course, the nonverbal communication of demands

and threats may well present insuperable difficulties, since the dimensions of business strategy are then likely to be extraordinarily numerous and since at least some of those strategies are almost certain to have nonquantitative aspects.

In the case where the duopolists produce a homogeneous product at equal and constant cost, and in this case alone, their contract line is also their joint-maximization locus. Therefore, since any distribution of a jointly maximized profit is possible without side payments, the duopolists would never have any occasion even to consider such supplementary payments, as they would if the maximization of their combined profit proved to be inconsistent with an "acceptable" profit division.

As the duopolists now begin their indefinitely recurrent relationship, if they are in fact willing to share a jointly maximized profit equally, they will immediately establish the symmetrically collusive solution. Thus, if each duopolist is in fact willing to settle for a profit of $\pi_i = 72$ and conjectures that the other will be too, both will set their outputs at 6 units and thereby find their expectations confirmed. On the same expectation as to the other's output, of course, either duopolist could expect to gain a larger profit for at least one period (such as $\pi_i = 81$, with $q_i = 9$ and $q_j = 6$); but in a recurrent market this greedy short-run strategy may well be rejected by both men because of an entirely plausible expectation that it would result in a self-protective or, worse, retaliatory response, which would depress profits in subsequent periods below the level of 72. In a recurrent market, in short, the mere expectation of a continuing relationship may be sufficient to inspire a pair of basically compatible duopolists to behave with the self-restraint necessary to maintain a tacitly collusive agreement.³

Even in this clearest of all instances of spontaneous collusion, it is still important to appreciate all of the conditions on which this feat of mutually successful mind-reading depends. First, the empirical likelihood of this outcome is appreciable only because the inherent symmetry of the duopolists' situations, when it is fully known to both, provides such a strong "signal" for symmetrical collusion. Obviously, no such signal exists when there is even the slightest asymmetry in the objective data for the duopolists' market decisions.

Second, this outcome also depends on the assumed condition that the duopolists are in fact willing to share a jointly maximized profit

³In a nonrecurrent market, by contrast, these grounds for self-restraint would be absent, and even an explicit agreement would be something less than self-enforcing. In that context, interestingly enough, the Cournot solution is by all odds the most eligible one, since it is the only situation in which each man selects the output that will maximize his profit on the basis of a *correct* expectation as to the other's output. (See [9] [13, esp. pp. 60-63].)

equally. Game theorists are willing to underwrite this symmetrical solution only when there is an additional symmetry in the "utilities" of the duopolists—that is, utilities of the von Neumann-Morgenstern type, as based on the subjects' attitudes toward risk. I choose not to adopt this approach here, however, because it involves the extremely dubious assumption that the duopolists' utility functions are known to one another. If the duopolists' shares of the market were to depend on their utility patterns, they would obviously have an incentive to misrepresent those wholly subjective data. It is doubtful, therefore, that such data are even conceivably knowable, let alone that they can be casually assumed to be actually known. The plausible relevance of such data—if they could be known—is, however, one of the reasons why duopolists may not be willing to share a market equally, even when the objective data are both symmetrical and known. This consideration reinforces, therefore, my view that the willingness or unwillingness of both duopolists to accept the symmetrically collusive solution in the present example is a critical question in its own right.

Third, even when the duopolists are in fact favorably disposed toward symmetrical collusion and in fact arrive at that result immediately and spontaneously, neither can really be sure in advance that this will actually be the outcome. Hence each must be prepared for a different development, with at least a tentative reaction plan if his rival should set some output other than his symmetrically collusive one. At least a threat of warfare thus lurks in the background as an alternative to successful collusion in the minds of both duopolists, even when they are spared the necessity for any actual warfare.

Accordingly, let us take the point of view of the first duopolist as he considers his policies at the outset of a continuing relationship with his rival. He may hope—and more than half expect—that his rival will set his quantity at $q_2 = 6$; and, being willing to collude symmetrically, he therefore sets his own quantity in the first period at $q_1 = 6$. If he then proves to be correct about his rival's quantity in the first period, that is presumably the end of the matter; provided that neither has a change of heart, they will live happily ever after as compatibly collusive duopolists. Each duopolist must, however, be prepared to react somehow if his rival sets some other quantity. A variety of such reaction patterns are, of course, possible. The one assumed for the sake of illustration implies that the first duopolist will seek a solution on the contract curve if q_2 is less than (as well as equal to) 6 units, and that he will respond with a particular form of restrained belligerence if q_2 is greater than 6 units.

The reaction schedule of the first duopolist is specifically assumed to consist of the three connected linear segments labeled R_1R_1 in Figure 2. Its three branches imply, respectively, that

if $q_2 \leq 6$, $q_1 = 12 - q_2$;
 if $6 \leq q_2 \leq 12$, $q_1 = q_2$;
 and if $q_2 \geq 12$, $q_1 = 12$.

The first is a "conciliatory" branch: as the second duopolist's quantity is visualized to be increased toward a maximum of $q_2 = 6$, the first *reduces* his quantity along the contract and joint-maximization locus, thus reducing his own profit and increasing that of his rival. But this first branch also implies that, if the second duopolist seems to be satis-

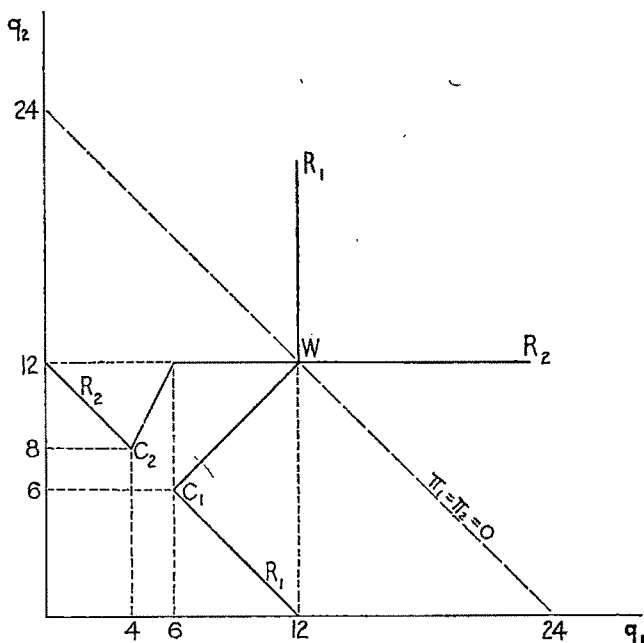


FIGURE 2

fied with a profit of less than $\pi_2 = 72$, the first duopolist is pleased to adjust his quantity so that at least their combined profit is maximized.⁴

⁴ Notice that, if q_2 is set below 6 units and is held constant there, the first duopolist by raising q_1 above 6 units increases his own profit and reduces that of his rival. This is still a good idea, however, if the second seller's timid choice reflects a willingness to accept an *efficient* collusion in which he receives only the profit implied by his initially selected output. If, on the other hand, the second seller is perverse enough to make his initial decision contingent on the first seller's selecting an output other than the one that would put them on their contract curve, the first seller can hardly be expected to know what quantity he was in fact expected to set. Finally, however, even if the second seller makes his choice with an initial willingness to accept less than half of a jointly maximized profit, the fact that the first seller shows himself willing to accept just half of such a combined profit may now persuade the second seller to raise his profit demand, even as the first seller does the same. To attribute this type of progressive learning process to one's rival implies the adoption of a more complex reaction schedule than the one now

The second branch reflects the antagonistic behavior that is the essence of warfare: as the second duopolist's quantity is increased from 6 units toward 12 units, the first responds with matching increases in his own output. This is a warring response because its sole purpose is to punish one's rival, even at the cost of hurting oneself in the process. Along this branch, specifically, the profits of both duopolists decline steadily from 72 to zero. The third branch reflects a limiting influence on the warfare: if q_2 were increased beyond 12 units, the first duopolist would simply maintain his output at the ceiling of $q_1 = 12$, instead of continuing to match his rival's quantity. This is the indicated response, specifically, if the first duopolist is willing to suffer a loss provided that his enemy is thereby made to suffer a larger one.⁵

Somewhat more should be said about the warfare responses that the particular second and third branches of R_1R_1 reflect. Consistent with this seller's assumed willingness to share a jointly maximized profit equally, the second branch also emphasizes his refusal to take less than an equal share of *any* positive profit. Thus, when the sum of q_1 and q_2 is less than 24 units (allowing the same positive profit per unit for both sellers), the first seller will not permanently allow q_2 to be greater than his own q_1 . When $q_1 + q_2 > 24$, however, it is no longer desirable to produce the greater quantity, since both sellers then suffer losses proportional to their outputs. Hence that is another way of looking at the first seller's unwillingness to increase his output above the level of 12 units.

Finally, this seller's warfare output is uniquely 12 units if he adopts as his war policy the criterion of maximizing the algebraic excess of his own profit over that of his rival. Thus, from our earlier profit equations,

$$\pi_1 - \pi_2 = 24q_1 - q_1^2 - 24q_2 + q_2^2.$$

This expression is a maximum when $q_1 = 12$, irrespective of the magnitude of q_2 . This is by no means the only warfare strategy that a duopolist might choose to adopt; but it is a plausible one, and it effectively illustrates as the essence of warfare the duopolist's deliberate intention to reduce the profit of his rival provided that the simultaneous effect on his own profit is not disproportionately painful.

When the second duopolist is also disposed to accept just half of a

under consideration. At any rate, there may well be almost as much uncertainty as to ensuing developments when duopolists are overly compatible as when they are incompatible.

⁵ On the other hand, if the first duopolist should wish to protect himself against losses, irrespective of the comparative losses of his rival, he may do so by adopting an alternative third branch that coincides with the zero-profit locus in the range where his rival's output exceeds 12 units (i.e., if $q_2 \geq 12$, $q_1 = 24 - q_2$).

jointly maximized profit, he may very well have a reaction schedule that is the exact counterpart of the one just described for the first duopolist. If so, these reaction schedules would coincide over the range of $6 \leq q_1 = q_2 \leq 12$. By itself, that would imply a neutral equilibrium anywhere in that range. On the other hand, if either duopolist is then flexible enough to experiment with alternative outputs down to and including 6 units, the fundamental compatibility of the two rival sellers will thereby be revealed; and the symmetrically collusive solution will be attained.

Notice, however, that there is always a fundamental asymmetry when the teaching-and-learning process is based on one duopolist's setting up a reaction schedule and the other duopolist's experimenting in such a way as to find out what it is. Thus, even fundamentally compatible duopolists might conceivably fail to discover that fact about themselves if both were so rigidly addicted to the teaching role that neither ever adopted the learning role. Similarly, there would be an even more chaotic result if each could visualize himself only in the learning role, so that neither ever set up a reaction schedule for the other to discover. These are difficulties, of course, that even the slightest explicit communication would avoid. Even without that aid, however, it would require only a minimum of flexibility on the part of at least one duopolist for them to achieve mutual understanding.

To illustrate the contrasting case where the duopolists are not immediately compatible, let us now suppose that the second duopolist—unlike the first—wants more than half of a jointly maximized profit. Specifically, the second duopolist sets his sights on an asymmetrically collusive equilibrium in which his own profit will be twice that of his rival. In Figure 2, therefore, by contrast with the first seller's willingness to collude at the point C_1 , the second duopolist is unwilling to collude unless he can receive a profit at least as great as that implied at the point C_2 —where $q_1 = 4$, $q_2 = 8$, $\pi_1 = 48$, and $\pi_2 = 96$. Accordingly, let us suppose that the second duopolist sets up the three-segmented reaction schedule, R_2R_2 . This implies that

$$\begin{aligned} &\text{if } q_1 \leq 4, q_2 = 12 - q_1; \\ &\text{if } 4 \leq q_1 \leq 6, q_2 = 2q_1; \\ &\text{and if } q_1 \geq 6, q_2 = 12. \end{aligned}$$

Here the first branch is similar to that of the first duopolist's reaction schedule, except that it stops short of the midpoint of the contract line. Similarly, the second branch also resembles the first duopolist's, except that it implies a different ratio of the two sellers' outputs. As in the case of the first duopolist, however, the second is assumed to set

the same maximum warfare output of 12 units; so the third branch of his reaction schedule is at the level of $q_2 = 12$.⁶

As long as both duopolists maintain the attitudes that these reaction schedules represent, warfare is of course the only possible outcome; for they will quickly gravitate toward the unique warfare point (labeled W in Figure 2) where $q_1 = q_2 = 12$ and $\pi_1 = \pi_2 = 0$.⁷ That warfare will persist, moreover, as long as neither duopolist is willing to accept the terms on which the other is willing to collude, or as long as neither is willing to change his initial reaction schedule.

If only as a matter of experimental interest, however, either can at least learn what his rival's terms are by varying his own output and observing the reaction-schedule responses of his rival. Thus the first duopolist could discover, as the essential property of his rival's R_2R_2 schedule, that his own profit would be maximized at $\pi_1 = 48$ at the point C_2 .⁸ As long as the second duopolist stubbornly maintains the reactions of R_2R_2 , therefore, the first duopolist must make a choice: (1) he may simply surrender, accepting his profit of $\pi_1 = 48$ while conceding twice as great a profit to his greedy rival; (2) he may surrender partially by producing an output somewhere between 4 and 12 units, thus accepting a positive profit that is smaller than 48 and also

⁶ Again, of course, variations are possible. For example, the second duopolist might have extended the second branch all the way to the zero-profit locus (i.e., if $4 \leq q_1 \leq 8$, $q_2 = 2q_1$). If so, however, the third branch would then have had to coincide with that zero-profit locus at least as far as its midpoint, unless the second duopolist happened to be willing to suffer a loss greater than that suffered by his rival.

⁷ As will appear later, the assumption that I have made about the duopolists' warfare outputs yields this zero-profit result for both of them only in the present special case of constant and equal costs. This warfare point may be reached in a variety of ways. Thus, if the duopolists set the initial outputs of $q_1 = 6$ and $q_2 = 8$, W will be reached in two steps if q_2 is changed first and then q_1 ; it will be reached in three steps if q_1 is changed first, then q_2 , and then q_1 again; or it will be reached in two steps if both duopolists change their outputs simultaneously in accordance with their respective reaction schedules.

⁸ The first seller may also learn, incidentally, that another acceptable point, at the juncture of the second and third branches of R_2R_2 , implies $q_1 = 6$, $q_2 = 12$, $\pi_1 = 36$, and $\pi_2 = 72$. By a coincidence, this happens to be the second seller's Stackelberg-equilibrium point, which would be the point most favorable to the second seller if he were constrained to set up some reaction schedule perpendicular to his own axis. But this also reveals the shortcoming of a Stackelberg policy when both sellers are sophisticated: it would be silly to try to force a rival to a Stackelberg point (cf. the contract curve) rather than, with a different type of reaction schedule, to a point (on the contract curve) where both sellers' profits could be higher; cf. [5, pp. 116-19]. On the other hand, reaction schedules such as R_2R_2 also lend themselves to an attempt to force a rival to a contract-curve point where his profit would be even less than in the Stackelberg equilibrium unfavorable to him. This also reveals the essential pointlessness of attempting to narrow the "relevant range" of contract-curve solutions in the light of a Stackelberg solution, as the late A. M. Henderson sought to do [6, pp. 572-73]. In my view, such a relevant range can be justified only in terms of the profits at a theoretically defensible warfare point. In the present instance, accordingly, the "relevant range" is nothing less than the entire contract line.

smaller than his rival's; or (3) he may meet stubbornness with stubbornness by producing an output of 12 units, thereby inflicting on both his rival and himself the punishment of a zero profit.

Let us examine further, with the first seller, the features of his "trilemma." Option (1), although somewhat humiliating, holds out at least the apparent prospect of an immediately maximizable profit; and, if the second duopolist should prove to be irreconcilably stubborn, it would represent the best that the first could do for himself financially. But even such a surrender would be dangerous, especially if the "appeasement" was meekly accorded; for, if the second seller should thereby become scornfully convinced of his rival's timidity, he might increase his demand by moving his reaction schedule so that its critical point would leave his submissive rival with an even smaller profit. Option (3), while the most clearly consistent with conventional standards of self-respect (which may be no less relevant in this type of situation than pecuniary self-interest), also entails the greatest financial sacrifice, at least immediately. At the same time, this choice has the merit of putting maximum pressure on the ambitious second seller to soften his profit demand; and if, for example, that seller should sooner or later give up the struggle by accepting the solution at C_1 , the first seller would very likely feel that his own intransigence was thereby justified.⁹ Finally, option (2) represents merely some degree of compromise between complete surrender on the one hand and complete intransigence on the other. Hence it also entails some variable mixture of the advantages and disadvantages that are implicit in the more extreme reactions. Merely partial surrender may also be the means, however, of signaling a willingness to compromise—at some point between C_1 and C_2 in Figure 2.

Clearly, we cannot predict just what the outcome of this conflict of interest will be; for it will depend on the tempers of the two men, including their subjective assessments of one another's attitude. Indeed, one possible result is a more or less permanent state of economic war. To many analysts, this outcome is inconsistent with "rational" behavior. The duopoly problem is not solved, however, by invoking that adjective. We must first ask: who is irrational? If it be replied that the second seller is, because he deliberately suffers a zero profit rather

⁹ In that event, if a purely financial hindsight test is applied, the seller who ultimately surrenders will presumably regret the duration of his resistance—except as this constitutes at least partial insurance against his having to meet still stronger demands later on. But even the victor may regret his own policy of resistance if it has to be especially prolonged; for hindsight may reveal that the profits of war followed by victory, when discounted from the beginning of the warfare, were less than the similarly discounted profits of immediate surrender. As with all dynamic problems, interest-rate considerations obviously enter into the problem of warfare among oligopolists in a recurrent market.

than share a monopoly profit equally with his amiable rival, then it must also be granted that the first seller is hardly less irrational, since he deliberately elects a zero profit rather than a one-third share of the same monopoly profit. But if the unaggressive first seller now becomes rational in this sense—that is, surrenders—the greedy second seller is thereby revealed as having been supremely rational. With duopolists, in short, it takes two to be irrational, and it also takes two to be rational.

Accordingly, there is no uniquely rational behavior that can be specified for an individual duopolist, even in the simplest possible situation of equal and constant costs, since the ultimately most profitable behavior for one seller depends crucially on the responses of the other. Thus, if a tough-minded seller finds himself in a recurrent duopoly relationship with an essentially timid (or perhaps just a financially needy) rival, it would be irrational for him not to exploit his rival's weakness. Even then, however, there is bound to be a fundamental uncertainty as to how far the more timid rival can be pushed. On the other hand, if the same tough-minded seller confronts a more worthy opponent, he would be well advised to pursue a less aggressive strategy. In either case, of course, the danger is that each duopolist may underestimate the stubbornness of the other; for then warfare will ensue, at least until one or the other surrenders or they both compromise their differences in some fashion.

The problem is further complicated, of course, by the strategy of "bluffing." Thus a duopolist may succeed in gaining a bigger profit share than the minimum that he would actually be willing to accept if he can successfully convey an impression of exaggerated stubbornness. And indeed, the more "unreasonable" he makes himself appear, the more likely is this strategy to succeed, at least against certain types of opponents. With reference to the dubious concept of rational behavior, in other words, the highest rationality may consist of the appearance, if not the fact, of the greatest irrationality. On the other hand, the resistance of an opponent typically grows out of at least the suspicion that his rival *may* be bluffing, whether he is or not. Yet it is the essence of bluffing that it cannot be reliably distinguished, at least at the time, from the real thing; and it is this, more than anything else, that may breed a long stalemate.

The foregoing analysis is not appreciably changed if the duopolists are free to communicate explicitly. The ability to put their demands and their threats into words may or may not facilitate their arriving at a collusive agreement. With or without verbal communication, there is still the same range of alternative divisions of a jointly maximized profit, and the same alternative of warfare when no agreement is reached.

Although, for the sake of definiteness, the foregoing discussion has

also been based on a specific institutional procedure, its relevance is not confined to markets with that procedure. For example, if one or the other duopolist always has the opportunity of specifying his output first, this does not give him any net advantage (as it would in a nonrecurrent market). Rather, when the market is recurrent, either duopolist still has the privilege of setting up a reaction schedule as in Figure 2; and the same range of possible outcomes, involving either collusion or warfare, will still apply. Similarly, if the market happened to be a more or less continuous one, instead of being broken up into discrete periods, the same possibilities would still exist.

Finally, even if both duopolists are free to improvise their own market procedures—for example, as between price-quoting and quantity-setting—the same analysis again applies. To be sure, if they were restricted to just price-quoting, this would pose once again the difficulty that, with strictly homogeneous products, the quantity demanded of each duopolist is indeterminate when they both quote the same price. This has sometimes been interpreted, moreover, as facilitating collusion.¹⁰ Actually, however, even if the mere quoting of equal prices in this case always resulted in an exactly even division of the market, a duopolist who was determined to hold out for more than half of the market would still be free to try to force his rival into a policy of quantity limitation by setting up a quantity-reaction schedule such as R_2R_2 in Figure 2. In that way, if the rival is reasonably sophisticated, the problem can be brought right back to the terms on which it has been analyzed above.¹¹

With even the slightest product differentiation, of course, a collusive agreement in terms of prices comes to exactly the same thing as one in terms of quantities. In other words, when the duopolists' prices are determinate for all possible pairs of their quantities and when their quantities sold are similarly determinate for all possible pairs of their

¹⁰ For example, Schumpeter [12, pp. 980-81, n. 25] expressed the opinion that the pure duopoly problem has a unique solution only when the duopolists are price-quoters and do *not* attempt to agree as to their relative outputs.

¹¹ I thus sympathize entirely with Stackelberg's refusal to analyze duopoly with homogeneous products except with reference to quantities as the relevant independent (or policy) variables. Moreover, I also have grave misgivings about the glib assumption that the duopolists will always sell equal quantities—or equal quantities "on the average"—if they merely quote equal prices. Thus, are they supposed to have expectations of selling equal quantities solely by virtue of their existence as "firms"? And if so, could either firm acquire two-thirds of the market just by reconstituting itself as two firms, even without any change of a more tangible kind? As these questions suggest, some physical conception of the market and its selling outlets and procedures is really needed before one can arrive at a defensible concept of even the "probabilities" of patronage. Furthermore, the concept of "identical products" is also revealed as a logically treacherous one when sellers have an urge to sell more than they are able to sell at a common price; for then even the slightest differences in such things as a seller's location, personality, or appearance become decisive.

prices, there is a perfect one-to-one correspondence between any point on a quantity-reaction diagram and some one point on a price-reaction diagram, and vice versa. Hence, even though "naïve" solutions (Cournot or Bertrand) and "naïve-sophisticated" solutions (Stackelberg) differ according as the duopolists are price-quoters or quantity-setters, there are no such differences in the context of efficient collusion.¹²

Warfare, however, may or may not be equivalent with price-quoting and quantity-setting, even when products are differentiated. Thus, when each duopolist seeks to maximize the excess of his own profit over his rival's, the quantity-setting practice regularly calls for pure strategies in the warfare "equilibrium"—that is, definite outputs rather than a probability mixture of various alternative outputs. In many instances price-quoting warfare also calls for pure strategies; and, when it does, the implied warfare equilibrium is exactly the same as with quantity-setting, provided again that the products are at least slightly differentiated. On the other hand, price-quoting warfare sometimes involves mixed strategies; and its implications are then very different.¹³

II. *Collusion under Other Cost Conditions*

1. Suppose that both duopolists have the same schedules of increasing average and marginal cost. Their contract locus is then no longer linear, but a curve concave to the origin, as a reflection of the fact that

¹²I am assuming here that price-quoting duopolists will both satisfy the total demands for their respective products at the collusive prices that they set. As Shubik has pointed out [13, p. 104], it may sometimes be possible for duopolists who supply a homogeneous product to make a greater combined profit by quoting different prices if the quoter of the lower price then satisfies only a portion of the total demand at that price. For example, if he could effectively choose which buyers to satisfy (or if the satisfied buyers just happened to be a favorably nonrepresentative sample of all of his would-be customers), and if the satisfied buyers could not effectively resell to others, then he and his fellow duopolist could make the same kind of extra profit that a monopolist could make under similarly favorable conditions by a policy of price discrimination. A similar possibility might also exist when products are differentiated; but I ignore it here since it would drastically complicate the entire analysis.

¹³With homogeneous products, price-quoting warfare may call for mixed strategies when both duopolists have increasing marginal costs; for, even though each warring duopolist then has an incentive to undercut his rival within a certain price range, he may have an incentive to set an appreciably higher price when his rival's price is sufficiently low (provided that it would not be to the rival's interest to satisfy the full demand at his low price, which depends on the precise configurations of demand and the duopolists' costs); cf. [13, p. 104]. The same phenomenon may also occur with differentiated products, even though differentiation tends to suppress it. Finally, if inventories are such that a low-price quoter can satisfy the full demand until he has a chance to raise his price nearer the high price quoted by his rival, this would frustrate the high-price policy and so re-establish the warfare equilibrium in terms of pure (low-price) strategies. On this interpretation, there would never be any difference between price-quoting and quantity-setting as long as the duopolists' products are at least slightly differentiated.

collusive, increasing-cost duopolists will always produce a greater combined output (at lower average and marginal costs) than either of them would in the role of monopolist. Moreover, this contract curve is no longer a joint-maximization locus. Rather, the combined profit of the duopolists is then a maximum only at the midpoint of their contract curve, where they both produce equal outputs at the same marginal cost. If anything, this would strengthen any tendency that they might have toward the symmetrically collusive solution. If the duopolists have asymmetrical personalities, however, asymmetrical collusion may still emerge, just as in the case of equal and constant costs. If side payments are admissible, of course, efficient collusion then requires the duopolists to produce equal outputs; and it would then be the purpose of the side payments to redistribute the maximized combined profit unequally. If side payments are ruled out, however, any asymmetrical collusion will fail to maximize the duopolists' combined profit, even when they collude efficiently in the weaker sense of ending up somewhere on their contract curve.¹⁴

2. The case where the duopolists have the same decreasing average and marginal costs is, I think, a realistically more important one. Then their contract curve is convex to the origin, as a reflection of the fact that a pair of collusive, decreasing-cost producers will produce a smaller combined output (at higher average and marginal costs) as compared with the profit-maximizing output of either of them alone. By the same token, their joint profit is maximized only at either end of their contract curve; and it is lower at the midpoint of their contract curve than anywhere else on that locus. If side payments are allowed, of course, efficient collusion requires one or the other of the duopolists to produce the entire monopoly output; and it would then be the purpose of the side payments to bring about at least a more nearly equal division of the maximized joint profit. When side payments are forbidden, on the other hand, this cost pattern would strengthen any tendency that the duopolists might have toward an asymmetrical collusion; and it might even lead (especially in the additional light of the warfare implications to be considered below) to the complete and permanent exclusion of one of them from the market. On the other hand, amicably symmetrical collusion might still be established, despite its relative inefficiency.¹⁵

¹⁴ Product differentiation also has much the same effect on the shape of the contract curve on a quantity diagram. For example, if duopolists produce symmetrically differentiated products at constant and equal average costs, their contract curve will also be concave to the origin; and their combined profit will again be maximized at the midpoint of that curve.

¹⁵ In the absence of fixed costs, a pair of producers with the same decreasing costs could collude efficiently even without side payments by taking turns producing. On the

3. If both duopolists have the same U-shaped average and marginal costs, the possible consequences are more complex. In general, those consequences vary according as the decreasing or increasing branches of the average and marginal cost curves have predominating influence.

4. The principal implications when the duopolists have different cost curves may be adequately illustrated by the case in which they both have constant but unequal average (and marginal) costs. There is then only one joint-maximization point, at the monopoly point of the low-cost producer. The contract curve then connects that point with the monopoly point of the high-cost producer; and, in that direction, it implies a progressively smaller combined output as well as a progressively smaller combined profit. As to the shape of the contract curve, it happens to be (at least slightly) convex to the origin (though very nearly linear even with a relatively sizable cost difference). When side payments are allowed, efficient collusion naturally requires that the low-cost duopolist produce his monopoly output; and any side payment that he then makes to the high-cost duopolist is in the nature of a bribe to insure his inactivity in the market. Naturally, there is not even a faint presumption of an equal sharing of the jointly maximized profit in this instance. When side payments are prohibited, and the high-cost firm produces some positive output, efficient collusion obviously then requires both a smaller combined output and a smaller combined profit. Then, especially when the difference in the two producers' costs is only moderate (relative, in particular, to the gap between the monopoly price and the unit cost of the low-cost producer), it is quite likely that both duopolists will be in fact active producers (although this conclusion also rests in part on the implications of warfare, to be considered below). On the other hand, it is also quite likely—if the duopolists succeed in agreeing upon some contract-curve solution—that the collusion point on which they agree will be at least somewhat closer to the low-cost producer's monopoly point than to the high-cost producer's.

In all such different-cost cases, of course, there is no such thing as "symmetrical collusion." Thus, if both producers were to end up with equal profits, that would require the high-cost duopolist to produce the larger output; and if they were to divide the market equally, that would imply a larger profit for the low-cost producer. Actually, however, since the low-cost duopolist is likely to hold out for a larger output than his rival's, there is no particular presumption that either of

other hand, there are obvious difficulties about this in view of the interdependence of each firm's costs from one period to another, owing to the fixed costs of durable equipment and organization. It is in the light of these considerations that I visualize both duopolists as active producers when they collude without side payments.

the foregoing special solutions would emerge as *the* solution. Moreover, no particular point on the asymmetrically curvilinear contract curve stands out with any remotely plausible obviousness as being *the* most eligible collusion point.

It then also follows that no particular collusive-equilibrium price can be predicted. In the case of unequal but constant costs, to be sure, the closer a collusion point is to either producer's axis, the closer will be the price to his monopoly price; but that merely limits an efficiently collusive price to the range between the two producers' monopoly prices. As corollaries of these observations, it further follows that simple "output leadership" is not likely to constitute more than approximately efficient collusion. Thus, if it is either explicitly or tacitly agreed only that one duopolist will keep his output in some constant ratio to the other duopolist's, the latter would then have the motive to make the combined output the same as his own monopoly output. In turn, this would make the combined output either too large or too small for precisely efficient collusion, according as the high-cost or low-cost producer was the output leader. In other words, efficient output leadership would call for a more complex formula relating the outputs of the unequal-cost duopolists—and a different formula for every alternative point on the contract curve. Simple price leadership, of course, would again imply an indeterminacy of individual outputs (if it merely constituted an agreement to charge the same price); and, if accompanied by an agreement fixing the ratio of outputs, it would then amount to the same thing as simple output leadership.¹⁶

5. Little can be profitably said in a general way about the cases in which costs are both nonconstant and different, except that they may exhibit any of the features already mentioned in the preceding separate discussions. In particular, however, when the duopolists' average and marginal costs are both unequal and decreasing, this puts the low-cost producer in an even stronger position. On the other hand, when the duopolists' average and marginal costs are unequal and increasing, there is then likely to be a single joint-maximization point at which both duopolists produce positive but unequal outputs.¹⁷

¹⁶ It should be appreciated that "leadership" is being used here in the same sense as in the familiar phrase, "price leadership," rather than in the quite different Stackelberg sense. Stackelberg's usage implies that the "leader" (or "active" strategist) fixes his output (or price) and then allows the "follower" (or "passive" rival) to set his output (or price) at whatever level will maximize his own profit. When output is the relevant independent variable for both producers, "leadership" is almost always the more advantageous role, but followership is typically the more advantageous when the duopolists are price quoters [5, pp. 110-13]. In neither event, however, does a Stackelberg equilibrium result in even approximately efficient collusion (save in certain degenerate special cases).

¹⁷ For a numerical illustration of this case, with most of the extant duopoly "solutions," see [7].

III. *The Anatomy of Warfare*

I have nominated as a central warfare hypothesis that each duopolist may adopt the strategy of maximizing the algebraic excess of his own profit over that of his rival. When this policy is followed by both suppliers of a homogeneous product, the key implications of the resulting warfare are relatively easy to deduce for all shapes of demand and cost curves. Thus, let us denote the demand-price function as $p = p(q) = p(q_1 + q_2)$, and the total-cost function of the duopolists as $C_1 = C_1(q_1)$ and $C_2 = C_2(q_2)$, respectively. In the following operations, the duopolists' marginal costs (dC_1/dq_1 and dC_2/dq_2) will be more simply designated as m_1 and m_2 , respectively. Furthermore, in view of the simple additive relationship of q_1 and q_2 in the demand function, use will be made of the fact that $\partial p/\partial q_1 = \partial p/\partial q_2 = dp/dq$. The first duopolist is then assumed to maximize:

$$\pi_1 - \pi_2 = p(q_1 + q_2)q_1 - C_1(q_1) - p(q_1 + q_2)q_2 + C_2(q_2).$$

Differentiating this equation partially with respect to q_1 and setting the result equal to zero, we obtain:

$$p - m_1 + \frac{dp}{dq}(q_1 - q_2) = 0.$$

Similarly, when the second duopolist maximizes $\pi_2 - \pi_1$,

$$p - m_2 + \frac{dp}{dq}(q_2 - q_1) = 0.$$

These two equations correspond to the duopolists' warfare-reaction schedules, such as the third branches of the R_1R_1 and R_2R_2 schedules in Figure 2.¹⁸ In general, moreover, since p , dp/dq , m_1 and m_2 are all functions of either or both q_1 and q_2 , these two reaction equations are presumptively sufficient to determine the two warfare-output unknowns, q_1 and q_2 .¹⁹

¹⁸ When a duopolist's warfare output is a constant, independent of his rival's output (as in that special case), it is always his monopoly output; for, when q_i is zero, the maximizing of $\pi_i - \pi_j$ is the same as maximizing π_i . This is always the case, moreover, whenever demand is linear and each duopolist's cost depends only on his own output. Thus, when $p = h - k(q_1 + q_2)$, such that

$$\pi_1 = hq_1 - kq_1^2 - kq_1q_2 - C_1(q_1)$$

and

$$\pi_2 = hq_2 - kq_2^2 - kq_1q_2 - C_2(q_2),$$

the only term involving both q_1 and q_2 in each of these profit functions drops out when one of them is subtracted from the other. Hence the maximizing of $\pi_i - \pi_j$ depends only on q_i .

¹⁹ When $\pi_1 - \pi_2$ is being maximized by one producer and minimized by the other, this type of warfare is in effect a two-person zero-sum game. At least with quantity-setting and ordinary demand and cost functions, moreover, the solution involves pure strategies, rather than the mixed strategies that are stressed in the general theory of two-person zero-sum games.

Furthermore, when both duopolists follow this warfare strategy, we may add the two preceding equations, to obtain this key implication of their warfare:

$$p = \frac{m_1 + m_2}{2}.$$

In short, this type of warfare implies that the price will be equal to the arithmetical mean of the duopolists' marginal costs.²⁰

Similarly, we may also discover the implied relationship between their warfare outputs by substituting the last equation in either of the two preceding ones:

$$q_1 - q_2 = \frac{1}{2} \frac{dq}{dp} (m_1 - m_2).$$

This tells us not only that the duopolists' warfare outputs will be the same when they have the same marginal costs, but also that the du-

²⁰ Although limitations of space force me to confine the present discussion to the case of duopoly, it might be extended to cases of three or more oligopolists. With n oligopolists, the present type of warfare would call for the i th man to maximize:

$$\pi_i - \frac{1}{n-1} (\pi_1 + \pi_2 + \dots + \pi_{i-1} + \pi_{i+1} + \dots + \pi_n).$$

In the special case where all of the oligopolists have constant and equal average costs, this warfare would again yield zero profits for all; and in general it is warfare of the type that corresponds to a symmetrical n -person zero-sum game, played noncooperatively—i.e., with coalitions effectively prohibited. In general, this type of warfare implies that $p = (m_1 + m_2 + \dots + m_n)/n$. With three or more oligopolists, on the other hand, the possible relevance of coalitions among some of the warring parties constitutes a major complication not present in the two-person game.

Product differentiation always softens the violence of this type of warfare. Thus, if each of n oligopolists has a demand-price function of the form $p_i = p_i(q_1, q_2, \dots, q_n)$, where $\partial p_i / \partial q_i < \partial p_i / \partial q_j < 0$, and a cost function of the form $C_i = C_i(q_i)$, the maximizing of the expression in the preceding paragraph with respect to q_i implies that:

$$p_i - m_i + q_i \frac{\partial p_i}{\partial q_i} - \frac{1}{n-1} \left(q_1 \frac{\partial p_i}{\partial q_1} + q_2 \frac{\partial p_i}{\partial q_2} + \dots + q_{i-1} \frac{\partial p_i}{\partial q_{i-1}} + q_{i+1} \frac{\partial p_i}{\partial q_{i+1}} + \dots + q_n \frac{\partial p_i}{\partial q_n} \right) = 0.$$

By adding these equations for all of the oligopolists, we may see that the sum of their prices exceeds the sum of their marginal costs in the warfare "equilibrium":

$$\sum p_i = \sum m_i - \sum q_i \left[\frac{\partial p_i}{\partial q_i} - \frac{1}{n-1} \left(\frac{\partial p_i}{\partial q_1} + \frac{\partial p_i}{\partial q_2} + \dots + \frac{\partial p_i}{\partial q_{i-1}} + \frac{\partial p_i}{\partial q_{i+1}} + \dots + \frac{\partial p_i}{\partial q_n} \right) \right].$$

In the special case where all of the oligopolists have the "same" demand and cost functions, the symmetrical warfare equilibrium implies that, for each duopolist:

$$p_i = m_i - q_i \left(\frac{\partial p_i}{\partial q_i} - \frac{\partial p_i}{\partial q_j} \right).$$

opolist with the lower marginal cost will produce the larger warfare output (since the slope of the demand curve is negative). Specifically, the difference in their outputs will be proportional to the difference in their marginal costs, the factor of proportionality being half the reciprocal of the demand curve's slope at the warfare point. The above relationships also show that the price and the outputs of the warring duopolists depend only on the product demand and their marginal costs.

The fact that the duopolists' warfare price equals the average of their marginal costs implies, of course, that it will equal the marginal cost of each producer whenever they both have the same cost curves.²¹ As in the earlier basic illustration, this also implies a warfare price equal to average cost whenever the latter is constant. When both duopolists have the same nonconstant average costs, however, their warfare price will be either greater or less than average cost, according as the latter is positively or negatively sloped. Similarly, when the duopolists have constant but unequal costs, their warfare price will be midway between their average costs; and the low-cost producer will make a positive profit per unit equal in magnitude to the negative profit per unit of the high-cost producer (on outputs that are unequal, however, in accordance with the last of the above equations).²²

The losses that this type of warfare would inflict on the high-cost producer in the latter case, and on both producers when they have the same (or not too different) schedules of decreasing average cost,

²¹ Just as this type of warfare happens to fulfill the purely competitive (more broadly, the welfare-efficiency) condition of price equal to each producer's marginal cost when they have the same cost curves, so does it entail a systematic social inefficiency when they have different costs. Specifically, the low-cost duopolist then produces too little (price exceeds his marginal cost) and the high-cost duopolist produces too much (price is less than his marginal cost).

It is no doubt surprising that a purely competitive equilibrium (with simple profit-maximizing by many rivals) may thus be identical to warfare between duopolists (with each maximizing the excess of his profit over his rival's). In pure competition, the inability of the individual competitor to effect the price also implies an inability to affect his rivals' profits. Hence maximizing his own profit is then the same as maximizing the excess of his profit over any weighted average of theirs.

²² Thus the present type of warfare may or may not be "predatory" or "cut-throat," in the sense of inflicting losses on either or both of the participants. Specifically, when they have the same schedules of constant or increasing costs, their warfare profits are either zero or positive, respectively; and it is then rather clearly in the social interest that such warfare continue. Conversely, when the duopolists have identical costs, warfare inflicts losses on both of them only when their average costs are decreasing; and it inflicts losses on just one of the decreasing-cost producers only when his costs are sufficiently higher than his rival's. In both of these cases, social efficiency requires only one active producer; so it is then at least questionable whether the duopolists' "cut-throat" competition should be discouraged as a matter of public policy. In these cases, even an unregulated monopoly equilibrium is more favorable to the general public than a duopoly solution with divided production anywhere on the duopolists' contract curve.

are noteworthy, especially in view of the casual assumption, frequently encountered in oligopoly discussions, that no one would be willing to suffer a loss "indefinitely." In opposition to that view it should be pointed out, first, that a firm may well be technically able to sustain such a loss. Thus, even a constant loss suffered to eternity has a finite negative present value when discounted at a positive rate of interest; so a firm need only have assets from which its annual return would be equal to its annual warfare loss in order to be at least *able* to withstand such warfare indefinitely. And if able, a "rational" duopolist may also be perfectly willing to stand such protracted losses—and in any event he will be eager to create the impression of such a willingness—if he has any reason to believe that he will thereby coerce his reluctant rival into an efficient collusion sufficiently more favorable to himself as compared with the pact that he could get immediately. In short, there is nothing sacred about the zero point that divides profits and losses. Whatever the algebraic sign of a duopolist's profit in a warfare situation, the fact remains that he is still receiving a profit that is less than what he could receive by surrendering. Hence, if it is rational to refuse to surrender when suffering merely a reduced positive profit, it need not be any less rational to make the same decision even when pushed by warfare below the point of zero return.²³

If only with respect to a possible inability to withstand prolonged losses, however, the difference between an outright loss and a merely reduced positive profit may then be decisive. For example, if a pair of duopolists with the same (or not too different) increasing average costs should both adopt the present war strategy, neither could ever drive the other entirely out of business, since even warfare would then leave both of them with positive profits. Hence, this is a case in which the relevant range of contract-curve solutions would indeed be narrower than the contract curve as a whole. When both duopolists have the same decreasing average costs, however, such that the present type of warfare would inflict a loss on each, an inability or reluctance of either to stand that loss would force him into at least a partial surrender.

²³ Moreover, even when a duopolist who would be forced to suffer losses during a war does not have sufficient assets of his own with which to bear those losses, he might still be able to persuade entirely rational outsiders to supply him with the extra equity capital that he would need—provided, of course, that he could find outsiders who shared his conviction that the mere threat of prolonged warfare would be sufficient to terminate it profitably. If this were not true, and if either or both of the duopolists had insufficient assets for indefinitely prolonged warfare, it might then be plausibly argued that the warfare could be rationally terminated by a negotiation in which the duopolists simply compared their assets in order to determine which one of them would be forced to surrender first. Such considerations, bearing on both the objective ability and the subjective willingness of duopolists to stand the costs of warfare, enter into the game-theory analysis under the heading of the duopolists' "utilities" of money.

Thus, as the timid duopolist contracted his output in an effort to escape his loss, he would thereby allow an even greater algebraic increase in the profit of his hardier rival; and, if the timid one was indeed able to get back to a zero profit before his output was reduced to zero, his rival would thereupon be making a necessarily positive profit. This follows, of course, because the duopolist with the higher output and the lower unit cost would then be selling it at the same price that enabled the timid duopolist to break even. On the other hand, if the timid duopolist could not cover his average cost at any positive output when his rival produced his monopoly output, a reluctance to suffer any warfare loss would then require a permanent exit from production.²⁴

On the other hand, if both duopolists seem to be both able and willing to stand the costs of war indefinitely, and more specifically if they can convince one another of their mutual stubbornness, then the present warfare hypothesis also provides some possible clues as to the terms on which "reasonable" men might be willing to terminate that warfare, or perhaps avoid it altogether. Thus, since each duopolist experiences a certain profit or loss at the warfare point, a fairly obvious arbitration rule might be to move to that point on the collusion locus that would give the duopolists equal increments of profit. When the duopolists have the same costs, of course, this would yield the symmetrical collusive solution—with either an equal sharing of the jointly maximized profit if side payments are permitted, or an equal sharing of profits at the midpoint of the contract curve if side payments are either prohibited or unnecessary. When the duopolists have different costs, the same rule has less trivial, though equally definite, results. Then the duopolists, having presumably different profits at their warfare point, would maintain that same profit differential in moving to the relevant collusion locus.²⁵

²⁴ This case, incidentally, is one that Bain would classify as "blockaded" entry, where the monopolist maximizes his profit at a price below the level that would invite entry, at least to the extent that Bain (like Cournot) hypothesizes that the would-be entrant would assume an existing producer's output to be a constant in the face of attempted entry [1, esp. pp. 22 and 98-99]. Bain's conclusion, however, is not general. In the present case, there is no inherent reason why a sufficiently determined rival might not attempt to force even an entrenched monopolist to share the market with him at a price above the monopoly level; and, what is more, there is no inherent reason why he might not succeed. It should be realized that a collusive duopoly price is always above the monopoly price of at least the low-cost producer when both duopolists produce at decreasing marginal cost.

²⁵ On the other hand, especially if there are wide disparities in the duopolists' costs and side payments are prohibited, it may be impossible for the high-cost producer to avoid a loss at the collusion point in question. In that event, the present arbitration rule would call for his permanent exit from the industry, which is not necessarily implausible in the light of his strong cost disadvantage. With side payments permissible, however, the same duopolist might qualify for a positive profit by that route. Finally, a really embarrassing case for the present arbitration rule would be one in which both duopolists have similar

This type of movement from the warfare point to the collusion locus would strictly justify the present type of warfare strategy. With each man's collusion and warfare profits denoted by the superscripts c and w , respectively, the suggested arbitration rule implies that $\pi_1^c - \pi_1^w = \pi_2^c - \pi_2^w$ or, equivalently, $\pi_1^c - \pi_2^c = \pi_1^w - \pi_2^w$. Accordingly, since the duopolists have squarely conflicting interests as to $\pi_1^c - \pi_2^c$, the same conflict applies to $\pi_1^w - \pi_2^w$. In short, if the duopolists first play at least a hypothetical warfare game in which the first maximizes $\pi_1^w - \pi_2^w$ and the second minimizes that magnitude, each will thereby attain his best possible position for the anticipated movement to the collusion locus. In other words, if the first duopolist should fail to maximize $\pi_1^w - \pi_2^w$, or if the second should fail to minimize that magnitude, the deviant strategist could only reduce the collusive profit that he ultimately receives.²⁶

Other similar arbitration "rules" can also be devised. For example, the duopolists, in moving from their warfare point to their collusion locus, might agree to keep constant the absolute difference, or perhaps the ratio, between their *outputs*; or they might ignore the consequences of warfare and settle for maintaining the absolute difference, or again the ratio, between either the profits or the outputs that would be implied in their respective equilibria as monopolists.²⁷

decreasing costs so nearly tangent to industry demand that it is impossible for both to produce at a profit simultaneously. This difficulty could be resolved if the duopolists could employ some chance mechanism to determine which of them should be the sole active producer in any given period (as Nash specifies); or it might be resolvable in a recurrent market even by tacit agreement if it is feasible for each duopolist to be the sole active producer some appropriate fraction of the time (see above, n. 15).

²⁶ This solution is identical to Nash's in special cases, notably when the slope of the profit frontier at the collusion point is $d\pi_2^c/d\pi_1^c = -1$ and both duopolists' utilities are linear functions of their profits. (This is illustrated in the side-payments case in [7].) In general, substituting utilities for profits in the above notation, Nash specifies a linear path from the threat (or warfare) point to the collusion point with a slope of $(u_2^c - u_2^w)/u_1^c - u_1^w = a$, where the slope of the utility frontier at the collusion point is $du_2^c/du_1^c = -a$. (For Nash's rationalization of this, see [8] and [10].) Hence, since the duopolists have conflicting interests as to $au_1^c - u_2^c$, this implies a similar conflict as to $au_1^w - u_2^w$. The latter, accordingly, is the magnitude maximized by the first man and minimized by the second in the hypothetical warfare (or threat) game, as a preliminary to the indicated linear movement to the collusion point.

²⁷ I refrain from suggesting that the duopolists might reproduce on the collusion locus the same ratio of profits as at the warfare point, for the obvious reason that either or both of the duopolists may well have negative profits during warfare.

It may also be at least provocative to point out that, in an example which I have used in my classes, involving linear demand and constant but unequal costs, *all* of the absolute-difference criteria mentioned here happen to be simultaneously satisfied. With the demand $p = 20 - q$ and the respective unit costs of $c_1 = 4$ and $c_2 = 6$, the duopolists' monopoly and warfare outputs are alike $q_1 = 8$ and $q_2 = 7$ (with a difference of 1); and their monopoly profits are $\pi_1 = 64$ and $\pi_2 = 49$, as compared to warfare profits of $\pi_1 = 8$ and $\pi_2 = -7$ (each reflecting an algebraic difference of 15). It then follows that any one of the absolute-difference rules suggested here would lead the duopolists to the

Similarly, the particular warfare hypothesis that I have been considering is also subject to variations. It is relevant only when each warring duopolist is consistently willing to reduce his own profit by a dollar whenever he can thereby reduce his rival's profit by a greater amount. On the other hand, if either duopolist is either more or less aggressive than this, his warfare-reaction schedule will be different. For example, suppose that the first warring duopolist is consistently willing to reduce his own profit by a_1 times a dollar whenever this would reduce his rival's profit by more than a dollar. He would then be more or less aggressive than he was on the initial hypothesis according as a_1 is greater or smaller than 1. Moreover, this would imply a policy of maximizing $\pi_1 - a_1\pi_2$. Then, as a_1 approached zero, this would reflect a growing timidity or distaste for warfare; and, if a_1 should equal zero, this would represent the extremely timid, though not entirely pacifist, policy of simply maximizing his own profit, without regard to the effect on that of his rival. Toward the other extreme, as a_1 rose above 1, this would reflect a growing aggressiveness or vindictiveness; and, if a_1 were to approach infinity, this would represent an approach to a policy of unlimited war, in which the warring party was willing (and able) to suffer any loss to himself, no matter how great, in order to inflict even the slightest damage on his opponent. In the limit, in short, this would reflect a policy of minimizing his rival's π_2 , without any regard whatever for the effect on his own π_1 . Needless to say, high values of a_1 become increasingly implausible, if only because of the limited actual and potential resources of any warring party. Conversely, the corre-

point on their contract curve where $q_1 = 4\frac{1}{4}$ and $q_2 = 3\frac{1}{4}$ (with a difference of 1), and where $\pi_1 = 36\frac{7}{8}$ and $\pi_2 = 21\frac{1}{8}$ (with a difference of 15). Moreover, this is the only point on the contract curve, other than the monopoly points at either end, where both duopolists' outputs and profits happen to be rational numbers. Finally, the sum of the duopolists' outputs at this point on their contract curve happens to equal just half the sum of their warfare outputs, and likewise the simple average of their separate monopoly outputs. Hence the price to consumers ($p = 12\frac{1}{2}$) is also the simple average of the duopolists' respective monopoly prices ($p = 12$ and $p = 13$). I realize that all of these implications are the consequence of the pervasive linearities of the example; but it is difficult to resist the temptation to become at least somewhat mystical in the presence of so many surprising coincidences.

As at least a cautioning noncoincidence, however, Nash's more subtle logic gives a different result, more favorable to the low-cost duopolist. When side payments are permitted, on the other hand, his reasoning and mine do lead to the same conclusion: the low-cost producer's monopoly profit of 64 is divided into the shares, $\pi_1 = 39\frac{1}{2}$ and $\pi_2 = 24\frac{1}{2}$ (with a difference of 15). In Nash's analysis, there is a consistent and perhaps universal implication that is quite paradoxical: the low-cost producer seems always to get a lower absolute (as well as relative) profit when side payments are permitted, as compared to his profit when they are prohibited. The introduction of the mere possibility of side payments so enhances the bargaining power of the high-cost producer, so to speak, that the low-cost producer is made worse off by the very rule change that allows the joint profit of the two together to be increased to its maximum. On my alternative hypothesis, both duopolists share equally in the extra profit that the side payments allow.

sponding range of alternative warfare policies for the second duopolist would consist of his seeking to maximize $\pi_2 - a_2\pi_1$.

It will be sufficient to illustrate the implications of various values of a_1 and a_2 with reference to our initial example of linear demand and constant and equal costs. In that example,

$$\pi_1 - a_1\pi_2 = 24q_1 - q_1^2 - (1 - a_1)q_1q_2 - 24a_1q_2 + a_1q_2^2.$$

This expression is maximized with respect to q_1 when

$$q_1 = 12 + \frac{1}{2}(a_1 - 1)q_2.$$

Similarly, when the second duopolist maximizes $\pi_2 - a_2\pi_1$,

$$q_2 = 12 + \frac{1}{2}(a_2 - 1)q_1.$$

For any values of the constants a_1 and a_2 , both of these equations are linear. Furthermore, they would correspond to the third branches of the three-segmented reaction schedules that were labeled R_1R_1 and R_2R_2 , respectively, in Figure 2. Thus the third branches (where $q_1 = 12$ and $q_2 = 12$, respectively) correspond to our present equations when a_1 and a_2 are both equal to 1. When those constants are either greater or less than 1, however, the third branches of R_1R_1 and R_2R_2 in Figure 2 would have to be replaced with lines that would be other than perpendicular to the respective axes; and they would thus have different points of juncture with the respective second branches of R_1R_1 and R_2R_2 .

From the standpoint of the first duopolist, the various W_1 schedules that are shown in Figure 3 illustrate the implications of various values of a_1 . In particular, the superscripts of these alternative W_1 schedules reflect the alternatively assumed values of a_1 . Thus, when $a_1 = 1$, the vertical schedule W_1^1 corresponds to the third branch of R_1R_1 as actually depicted in Figure 2. When $a_1 = \frac{1}{2}$, by contrast, the schedule $W_1^{\frac{1}{2}}$ then reflects a more timid warring attitude—and an earlier and lower limit on the warring propensity that is expressed in the second branch of the schedule R_1R_1 in Figure 2. Even when $a_1 = 0$, however, some warring element still remains in the reactions of the first duopolist; for, as we may see in Figure 3, the schedule W_1^0 remains steeper than the contract line (Ct). Indeed, since W_1^0 reflects the policy of maximizing just π_1 for any given value of q_2 , it is the same as this seller's Cournot-reaction schedule. Thus the first duopolist does not necessarily surrender to whatever demand for a contract-curve solution his rival may be asserting against him, even when he timidly sets

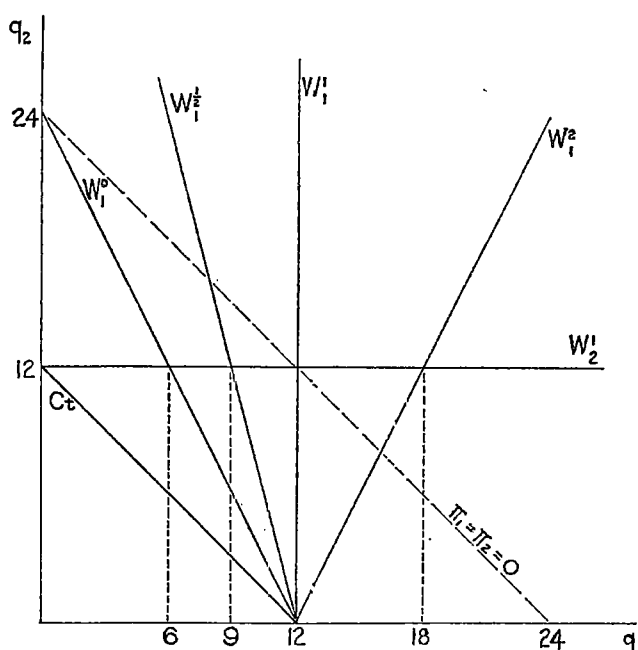


FIGURE 3

$a_1 = 0$.²⁸ Finally, the schedule W_1^2 illustrates the more aggressive war-
ring attitude that would be implied by $a_1 = 2$.

The only warfare-reaction schedule of the second duopolist actually illustrated in Figure 3 is W_2^1 , corresponding to the value of $a_2 = 1$. The points where this schedule intersects the various W_1 schedules thus illustrate the contrasting consequences that the different a_1 values may have. In particular, when $a_2 = 1$ and $a_1 < 1$, both duopolists make positive profits even in the midst of their warfare, but the more timid first duopolist makes a smaller profit than his rival. In this sense, the selection of $a_1 < 1$ is the equivalent of a partial surrender, at least by contrast with $a_1 = 1$; yet it is also a not implausible type of decision when a man is not willing to surrender completely but still wants to limit his warfare losses. When $a_2 = 1$ and $a_1 > 1$, however, both duopolists make losses, and the more aggressive first duopolist makes

²⁸ If the second duopolist's reaction schedule is R_2R_2 in Figure 2, and if the first duopolist's R_1R_1 schedule in that diagram is "cut off," so to speak, by an alternative third branch corresponding to W_1^0 in Figure 3, the implied warfare "equilibrium" would happen to occur at the juncture of the second and third branches of R_1R_1 —where $q_1 = 6$, $q_2 = 12$, $\pi_1 = 36$, and $\pi_2 = 72$. Thus even the timidity that is implied by $a_1 = 0$ does not necessarily imply acceptance of the second duopolist's proposed collusive equilibrium at the point C_2 in Figure 2—where $q_1 = 4$, $q_2 = 8$, $\pi_1 = 48$, and $\pi_2 = 96$. In other words, a surrender to that demand of the second duopolist requires a separate decision, irrespective of the a_1 value that the first duopolist might select as the limit on his own propensity to wage war.

the bigger loss. It is hard to see, therefore, what he might be seeking to gain by this ultra-aggressive strategy. To be sure, he might be adopting it in an effort to emphasize his insistence on being allowed to produce the larger output, even though it is possible to make that demand without standing ready to incur losses larger than those of one's rival.²⁹ Or he might be interpreted as trying to exhaust his rival's limited assets the more quickly, even though (at least in the present case of equal and constant costs) his rival can always protect himself against losses if he is so inclined, without allowing the more aggressive strategist to make any profit.³⁰ At least in the present case, therefore, I conclude that, even though values of $a_1 < 1$ may well be plausible, values of $a_1 > 1$ are not, except possibly in conjunction with values of $a_2 < 1$.³¹ In cases of other than constant and equal costs, however, there may well be more plausible scope for the more aggressive strategies.

Finally, if each duopolist's "marginal propensity for warfare" varies with his own and his rival's profit, a_1 and a_2 would no longer be con-

²⁹ Compare, for example, the attitude implicit in the R_2R_2 schedule in Figure 2.

³⁰ Consider, for example, the above-mentioned strategy (n. 5) of limiting one's warfare simply at the locus where $\pi_1 = \pi_2 = 0$. In terms of the present warfare-reaction schedules, both duopolists will also make zero profits whenever $a_1a_2 = 1$; for then the W_1 and W_2 schedules intersect at a point on the zero-profit locus.

When $a_2 = 1/a_1$, the second duopolist's warfare policy of maximizing $\pi_2 - a_2\pi_1$ is the equivalent of minimizing $\pi_1 - a_1\pi_2$, which his rival is seeking to maximize. Hence this is the general case of warfare as a two-person zero-sum game. It is also the type of hypothetical warfare implied by Nash's analysis when both duopolists' utilities are linear functions of their profits (see above, n. 25).

When $a_1 = a_2 = 1$, it was shown above (pp. 950-51) that warfare implied $p = (m_1 + m_2)/2$. More generally, when $a_2 = 1/a_1$, it can be similarly shown that $p = (m_1 + a_1m_2)/(1 + a_1)$; that is, price is a weighted average of the duopolists' marginal costs, approaching m_1 as a_1 declines toward zero.

In the corresponding zero-sum warfare among n oligopolists (with no coalitions—cf. above, n. 20), the first man maximizes:

$$\pi_1 - \frac{1}{n-1} (a_{12}\pi_2 + a_{13}\pi_3 + \cdots + a_{1n}\pi_n);$$

the second maximizes:

$$a_{12}\pi_2 - \frac{1}{n-1} (\pi_1 + a_{13}\pi_3 + \cdots + a_{1n}\pi_n);$$

and so on. The price implied by this warfare is again a weighted average of all the oligopolists' marginal costs, specifically:

$$p = \frac{m_1 + a_{12}m_2 + a_{13}m_3 + \cdots + a_{1n}m_n}{1 + a_{12} + a_{13} + \cdots + a_{1n}}.$$

³¹ As implied in the preceding footnote, both duopolists' profits are positive when $a_1a_2 < 1$. Therefore, not only may the first duopolist (for example) protect himself against a loss by reducing a_1 when $a_2 > 1$, but he may also force his rival (and himself) to make a zero profit by increasing a_1 sufficiently when $a_2 < 1$. Thus it is only when $a_2 \geq 1$ that values of $a_1 > 1$ seem rather pointless. In view of this conclusion, it is only a matter of academic interest that, were both duopolists ultra-aggressive (with a_1 and a_2 both greater than 1), there might not even be a determinate warfare "equilibrium." That is, the W_1 and W_2 schedules intersect only when $(a_1 - 1)(a_2 - 1) < 4$.

stants, but functions of π_1 and π_2 ; and the warfare-reaction schedules analogous to W_1 and W_2 in Figure 3 would then become curvilinear. Even in that event, however, there would presumably still be a definite warfare "equilibrium," with definite equilibrium values of a_1 and a_2 ; and it would therefore still be possible to calibrate the degrees of aggressiveness on both sides in terms of those values.

IV. *Conclusion*

In this paper I have first discussed and illustrated, in Section I, a method by which each duopolist may convey to the other both his demand for a certain collusive profit and the threat of the warfare he would be prepared to wage, if necessary, to enforce that demand. The central concept in that exposition is a reaction schedule whose three segments successively reflect (1) the collusive equilibrium proposed, (2) a warring response if that offer is refused, and (3) a limiting influence on the severity of the warfare. Although the discussion was primarily based on an assumed absence of verbal communication, the same elements would inevitably be involved in explicit negotiation as well. In general, the outcome may be collusion on some set of terms within a wide spectrum of possible ones or, if the duopolists persist in mutually incompatible demands, a state of warfare. Although the basic exposition concerned a homogeneous product produced at constant and equal costs, the technique is applicable with differentiated products as well and under various cost conditions, some of the distinctive implications of which were indicated in Section II.

When the duopolists are unable to agree on a collusive solution, the possible types of warfare that may then be waged were analyzed in Section III. The central hypothesis proposed was that $\pi_1 - \pi_2$ might plausibly be maximized by the first warring duopolist and minimized by the second. The consequences of this type of warfare were then spelled out for a wide variety of demand and cost conditions. As its key implication, the warfare price is equal to the arithmetical mean of the duopolists' marginal costs when their products are homogeneous, and it is higher than that when their products are differentiated. This type of warfare is strictly justified from the point of view of both parties if the warfare is then settled by a movement to the collusion locus yielding the duopolists equal increments of profit.

A more general framework for the analysis of warfare was also presented. Thus, if the i th duopolist is willing to reduce his own profit by a_i times a dollar whenever this would reduce his rival's profit by more than a dollar, his warfare policy then implies a maximizing of $\pi_i - a_i\pi_j$. His warfare is then more or less aggressive than in the

initial case according as a_i is greater or less than one. It would further seem that a_i is more likely to be less than one rather than greater.

My central hypothesis as to "plausible" warfare and a "reasonable" formula for settling or avoiding it cannot be established as empirically relevant, of course, on the basis of purely logical speculation. It is a hypothesis that can be tested, however, both in "games" that simulate duopoly and conceivably in real markets as well. Even if this hypothesis proves to be wide of the mark, the framework suggested here will remain useful for calibrating observed behavior both in states of actual warfare and in the agreements by which warfare is settled or avoided.

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ANNUAL AND LIFETIME INCOME IN RELATION TO EDUCATION: 1939-1959

By HERMAN P. MILLER*

Nearly one-quarter of a century has elapsed since Harold F. Clark and his colleagues produced their pioneer study on life earnings in selected occupations [5]. Clark expressed the hope that his rough procedures would be improved upon with time and that the figures would be recalculated at least annually. Aside from a relatively few attempts, however, the challenge has not been taken up by contemporary economists or statisticians despite an increased need for such information. In part, this neglect must be attributed to a lack of data. Although a vast amount of data can be found on hourly, daily, or weekly wages for many skilled trades, information on annual earnings, which are used as a basis for computing lifetime earnings, is still quite scarce. The picture has changed somewhat as a result of the past two decennial censuses and the annual income surveys conducted by the Bureau of the Census since 1945. It is the purpose of this study to examine the relationship between income and education as revealed in these data. The first two sections consider the findings with regard to annual income and the third section presents some newly developed data on lifetime income for men with different amounts of schooling.

Although the material gains of an education have been selected for study, the intent has not been to slur the more subtle satisfactions that come with greater educational attainment. The cultural and social advantages associated with more schooling may well be worth their cost in time, money, and effort, even if the economic advantages should cease to exist. The only justification for focusing on the economic advantages is that at present they are the only ones capable of even approximate measurement.

Since the present study makes no allowance for the individual and social costs incurred in the completion of additional schooling, the income gains associated with greater educational attainment, as shown in this report, are overstated. Even if allowance were made for these costs,

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however, the evidence available from recent studies suggests that an investment in schooling pays, on the average, a better return than most other investments.¹

There is one further caution to be noted. Although the figures show that, on the average, there is a monetary return to the individual for an investment in education, there is no guarantee that such an investment will earn this rate of return in any given case. This fact seems obvious, but it is often ignored. In 1958, for example, about 2.7 million men with college degrees had incomes under \$7,000 whereas 1.9 million high school graduates received more than this amount [21]. How can we explain the relatively low incomes of so many highly educated men and the higher incomes of so many men with relatively little education? The answer lies partly in response errors which abound in the reporting of income and education in household surveys and censuses;² but the differences cannot entirely be swept under the rug by attributing them to errors in the statistics. The major part of the explanation must be sought in differences in the quality of education, the abilities and efforts of individuals, and many other forces that impinge on the observed relationship between income and education. Many intelligent individuals never get as much schooling as they should, and too many individuals with relatively low intelligence get more schooling than they should. Training completed at inferior schools cannot be equated with equal amounts of time spent in training at excellent schools. For these and many other reasons it would be fallacious and perhaps even harmful to draw inferences about individual cases from the evidence presented here for the general population.

I. Annual Income in Relation to Education: 1939-1958

Numerous studies, conducted under varying economic conditions, have shown that persons with more schooling tend to earn more money. This relationship seems reasonable if it is assumed that the attainment of more schooling, particularly at the secondary school and college level, in some measure improves the productivity of the individual and thereby compensates for the investment of time, effort, and money. On the other hand, it is by no means inevitable that money invested in education will necessarily pay dividends or that the rate of return will

¹ This conclusion is based largely on [16]. See also [1] and [12].

² There is evidence that when a given set of questions on income or education is asked for an identical group of persons in surveys conducted several weeks apart, there are marked variations in the replies that are received. These variations contain overestimates as well as underestimates and they tend to cancel each other, leaving the resultant distributions unchanged. Although variability of response does not appear to create serious problems in one-dimensional distributions such as of income alone, it does tend to produce distortions when cross-classifications are considered.

be constant over time. There is always the possibility, indeed the probability, that the higher incomes of those with more years of schooling are due in part to differences in intelligence, home environment, family connections, and other factors which result from individual differences in ability and opportunity. Therefore, to some extent, the observed relationship between schooling and earnings may be a spurious one. It is, of course, difficult if at all possible to measure the extent to which these extraneous factors enter this relationship. There is, however, some evidence that "ability" as measured by scholastic achievement is highly correlated with earnings.³

Economists have also long argued that earnings differentials could be reduced by an increase in education. Von Thünen was one of the earliest proponents of the use of educational policy as a means of reducing income differentials. In 1826 he asked "why in a competitive organization the incomes of manual workers remained persistently so far below the incomes of manufacturers and farmers." His explanation was that "manual workers were lacking in the elements of school knowledge without which, in spite of any other qualifications, it was impossible to be an entrepreneur" [7, pp. 745-46]. In 1887, Marshall also saw in education the beneficent possibility for narrowing wage differentials.⁴ More recently, Seymour Harris, noting the rapid rise in the extension of higher education, has expressed concern about the possibility that the persistent increase in the supply of college-trained workers will so flood the market that "college students within the next twenty years are doomed to disappointment after graduation, as the number of coveted openings will be substantially less than the numbers seeking them." [11, p. 64]. The same concern has been expressed by several noted educators including James B. Conant [6, p. 198].

During a relatively short period, such as that considered in the present report, the tendency for education to result in a reduction of income differentials could be more than offset by an increase in the demand for the services of skilled workers due to technological changes in the economy. Since the supply of skilled workers, particularly those with college training, has increased considerably during the past generation, we shall attempt to determine to what extent the increase in the demand for their services has offset the tendency for their incomes to increase proportionately less than other workers.

³This conclusion is based on unpublished data underlying [3, pp. 175-97]. See also [4, pp 1-19]: Preliminary findings from a current study in the Bell System tend to confirm the findings in [4].

⁴"The normal earnings of a carpenter and surveyor might be brought much nearer together than they are, by even so slight and easy an improvement on our present social arrangements as the extending to all persons of adequate natural ability the opportunity of receiving the training required for the higher ranks of industry." [15, p. 214].

Some of the basic statistics pertaining to the relationship between annual income and educational attainment are presented in Table 1, which shows the variations in average (mean) annual income over the past generation for men with different amounts of schooling.⁵ The data are presented separately for each age group, as well as for all men 25 years old and over, in order to permit an examination of the figures without having to take account of changes in the age distribution of the population. Women have been excluded from the analysis; since a large proportion of them do not enter the labor market and many of those who do are employed on a part-time basis only, the relationship between their income and education may be distorted. In contrast, practically all adult men are full-time workers and it can therefore be assumed that any advantages which may accrue from more schooling are reflected in their incomes.

Table 1 shows that in every year for which data are presented the completion of an additional level of schooling was associated with higher average incomes for men. This finding parallels that obtained in numerous other studies of the relationship between education and income dating back to the early part of this century [19, p. 115]. Although the income levels have changed considerably during the past 20 years, the basic relationship between the extent of schooling and income appears to have remained much the same. Contrary to the expectations of some analysts, the economic advantages accruing from the completion of additional years of schooling have not diminished in recent years.

Although income generally tends to increase with education, Table 1 shows that a year spent in completing a given level of schooling (e.g., the fourth year in high school) yields a greater return than any of the years leading up to graduation. This difference may reflect a selection in terms of ability between those who do and those who do not complete their schooling. Thus in 1958, men who started high school but did not graduate, received on the average an annual income of about \$400 more per year of schooling than men who completed their schooling with graduation from elementary school. High school graduates, however, received about \$500 more of annual income per year of school-

⁵ For each year, the mean income was obtained as a summation of the product of the average income and the proportion of persons for each income level. For income levels below \$10,000 in 1949, 1956, and 1958, below \$6,000 for 1946, and below \$5,000 for 1939, the midpoint of each class interval was assumed to be the average. For 1949, 1956, and 1958, \$20,000 was used for the "\$10,000 and over" interval; for 1946, \$12,000 was used for the "\$6,000 and over" interval; and for 1939, \$9,000 was used for the "\$5,000 and over" interval. Medians corresponding to the means shown in Table 1 may be obtained from the author. Tax return data for recent years suggest a drop in the average for the open-end interval. An alternative calculation made for 1958, using a mean of \$17,000 for the "tail," revealed no substantial changes in the relationships.

TABLE 1—MEAN INCOME (OR EARNINGS) FOR MALES 25 YEARS OF AGE AND OVER, BY YEARS OF SCHOOL COMPLETED AND AGE: 1939, 1946, 1949, 1956, AND 1958

Years of School Completed and Age	1939 ^a	1946 ^b	1949 ^c	1956 ^c	1958 ^c
Total: 25 Years Old and Over:					
Elementary: Total	\$1,036	\$2,041	\$2,394	\$3,107	\$3,096
Less than 8 years ^d	(e)	1,738	2,062	2,613	2,551
8 years	(e)	2,327	2,829	3,732	3,769
High School: 1 to 3 years	1,379	2,449	3,226	4,480	4,618
4 years	1,661	2,939	3,784	5,439	5,567
College: 1 to 3 years	1,931	3,654	4,423	6,363	6,966
4 years or more	2,607	4,527	6,179	8,490	9,206
25 to 34 Years:					
Elementary: Total	837	1,729	2,185	3,061	3,143
Less than 8 years ^d	(e)	1,394	1,880	2,662	2,670
8 years	(e)	2,011	2,540	3,685	3,663
High School: 1 to 3 years	1,150	2,062	2,837	4,407	4,341
4 years	1,335	2,335	3,246	4,813	4,909
College: 1 to 3 years	1,566	2,875	3,444	5,437	5,774
4 years or more	1,956	3,237	4,122	6,307	7,152
35 to 44 Years:					
Elementary: Total	1,110	2,095	2,610	3,694	3,686
Less than 8 years ^d	(e)	1,730	2,244	3,169	3,023
8 years	(e)	2,425	3,029	4,256	4,403
High School: 1 to 3 years	1,574	2,607	3,449	4,799	5,035
4 years	1,979	3,463	4,055	5,992	6,007
College: 1 to 3 years	2,270	4,069	5,014	7,131	8,015
4 years or more	3,141	5,054	7,085	9,790	10,106
45 to 54 Years:					
Elementary: Total	1,199	2,349	2,797	3,672	3,660
Less than 8 years ^d	(e)	2,027	2,418	3,078	3,008
8 years	(e)	2,629	3,247	4,289	4,337
High School: 1 to 3 years	1,732	2,959	3,725	4,876	4,864
4 years	2,256	3,744	4,689	6,104	6,295
College: 1 to 3 years	2,428	4,671	5,639	7,426	8,682
4 years or more	3,575	5,242	8,116	11,702	12,269
55 to 64 Years:					
Elementary: Total	1,057	2,082	2,577	3,462	3,436
Less than 8 years ^d	(e)	1,314	2,278	2,922	2,956
8 years	(e)	2,365	3,010	3,932	3,960
High School: 1 to 3 years	1,551	2,648	3,496	4,398	5,034
4 years	2,104	3,179	4,548	5,920	6,510
College: 1 to 3 years	2,065	3,838	5,162	6,677	6,992
4 years or more	3,247	5,461	7,655	9,595	10,966
65 Years Old and Over:					
Elementary: Total	(e)	1,541	1,560	1,875	1,903
Less than 8 years ^d	(e)	1,434	1,366	1,686	1,672
8 years	(e)	1,670	1,898	2,247	2,337
High School: 1 to 3 years	(e)	1,894	2,379	2,560	2,661
4 years	(e)	2,601	3,115	3,314	3,036
College: 1 to 3 years	(e)	2,720	3,435	4,269 ^t	4,180
4 years or more	(e)	3,902	5,421	5,835	6,091

(Notes and sources are on following page.)

ing than men who started high school but never graduated. Similarly, men who attended college but did not graduate had, on the average, about \$700 more per year of schooling than high school graduates. The comparable differential for college graduates was about \$900 per year of schooling.^a

The educational attainment of the population has grown considerably during the past generation. The proportion of college graduates has nearly doubled during the period and the proportion of high school graduates has also risen dramatically (Table 2). How has this change in the relative supply of more highly educated workers affected income differentials? Have the incomes of college graduates, relative to other groups in the population, been pushed down because of the relative increase in their numbers or has the demand for their services increased sufficiently to offset any tendency for their incomes to be lowered?

Although these questions cannot be answered categorically, there is

^a Restricted to persons reporting \$1 or more of wage or salary income and less than \$50 of other income for native white and Negro males 25 to 64 years old only.

^b Total money earnings.

^c Total money income.

^d Includes persons reporting no years of school completed, not shown separately.

^e Not available.

^f Base is less than 100 sample cases.

Source: Data for 1939 derived from *1940 Census of Population, Education: Educational Attainment by Economic Characteristics and Marital Status*, Tables 29 and 31. Data for 1949 derived from *1950 Census of Population*, Ser. P-E, No. 5B, *Education*, Tables 12 and 13. Data for 1946, 1956, and 1958 derived from the consumer income supplements to the April 1947, March 1957, and March 1959 *Current Population Survey*.

Note regarding comparability of the figures: Neither the income concept nor the universe covered is directly comparable for all the years shown. Most of the differences, however, are relatively small and are not believed to seriously distort the relationships. Thus, for example, the figures for 1956 and 1958 are entirely comparable since they are based on the Current Population Survey and represent the total money income of the civilian noninstitutional male population 25 years old and over. The 1949 figures are based on the 1950 Census and also represent the total money income of all males 25 years old and over, including a relatively small number of institutional inmates. The 1946 figures are based on the Current Population Survey and represent the total money earnings (not total income) of the civilian noninstitutional male population 25 years old and over. Although the conceptual differences between income and earnings are substantial, the actual differences in the averages are quite small, primarily because the amount of nonearned income is small relative to the total and this type of income tends to be seriously underreported in household surveys of income. The figures for 1939 are based on the 1940 Census and are restricted to males 25-64 years of age with \$1 or more of wage or salary income and less than \$50 of nonwage income. For this group, of course, the averages represent total money income; however, the universe has been restricted, because of the way in which the data were collected, to those persons who received only wage or salary income. Only about three-fifths of all men 25-64 years old in 1940 were in this category. The effects of this restriction cannot be measured, but it is undoubtedly more important than restrictions cited for other years. It is also possible that this restriction affects college graduates more than persons with less schooling and for them tends to create an adverse selection since college graduates are more likely to have income other than earnings.

^g For similar findings based on earlier data see [10].

some evidence that elementary school graduates have had smaller relative income gains than high school graduates, despite the reduction in their relative numbers. In contrast, the income differential between high school and college graduates has remained fairly constant over time and there is even some evidence that it has increased in favor of college graduates during the past few years (Table 3).

In the absence of 1939 income data for elementary school graduates, comparisons between the incomes of elementary and high school graduates must be restricted to the period since 1946. If attention is focused

TABLE 2—PER CENT DISTRIBUTION BY YEARS OF SCHOOL COMPLETED
FOR MALES 25 YEARS OLD AND OVER, FOR THE UNITED STATES:
1940, 1947, 1950, 1957, AND 1959

Years of School Completed	1940	1947	1950	1957	1959
Total	100	100	100	100	100
Elementary School: Total	62	51	49	42	39
Less than 8 years ^a	34	(b)	28	23	22
8 years	28	(b)	21	18	17
High School: 1 to 3 years	14	16	16	17	18
4 years	12	18	18	22	23
College: 1 to 3 years	5	7	7	7	8
4 years or more	5	6	7	9	10
Not reported	2	2	3	2	2

^a Includes persons reporting no years of school completed, not shown separately.

^b Not available.

Source: Data for 1940 derived from *1940 Census of Population: Pt. 1, Vol. IV, Characteristics by Age*, Table 18. Data for 1950 derived from *1950 Census of Population*, Ser. P-E, No. 5B, *Education*, Table 12. Data for 1947, 1957, and 1959 derived from the educational attainment supplements to the April 1947, March 1957, and March 1959 *Current Population Survey*, P-20, No. 15, Table 1; P-20, No. 77, Table 1; and P-20, No. 99, Table 1.

on these years, it is evident that the incomes of high school graduates have risen considerably more, in percentage terms, than those of elementary school graduates. In 1946, the differential between these two groups was only \$600 or about 26 per cent. By 1958, the differential rose to about \$1,800 or 48 per cent. This change is in part related to the fact that a large proportion of the elementary school graduates are employed in occupations such as farmers, farm laborers, and nonfarm laborers which tended to have lower relative income gains in recent years than most other occupations.⁷ It is also possible, of course, that even for occupations such as operatives and craftsmen, in which a

⁷ [21, Table E] shows that median total money income of employed males increased between 1950 and 1958 by 3 per cent for farm laborers, 51 per cent for nonfarm laborers, and 27 per cent for farmers. In contrast the median income during this period increased by 57 per cent for professional workers, 54 per cent for managers and officials, and 55 per cent for craftsmen.

relatively large number of elementary school graduates are employed, high school graduates received relatively greater increases than persons who never attended high school. There is also a possibility that the reduction in the relative number of elementary school graduates reflects a constant transfer of the "cream" of that group to the high school group, so that the average elementary school graduate in 1958 may have been a less "able" person than in 1946; but there is no objective evidence on this point.

In contrast to the changing relationship between the incomes of ele-

TABLE 3—MEAN INCOME (OR EARNINGS) BY LEVEL OF SCHOOL COMPLETED,
FOR MALES 25 YEARS OLD AND OVER, FOR THE UNITED STATES:
1939, 1946, 1949, 1956, AND 1958

Year	Elementary-High School Differential			High School-College Differential		
	Average Income		Per Cent Difference	Average Income		Per Cent Difference
	Elementary School Graduate	High School Graduate		High School Graduate	College Graduate	
1939	(a)	\$1,661	(a)	\$1,661	\$2,607	57
1946	\$2,327	2,939	26	2,939	4,527	54
1949	2,829	3,784	34	3,784	6,179	63
1956	3,732	5,439	46	5,439	8,490	56
1958	3,769	5,567	48	5,567	9,206	65

^a Not available.

Source: Table 1.

mentary school and high school graduates, there has been relatively little change in the income differential between high school and college graduates. In 1939, the average income of college graduates was about \$900, or 57 per cent more than for high school graduates. In 1956, the absolute difference between the incomes of these two groups increased to \$3,100, but the relative difference was unchanged. By 1958, the absolute difference rose to \$3,600 and the relative difference also increased to 65 per cent. The data suggest that during the recession years 1949 and 1958 the incomes of college graduates were less affected than other groups, reflecting, perhaps, a greater tendency for persons with lesser schooling to be subject to unemployment. There is also some possibility that the income gains for college graduates partly reflect a rise in the proportion of men in this group with graduate school training. The influence of this factor is probably quite small, however, since there is no evidence of a sharp rise in the proportion of college men with graduate training. Moreover, the income differential between all college gradu-

ates and those with graduate training is quite small, amounting to only about \$200 in 1958 [21, p. 38].

Why has the relative income differential between high school and college graduates been maintained, and indeed recently increased, despite the large relative increase in the size of the college-trained population? One important part of the explanation must be that the demand for college graduates has kept pace with the supply. Due to our changing technology, the demand for trained workers has accelerated since the end of the second world war, and industry has absorbed the increased flow of graduates from our universities. The nature of this change can be seen most clearly by the sharp rise in the proportion of the labor force engaged in professional and managerial work, the two

TABLE 4—NUMBER AND PER CENT OF MALES EMPLOYED IN PROFESSIONAL AND MANAGERIAL OCCUPATIONS, FOR THE UNITED STATES: 1940, 1950, 1957, AND 1959
(Numbers in thousands)

Major Occupation Group	Number				Per Cent			
	1940	1950	1957	1959	1940	1950	1957	1959
Total employed males	33,750	40,519	43,273	42,842	100.0	100.0	100.0	100.0
Professional, technical and kindred workers	2,075	2,971	4,141	4,471	6.1	7.3	9.6	10.4
Managers, officials and proprietors, except farm	3,231	4,341	5,598	5,695	9.6	10.7	12.9	13.3

Source: Data for 1940 and 1950 from *U. S. Census of Population: 1950*, Vol. II, *Characteristics of the Population*, Pt. 1, Table 54. Data for 1957 and 1959 from Bureau of the Census, *Current Population Reports*, Ser. P-60, No. 27 and No. 33.

occupations in which the great majority of college graduates are employed. Table 4 shows that since 1940 there has been a relative increase of about 50 per cent in the proportion of men employed in the two major occupation groups which serve as the major outlet for men with college training.

The conclusion based on Census Bureau data that the income differential between high school and college graduates has been maintained since 1939 presents a somewhat different picture from the one that might be obtained from other information. A study completed in 1957 by Blank and Stigler concludes that "the pronounced downward drift of earnings in all professions (except medicine) relative to earnings of the working population as a whole is well known. . . . This downward drift is known only since 1929, but one may plausibly conjecture that it began much earlier because the main force working in that direction—the rapid expansion in the number of trained professional workers—also began much earlier" [2, p. 25]. There is no necessary incon-

sistency between this finding and the results based on census data described above since (a) Blank and Stigler measured trends since 1929 whereas the present study considers trends since 1939; and (b) college graduates are not all employed in professional jobs. In 1950, only about one-half of the male college graduates worked at professional jobs.⁸ It is entirely possible that the differentials for college graduates as a whole have been maintained since 1950, but that the differential for the professional group has decreased. This could have happened if there was a substantial increase in the differential for college graduates employed in nonprofessional jobs.

Historical time series on income are not available for all, or even for many, professional occupations and much of the information that is available does not quite measure up to acceptable standards of reliability or comprehensiveness. The figures for engineers used by Blank and Stigler, for example, relate to median base monthly salary rates rather than annual earnings, and therefore cannot be readily compared with other estimates for the general population or for other professions. Moreover, the estimates for 1929, 1932, and 1934 are based on a mail survey in 1935 with a nonresponse rate of 67 per cent [2, p. 16]. The estimates for 1939, 1943, and 1946 are based on a mail survey in 1946 with a nonresponse rate of 47 per cent [2, p. 111]. One might well question the credence that should be attached to an estimate of monthly income made five years after the income was earned, based on a survey in which about half or two-thirds of the population covered did not respond.

Blank and Stigler present indexes for 1929-1954 of the ratio of median monthly engineering salaries to average wage or salary or net income for full-time wage and salary employees, full-time manufacturing wage earners, lawyers, physicians, dentists, and college teachers. Incomes are shown for only five professional groups which in 1950 accounted for only about two-fifths of all male professional workers,⁹ including engineers for whom the income data are of doubtful utility. The four professional occupations shown, excluding engineers, represented only about one-fifth of all male professional workers in 1950 [22].

Table 5 has been prepared to facilitate a closer examination of trends in professional incomes since 1939. This table contains the five series used by Blank and Stigler and a sixth, annual earnings of public school

⁸ Estimate derived from [23]. This source shows 1.6 million, or 55 per cent, of all male college graduates classified as "professional, technical, and kindred workers." It is estimated that 1.5 million men were professional workers and that .1 million were technicians.

⁹ [22]. This source shows 3 million men classified as "professional, technical, or kindred workers." It is estimated that 2.5 million of these men were professional workers and that .5 million were technicians.

TABLE 5—EARNINGS OF FULL-TIME EMPLOYEES IN ALL INDUSTRIES, AND OF SELECTED PROFESSIONAL AND NONPROFESSIONAL OCCUPATIONS: 1939 TO 1954

Year	Average Annual Earnings per Full-Time Employee, all Industries	Average Annual Salary		Average Annual Net Income			Median Base Monthly Salary Rate, Engineers ^b
		Public School Teachers ^a	College Teachers	Non-Salaried Lawyers	Non-Salaried Physicians	Non-Salaried Dentists	
Dollar Values							
1939	\$1,264	—	—	\$4,391	\$ 4,229	\$3,096	\$277
1940	1,300	\$1,441	\$2,905	4,507	4,441	3,314	—
1941	1,443	—	—	4,794	5,047	3,782	—
1942	1,709	1,507	2,914	5,527	6,735	4,625	—
1943	1,951	—	3,039	5,945	8,370	5,715	334
1944	2,108	1,728	3,331	6,504	9,802	6,649	—
1945	2,189	—	3,277	6,861	10,975	6,922	—
1946	2,356	1,995	3,465	6,951	10,202	6,381	409
1947	2,589	—	3,735	7,437	10,726	6,610	—
1948	2,795	2,639	4,123	8,003	11,327	7,039	—
1949	2,851	—	4,234	7,971	11,744	7,146	—
1950	3,008	3,010	4,354	8,349	12,324	7,436	—
1951	3,231	—	—	8,855	13,432	7,820	—
1952	3,414	3,450	5,105	9,021	—	—	—
1953	3,587	—	—	9,392	—	—	518
1954	3,670	3,825	—	10,258	—	—	—
Index: 1939=100 ^c							
1939	100	—	—	100	100	100	100
1940	103	100	103	103	105	107	—
1941	114	—	—	109	119	122	—
1942	135	105	103	126	159	149	—
1943	154	—	105	135	198	185	121
1944	167	120	115	148	232	215	—
1945	173	—	113	156	260	224	—
1946	186	138	113	158	241	206	148
1947	205	—	123	169	254	214	—
1948	221	183	142	182	268	227	—
1949	226	—	145	182	278	231	—
1950	238	209	153	190	291	240	—
1951	256	—	—	202	318	253	—
1952	270	239	175	205	—	—	—
1953	284	—	—	214	—	—	187
1954	290	265	—	234	—	—	—

^a Public elementary and secondary school teachers, supervisors, and principals. Figures are for "school" years ending in the year indicated; for example, the figure shown for 1954 is for the school years 1953-1954.

^b For 1953, graduate engineers only. All other figures are for graduates and nongraduates. The corresponding figure for graduate engineers for 1946 is \$405.

^c 1940 was used as the base year for public school and college teachers.

Source: Bureau of the Census, *Historical Statistics of the United States-Colonial Times to 1957*, July 1960.

teachers. Excluding engineers, the relative figures for only two of the professions, college teachers and nonsalaried lawyers, show a distinct downward drift as compared with average earnings for all workers. The increase in income for doctors was somewhat better than, and for dentists about on a par with, that recorded by the average worker; and the increase for public school teachers was only slightly behind that recorded by the average full-time employee. On the basis of this limited evidence it cannot be concluded beyond a reasonable doubt that the income differential between professional and nonprofessional workers has been narrowed since 1939.¹⁰

II. Annual Income in Relation to Age and Education: 1939-1958

Next to be considered is the impact of age on the relationship between educational attainment and earnings. As might be expected, the advantages of additional years of schooling do not have a very strong immediate impact on earnings. Inexperienced workers in most occupations start at a relatively low level of earnings, but the latter tend to increase as skill and experience are acquired. Therefore, the financial benefits of additional schooling tend to accumulate over time, and the greatest impact is felt during the period of peak earnings. These tendencies are clearly reflected in Table 6 which shows the earnings at an average age of 30 (i.e., after about 10 years of work experience) and 50 (i.e., about the age of peak earnings) for men with different amounts of educational attainment. If data were available, an average age of 22 might have provided a better basis for comparison, although at this age many persons undergoing professional training have not yet completed their schooling. In each education group (with the exception of the lowest one in 1958), the period of maximum earnings is between 45 and 54 years of age.

In view of the limited range of earnings possible in most of the jobs for which elementary school graduates can qualify, it is not surprising that their annual earnings after a lifetime of work do not much exceed their initial earnings. College graduates, on the other hand, tend to work at jobs in which the possibilities of high earnings are much greater

¹⁰ Census Bureau data show that during 1939-58 the median wage or salary income of professional workers rose 2.3 times as compared with gains ranging from 2.7-2.9 times for craftsmen, operatives, service workers and nonfarm laborers [21, p. 6]. These data tend to support the view that professional workers made smaller relative income gains than the general working population during this period. The data, however, are subject to the limitation of being restricted to persons with wage or salary income and therefore exclude independent professionals. Moreover, the data also show that it was only during the first half of the period that professional workers made the lower relative gains. Since 1950, the relative gains in wages and salaries and total income have been greater for professional workers than for the general working population.

and therefore have peak earnings which, on the average, far exceed initial earnings.

Table 6 also shows that the differential between initial and peak earnings has decreased progressively for elementary and high school graduates, but has remained fairly constant for college graduates. Evidently, among workers who have not attended college, the younger groups have succeeded in making greater relative gains in earnings than those with more experience. Between 1939 and 1949, these gains were probably due in large measure to reduced unemployment among the

TABLE 6—MEAN INCOME (OR EARNINGS) FOR MALES 25 TO 34 YEARS AND 45 TO 54 YEARS OF AGE, BY LEVEL OF SCHOOL COMPLETED, FOR THE UNITED STATES: 1939, 1946, 1949, 1956, AND 1958

Age and Level of School Completed	1939	1946	1949	1956	1958
Elementary school graduate:					
25 to 34 years	(a)	\$2,011	\$2,540	\$3,685	\$3,663
45 to 54 years	(a)	2,629	3,247	4,289	4,337
Per cent increase	(a)	31	28	16	18
High school graduate:					
25 to 34 years	\$1,335	2,335	3,246	4,813	4,909
45 to 54 years	2,256	3,744	4,689	6,104	6,295
Per cent increase	69	60	44	27	28
College graduate:					
25 to 34 years	1,956	3,237	4,122	6,307	7,152
45 to 54 years	3,575	5,242	8,116	11,702	12,269
Per cent increase	83	62	97	86	72

^a Not available.

Source: Table 1.

younger (and less skilled) groups. The further gains during the past decade may reflect the "across-the-board" increases in union contracts during the postwar period which resulted in greater relative gains for lower-paid workers and perhaps also the gradual rise in the minimum wage required by law in many industries.

As previously noted in the discussion of the figures for all age groups combined, there was no reduction in the income of college graduates relative to high school graduates despite the great rise in the proportion of persons completing the requirements for a college degree. Table 7 shows that among men 25 to 34 years old, the proportion of college graduates doubled between 1940 and 1959 whereas the proportion of men who terminated their schooling with elementary school graduation was reduced by two-thirds.

Table 8 shows that despite the relative decrease in the number of

TABLE 7—PER CENT DISTRIBUTION BY YEARS OF SCHOOL COMPLETED FOR MALES 25 TO 34 YEARS OLD, FOR THE UNITED STATES: 1940, 1947, 1950, 1957, AND 1959.

Years of School Completed		1940	1947	1950	1957	1959
Total		100	100	100	100	100
Elementary School:	Total	45	30	29	22	20
	Less than 8 years ^a	22	(b)	16	12	11
	8 years	22	(b)	13	10	9
High School:	1 to 3 years	21	23	21	22	20
	4 years	20	31	28	31	33
College:	1 to 3 years	7	9	10	10	11
	4 years or more	7	7	9	14	15
Not Reported		1	1	3	1	1

^a Includes persons reporting no years of school completed, not shown separately.^b Not available.

Source: See source note for Table 2.

elementary school graduates 25 to 34 years old since 1946, their earnings have not risen as much as the earnings of high school graduates, and therefore the income differential between these two education groups has widened. Looked at another way, the earnings of elementary school graduates increased by about 82 per cent between 1946 and 1958 as compared with an increase of about 110 per cent for high school graduates and 121 per cent for college graduates. This table also shows a comparative decline in the relative income position of younger college graduates, a tendency which appears to have been reversed in 1956-58.

The reduction in differentials between 1939 and 1956 in favor of the younger high school graduates could be in part the result of the large increase in the number of college graduates. It is significant, however,

TABLE 8—MEAN INCOME, (OR EARNINGS) BY LEVEL OF SCHOOL COMPLETED FOR MALES 25 TO 34 YEARS OLD, FOR THE UNITED STATES: 1939, 1946, 1949, 1956, AND 1958

Year	Elementary-High School Differential			High School-College Differential		
	Average Income		Per Cent Difference	Average Income		Per Cent Difference
	Elementary School Graduate	High School Graduate		High School Graduate	College Graduate	
1939	(a)	\$1,335	(a)	\$1,335	\$1,956	47
1946	\$2,011	2,335	16	2,335	3,237	39
1949	2,540	3,246	28	3,246	4,122	27
1956	3,685	4,813	31	4,813	6,307	31
1958	3,663	4,909	34	4,909	7,152	46

^a Not available.

Source: Table 1.

that the decrease in income differentials between high school and college graduates which took place between 1939 and 1946, was not accompanied by a relative increase in the number of college graduates. As Table 7 shows, only 7 per cent of the men 25 to 34 years old in 1940 and 1947 were college graduates. It does appear, therefore, that other factors than the relative supply of college graduates must be responsible for the observed change in income differentials between these two groups. The greater relative gains for high school graduates between 1939-49 may in part be due to reductions in unemployment for this group; and the reversal in trend since that time is perhaps associated with a rising demand for college-trained personnel.

In evaluating income changes in relation to education for specific age groups, the experience of veterans and nonveterans during the past 12 years is worth considering. While the second world war was still in progress, the government instituted a program under the Servicemen's Readjustment Act of 1944 (the GI bill) designed to assist veterans in re-establishing themselves in civilian life. A most important part of this program was the provision of government-financed education intended to permanently improve the economic status of veterans. Nearly 8 million veterans of the second world war made use of the education and training benefits at a cost of \$14.5 billion to the federal government. Over 2 million men received college and university training and an additional 3.5 million received free education below the college level at elementary and secondary schools, vocational and trade schools, technical institutions, and business schools [20, p. 28]—the largest program ever undertaken by the federal government in providing financial aid to individuals in completing their education and training.

The impact of the GI bill on the educational attainment of veterans is clearly shown in Table 9. In 1947, when most of the former servicemen were in the initial phase of their training under the GI bill, veterans were already a more highly educated group than nonveterans. This is, of course, to be expected since many men were rejected for military service because they were of low intelligence. There was no difference in the proportions of younger veterans and nonveterans who had completed college; but a larger proportion of the veterans had been exposed to some college training, even if they did not graduate. By 1952, this picture had changed considerably. The proportion of college graduates among younger veterans increased from 7 per cent to 12 per cent, as compared with an increase from 6 per cent to 9 per cent for nonveterans. At the lower educational levels, the gains for veterans were equally striking.

Since older veterans did not make as much use of the education and training provisions of the GI bill as the younger veterans, their educa-

tional attainment did not change as much. The most significant change for the older veterans was a sharp drop in the proportion who quit school upon completion of the eighth grade and an increase in the proportion of high school graduates. Between 1947 and 1952 there was no change in the proportion of college graduates among older veterans.

Are the shifting patterns of educational attainment for veterans and nonveterans associated with corresponding income differences? Table 10 suggests an affirmative answer. Veterans in age groups which made greatest use of the educational and training provisions of the Readjust-

TABLE 9—PER CENT DISTRIBUTION BY YEARS OF SCHOOL COMPLETED FOR MALE VETERANS AND NONVETERANS OF THE SECOND WORLD WAR, BY AGE, FOR THE UNITED STATES: 1947 AND 1952

Years of School Completed	Veterans of Second World War				Nonveterans			
	25 to 34 Years		35 to 44 Years		25 to 34 Years		35 to 44 Years	
	1947	1952	1947	1952	1947	1952	1947	1952
Number (thousands)	6,851	8,428	2,035	4,130	4,043	2,472	7,791	6,070
Total	100	100	100	100	100	100	100	100
Elementary- Total	24	20	37	27	40	43	48	41
Less than 5 years ^a	3	2	4	2	9	14	8	8
5 to 8 years	21	18	33	25	31	29	40	33
High School: Total	58	56	40	49	45	39	37	44
1 to 3 years	24	22	18	20	21	17	19	20
4 years	34	34	22	29	24	22	18	24
College: Total	17	23	20	23	13	16	14	15
1 to 3 years	10	11	8	11	7	7	7	8
4 or more years	7	12	12	12	6	9	7	7
Not reported	1	1	2	1	1	2	1	1

^a Includes persons reporting no years of school completed, not shown separately.

Source: Bureau of the Census, *Current Population Reports*, Ser. P-20, No. 15, *Educational Attainment of the Civilian Population: April 1947*, Table 3; and Ser. P-20, No. 45, *School Enrollment, Educational Attainment and Illiteracy*, Oct. 1952, Table 16.

ment Act also made the greatest relative income gains during the post-war period. In 1947, veterans 25 to 34 years old had somewhat lower incomes than nonveterans despite their greater educational attainment. Thus, any selective factors which may have produced higher incomes for veterans were not operative immediately after the war. The lower incomes of veterans at this time may have been due to several factors including the greater work experience of the nonveterans as a result of their civilian employment during the war and also the loss of civilian employment during 1947 by many veterans who went to school part time or who served in the armed forces during part of the year. By 1948, veterans and nonveterans had the same average incomes and in every year thereafter veterans received greater relative income gains,

TABLE 10—COMPARISON OF MEDIAN INCOMES OF MALE VETERANS AND NONVETERANS OF SECOND WORLD WAR, BY AGE AND EXTENT OF EMPLOYMENT: 1947-1958

Year	Median Total Money Income				Ratio of Veterans' Income to Nonveterans	
	25 to 34 Years		35 to 44 Years		25 to 34 Years	35 to 44 Years
	Veterans	Non-veterans	Veterans	Non-veterans		
Total						
1947	\$2,401	\$2,585	\$2,689	\$2,900	93	93
1948	2,734	2,692	3,045	3,046	102	100
1949	2,828	2,562	2,984	2,935	110	102
1950	3,058	2,626	3,291	3,234	116	102
1951	3,359	2,875	3,647	3,595	117	101
1952	3,631	3,065	3,834	3,602	118	106
1953	3,948	3,183	4,118	3,867	124	106
1954	3,978	3,073	4,227	3,818	129	111
1955	4,330	3,294	4,483	3,946	131	114
1956	4,675	3,712	4,853	4,220	126	115
1957	4,984	4,041	4,985	4,279	123	117
1958	5,010	4,171	5,225	4,306	120	121
Year-Round Full-Time Workers						
1955						
Per cent	81	72	81	78	—	—
Median income	\$4,630	\$3,854	\$4,679	\$4,319	120	108
1956						
Per cent	83	72	80	78	—	—
Median income	\$4,944	\$4,150	\$5,122	\$4,554	119	112
1957						
Per cent	82	70	80	74	—	—
Median income	\$5,321	\$4,465	\$5,321	\$4,792	119	111
1958						
Per cent	76	66	77	70	—	—
Median income	\$5,453	\$4,804	\$5,609	\$4,844	114	116

Source: Bureau of the Census, *Current Population Reports*, Ser. P-60, annual issues.

reaching a maximum differential of 30 per cent in 1955. Because of the changing age composition of veterans in the 25 to 34 year age group during recent years, it is difficult to make meaningful comparisons between veterans and nonveterans in this age group since 1955.

A large proportion of the veterans who were 25 to 34 years old in 1947 have now moved into the 35 to 44 year age group. As a result, the income differential between veterans and nonveterans within this age group is now beginning to increase markedly. Until 1953, veterans who were 35 to 44 years old had only slightly higher incomes than nonveterans. By 1956, the differential increased to 15 per cent and in 1958 it rose still further to 21 per cent. These figures provide presumptive evidence that the educational training received by young veterans 12

years ago contributed in an important way to establishing for them a permanent advantage over nonveterans.

One small step toward bolstering this conclusion can be made by restricting the comparison to persons who are year-round full-time workers. In this way, account can be taken of the greater tendency for nonveterans to lose work, presumably because of ill health. Table 10 shows that the average income of veterans is about 20 per cent higher than that of nonveterans even when account is taken of the differential effects of part-time employment.

III. *Lifetime Income in Relation to Education: 1939-1958*

Estimates of lifetime income provide summary measures of the financial returns associated with education which cannot be readily obtained from the annual data presented above.¹¹ The estimates of lifetime income presented here are derived figures—one might say synthetic figures—based on variations in the payments to individuals in different age and education groups at a given time, specifically the calendar years for which data are presented. The figures are, therefore, based on a cross-section of the population in 1939, 1946, 1949, 1956, and 1958, and not on life-cycle data which would trace a man's income from the time he starts to work until he retires. Actually there is some question whether life-cycle data would be more suitable for the present analysis; cross-section data have the advantage that "they are free from the influence of variants such as periods of industrial depression or unusual activity with their changes in opportunities for employment, in wage rates, and in the cost of living."¹²

Standard life-table techniques were used in computing the figures shown in Table 11. First, an estimate was made of the number of 100,000 children born in 1939, 1946, 1949, 1956, and 1958 who would survive to each given year of age. These estimates were made from the appropriate life tables.¹³ By way of illustration, it was estimated that

¹¹ For additional information on the estimation of lifetime income see [5] [8] [9] [10] [13] [17].

¹² [18, p. 9]. Also [14].

¹³ The following life tables were used: 1939, *Life Table for White Males from U. S. Life Tables and Actuarial Tables*, 1939-1941, Bureau of the Census, 1946; 1946, *Abridged Life Table for White Males*, 1946, *Vital Statistics of the United States*, 1946, Pt. I; 1949, *Abridged Life Table for White Males*, 1949, *Vital Statistics of the United States*, 1949, Pt. I; 1956, *Abridged Life Table for White Males*, 1956, *Vital Statistics—Special Reports*, Vol. 48, No. 6, June 19, 1958; 1958, *Abridged Life Table for White Males*, 1957, *Vital Statistics—Special Reports*, Vol. 50, No. 2, July 28, 1959. A more complete source could have been used for 1949, "United States Life Tables, 1949-51," *Vital Statistics—Special Reports*, Vol. 41, No. 1, November 23, 1954. It was not used, however, because of the desire to retain comparability with previous estimates for the same year shown in [10]. As noted below, however, there are other differences between the estimates shown here for 1949 and those contained in [10].

out of 100,000 infants born alive in 1956, about 96,000 would survive to age 18, at which time they would enter the labor market. The basic problem consisted of estimating the life span of these 96,000 survivors and the amount of income they would receive during their lifetime. For this purpose, it was assumed that survival rates for men in each education group would be the same as for all white males in 1956. On this basis, it was estimated that these 96,000 men would live a total of nearly 5,000,000 man-years between age 18 and the time the last one died. It was further assumed that during each year of life, these men would receive an average income corresponding to that received by men in the same age group with the same amount of education. The averages used for this purpose are those shown above in Table 1 plus estimates for age groups under 25 based on published and unpublished data of the Census Bureau.¹⁴ The averages (means) are based on persons reporting \$1 or more of income, excluding the relatively small number of men in most age groups without income.¹⁵

There are several cautions that should be considered before discussing the figures in Table 11. First, the figures are not exactly comparable from year to year due to changes in the income concept. The data for 1939 are for wages and salaries, 1946 are for earnings, and 1949, 1956, and 1958 are for total income. These variations in concept may have some impact on changes over time. A more general consideration is the fact that the estimates reflect the economic conditions and other circumstances which existed in each of the years for which data are shown. Some of the differences from year to year may reflect changes in these circumstances. The increase, for example, in the value of a college education by about \$140,000 between 1949 and 1958 reflects the increase in prices as well as changes in the underlying rela-

¹⁴ Estimates of lifetime income based on medians rather than means may be obtained from the author. Some analysts contend that the median is a more useful basis than the mean for measuring lifetime income since it is less affected by extremes and more nearly shows what the "typical" individual may expect to receive. The more prevalent and more valid opinion (from a strictly mathematical viewpoint) is that the median should not be used for the computation of lifetime incomes. Thus, for example, Kuznets and Friedman advise that "the actuarial nature of the problem clearly requires arithmetic mean earnings, which are usually considerably higher than median earnings" [8, p. 87] and Houthakker states that "the median is clearly not the appropriate type of average for the present purpose" [13, p. 24].

¹⁵ Comparable figures for 1949 in [10] and [13] are based on all persons reporting on income rather than just those reporting \$1 or more. The method of computing the means in this report conforms to that generally used by the Bureau of the Census. In any event, the inclusion or exclusion of the zero income group would have little impact on the estimate of the mean for most age groups and on the estimate of lifetime income. A further difference between the figures for 1949 in the present report and in [10] and [13] relates to the ages covered by the estimates. In [10] the estimates cover incomes received between the ages 22 and 74, and [13] the estimates cover incomes received from age 14 to death. In the present report estimates for various age groups are shown.

TABLE 11—LIFETIME INCOME (EARNINGS) BASED ON ARITHMETIC MEANS FOR MALES IN
SELECTED AGE GROUPS, BY YEARS OF SCHOOL COMPLETED, FOR THE UNITED STATES:
1939, 1946, 1949, 1956, AND 1958

Years of School Completed and Age	1939 ^a	1946 ^b	1949 ^c	1956 ^d	1958 ^e
Income from Age 18 to Death:					
Elementary: Total	(e)	(e)	\$113,330	\$154,593	\$154,114
Less than 8 years ^d	(e)	(e)	98,222	132,736	129,764
8 years	(e)	(e)	132,683	180,857	181,695
High School: 1 to 3 years	(e)	(e)	152,068	205,277	211,193
4 years	(e)	(e)	185,279	253,631	257,557
College: 1 to 3 years	(e)	(e)	209,282	291,581	315,504
4 years or more	(e)	(e)	296,377	405,698	435,242
Income from Age 25 to Death:					
Elementary: Total	(e)	\$ 87,004	104,998	143,712	143,808
Less than 8 years ^d	(e)	74,369	91,095	123,295	120,965
8 years	(e)	98,702	122,787	168,004	169,976
High School: 1 to 3 years	(e)	107,940	141,870	192,254	198,881
4 years	(e)	135,852	174,740	237,776	241,844
College: 1 to 3 years	(e)	161,699	201,938	281,553	305,395
4 years or more	(e)	201,731	286,833	391,992	419,871
Income from Age 18 to 64:					
Elementary: Total	\$ 40,005	(e)	100,413	138,127	137,786
Less than 8 years ^d	(e)	(e)	86,912	117,930	115,418
8 years	(e)	(e)	116,968	161,124	161,643
High School: 1 to 3 years	56,653	(e)	132,371	182,795	188,362
4 years	71,453	(e)	159,487	224,529	231,509
College: 1 to 3 years	77,775	(e)	180,841	254,092	279,640
4 years or more	109,961	(e)	251,493	354,457	382,982
Income from Age 25 to 64:					
Elementary: Total	37,172	74,071	91,932	127,047	127,286
Less than 8 years ^d	(e)	62,334	79,654	108,310	106,449
8 years	(e)	84,687	106,889	148,033	149,687
High School: 1 to 3 years	53,011	92,044	121,943	169,501	175,779
4 years	67,383	114,023	148,649	208,322	215,487
College: 1 to 3 years	73,655	138,871	173,166	243,611	269,105
4 years or more	104,608	168,983	241,427	340,131	366,990

^a Restricted to persons reporting \$1 or more of wage or salary income and less than \$50 of other income for native whites and Negroes.

^b Total money earnings.

^c Total money income.

^d Includes persons reporting no years of school completed, not shown separately.

^e Not available.

tionships. These factors demonstrate an important advantage of the cross-sectional approach used in preparing the estimates. Since the averages used for each year are based on the experience for that year, they implicitly provide estimates in dollars of constant purchasing power for persons in all age groups in that year.

A final caution relates to the possible intrusion of extraneous factors

in the relationship between lifetime earnings and education. Family influence in the form of financial help while at college and assistance in obtaining relatively high-paying jobs cannot be ignored; but, at the same time, should not be exaggerated. In 1950, about one-third of the college students away from home came from families with less than average incomes.¹⁶ Few of these students could expect much financial help from their families, or assistance in locating lucrative job opportunities.

In every year for which data are presented, additional schooling is associated with a very substantial increase in lifetime income. On the basis of conditions in 1958, an elementary school graduate could expect to receive during his lifetime about \$52,000 (or two-fifths) more income, on the average, than the person who had no schooling or who terminated his formal education before completing the eighth grade. The difference between the expected lifetime income of the average elementary school and high school graduate was equally striking. In 1958, the average elementary school graduate could expect a lifetime income of about \$182,000 as compared with about \$258,000 for the average high school graduate. The expected income differential associated with the four years of high school education therefore amounted to about \$76,000 or 42 per cent.

Since a college degree is the "open sesame" to many, if not most, high-paying jobs, it should come as no surprise that the greatest income gains associated with additional schooling appear at the college level. On the basis of 1958 data, a college graduate could expect to receive about \$435,000 income during his lifetime as compared with \$258,000 for the average high school graduate. It can, therefore, be estimated that the approximately $4\frac{1}{2}$ years of schooling beyond the high school level were associated with an increase of about \$177,000 in lifetime income or about \$40,000 per year of schooling.

Due to the considerable decrease in the purchasing power of money during the past 20 years, meaningful comparisons over time in the absolute amount of lifetime income received by men with different amounts of schooling can be made only after some attempt has been made to adjust for price changes. However, some indication of the relative changes since 1939 in the lifetime incomes of elementary, high school, and college graduates can be made on the basis of the unadjusted data shown in Table 12. These data are restricted to the income received between the ages of 25 and 64 because this is the only age span for which data are available for each of the four years under consideration.

¹⁶ Based on unpublished data in the Census Bureau's *Current Population Survey* for March 1950.

Since 1939 the more highly educated groups have clearly made the greater relative gains in expected lifetime income. Thus, for example, in 1946, high school graduates could expect to earn only 35 per cent more between the ages of 25 and 64 than elementary school graduates. Twelve years later the differential in favor of the high school graduates increased to 44 per cent, reflecting the fact that the expected income of high school graduates rose more rapidly during this period.

A comparison of the relative income gains expected by high school and college graduates reveals essentially the same pattern. Evidently, the differential in favor of college graduates dropped from 55 per cent in 1939 to 48 per cent immediately after the end of the second world war, probably because a larger proportion of the college graduates con-

TABLE 12—INCOME RECEIVED FROM AGES 25 TO 64, FOR MALES, BY LEVEL OF SCHOOL COMPLETED, FOR THE UNITED STATES: 1939, 1946, 1949, 1956, AND 1958

Year	Elementary-High School Differential			High School-College Differential		
	Elementary School Graduate	High School Graduate	Per Cent Difference	High School Graduate	College Graduate	Per Cent Difference
1939	(a)	\$ 67,383	(a)	\$ 67,383	\$104,608	55
1946	\$ 84,687	114,023	35	114,023	168,983	48
1949	106,889	148,649	39	148,649	241,427	62
1956	148,033	208,322	41	208,322	340,131	63
1958	149,687	215,487	44	215,487	366,990	70

* Not available.

Source: Table 11.

tinued to serve in the armed forces during part of 1946. By 1958, however, the differential between high school and college graduates rose to 70 per cent, reflecting a greater relative income gain for the college group. These figures support the conclusion presented earlier that the large increase in the number of college graduates during the postwar period has not adversely affected their income position.

A very crude attempt to adjust the data for price changes is presented in Table 13, where the current dollar estimates of lifetime income have been modified on the basis of the consumers price index, and the results are expressed in dollars of 1958 purchasing power. Here again the data are restricted to incomes received between the ages of 25 and 64 years in order to present estimates for each of the years under consideration. There are, of course, several important limitations to the adjustment that has been attempted. It is not clear, for example, that the consumers price index is equally applicable to each education group or that it can be applied to figures which include the farm population,

since the index refers specifically to moderate-income urban wage-earner families. Moreover, since the figures under consideration refer to income expected to be received in the future, it is not clear that a price adjustment based on current data is applicable. For these and other reasons the estimates must be regarded as only the roughest sort of approximations. Nevertheless, the figures do show that lifetime income, adjusted for price changes, has risen considerably since 1939 for all education groups. The rise amounted to about \$75,000 (about 54 per cent) for high school graduates and \$150,000 (about 69 per cent) for college graduates. A rise in "real" income was, of course, to

TABLE 13—"PRICE-ADJUSTED" INCOME (EARNINGS) RECEIVED FROM AGES 25 TO 64,
FOR MALES BY YEARS OF SCHOOL COMPLETED, FOR THE UNITED STATES:
1939, 1946, 1949, 1956, AND 1958
(Dollars of 1958 purchasing power)

Years of School Completed	1939	1946	1949	1956	1958
Elementary: Total	\$ 77,281	\$109,735	\$111,568	\$135,013	\$127,286
Less than 8 years ^b	(a)	92,347	96,667	115,101	106,449
8 years	(a)	125,462	129,720	157,315	149,687
High School: 1 to 3 years	110,210	136,361	147,989	180,129	175,779
4 years	140,089	168,923	180,399	221,384	215,487
College: 1 to 3 years	153,129	205,735	210,153	258,885	269,105
4 years or more	217,480	250,345	292,994	361,457	366,990

^a Not available.

^b Includes persons reporting no years of school completed, not shown separately.

Source: Data in Table 11 adjusted by consumers price index.

be expected because of the general increase in productivity and incomes during this period.

IV. Conclusion

This study largely represents an attempt to ascertain if the marked increase in the number and proportion of high school and college graduates during the past generation has been associated with a reduction in income differentials for these groups. On theoretical grounds, such a reduction could be expected *in the long run*, assuming no changes in the demand for more highly educated workers. The period under consideration, however, is relatively short and is one in which there were changes in the demand for, as well as in the supply of, such workers. Therefore, no fundamental theoretical issues are involved in this paper. The problem is merely one of ascertaining what has taken place and why.

The figures show that despite large relative reductions in the supply

of workers whose schooling did not extend beyond the eighth grade, this group had smaller relative income gains than high school graduates. On the other hand, the large relative increase in the supply of college-trained workers did not adversely affect their relative income position. On this basis it is concluded that the demand for more highly educated workers has kept pace with the increased supply of such workers and, as a result, their relative income position has not changed. The fact that the proportion of men employed in professional and managerial work—the two major outlets for college-trained men—increased by 50 per cent during the past generation suggests that industry has absorbed the increased flow of graduates from our universities.

In this study an attempt has also been made to prepare estimates of lifetime income for persons with different amounts of educational attainment. These estimates, heretofore available only for 1949, have also been made for the years 1939, 1946, 1956 and 1958. The conclusions based on the lifetime data parallel, in most respects, those derived from the annual data.

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PROGRESSIVENESS OF A SALES TAX IN RELATION TO VARIOUS INCOME BASES

By DAVID G. DAVIES*

The weight of contemporary opinion bases the ability to pay taxes on the level of an individual's income. There are, however, various concepts of income, and these differ in adequacy as measures of taxable capacity. This paper is devoted to the calculation of different equity estimates for the Ohio state sales tax when alternative income concepts are used to determine the effective rates of taxation. The various income concepts to be considered are: gross money income, net money income (gross income minus personal taxes), the Irving Fisher measure of real income, and the Friedman type of permanent income. An attempt is also made at calculating equity estimates when alternative tax bases are considered.

I. *Method*

It is assumed that the sales tax is passed forward by the seller to the ultimate consumer. In the absence of increasing returns to scale, part of the tax will be shifted to the buyer if the elasticity of demand is greater than minus infinity, and the elasticity of supply is greater than zero. Given a negative slope to the demand curve, the change in price approaches the amount of the tax as the elasticity of supply approaches infinity. Given an elasticity of supply greater than zero, the change in price approaches the amount of the tax as the elasticity of demand approaches zero.

A progressive tax is defined as one in the case of which the ratio of the amount of tax to income (i.e. the effective rate of taxation) increases as income increases. A tax is proportional if the effective rate remains constant as income fluctuates; and a tax is regressive when the effective rate decreases as income increases. Unfortunately, none of the empirical tax-on-income structures in this paper fits any of the preceding definitions precisely.¹ The functional relationship between the effective

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¹Very few, if any, empirical tax structures would fit any of these definitions of progressivity, proportionality, or regressivity. The federal income tax law, for example, exempts income up to \$600 from any income taxation. The average rate of taxation, therefore, does

rate of taxation and income is in no case continuously positive, nor continuously zero, nor continuously negative.

We are thus obliged to construct a measure which will permit us to indicate whether a given total tax structure for a whole set of income classes is progressive, proportional, or regressive. Such an "equity" measure must utilize some kind of average concept in order to reveal the tax-income relationship for the entire income distribution rather than just two juxtaposed income classes. Since the amount of tax collected under a sales tax is a constant proportion of the tax base, we propose to use the elasticity of the tax base with respect to income as a measure of equity for the whole tax structure. If, for example, a sales tax statute provides no exemptions, then total consumption expenditure is the tax base. Then, if gross income is one of the alternative definitions of income, and the elasticity of consumption with respect to gross income is less than one, then the percentage change in sales tax is less than the percentage change in gross income and the tax structure is regressive. Alternatively, if the elasticity of consumption with respect to gross income equals one, the tax is proportional; and if it is greater than one, the tax is progressive.

For purposes of calculating the elasticity coefficients, we divide the entire income distribution into different income classes. Within the whole range of incomes, we consider the proportionate variation of the tax base with respect to income as our equity criterion. However, since various different tax bases and various definitions of income are possible, in principle, many distinct elasticity measures may be applied for each income range. Hence, if T_{jk} is the k th tax base for the j th income class, Y_{jl} is the l th income concept for the j th income class, and ϵ_{jkl} is the error term, we have as our basic relationship

$$(1) \quad \sum_{j=1}^J \log T_{jk} = \log A_{kl} + b_{kl} \sum_{j=1}^J \log Y_{jl} + \sum_{j=1}^J \log \epsilon_{jkl} \quad \begin{matrix} (k = 1, 2, \dots, K) \\ (l = 1, 2, \dots, L) \end{matrix}$$

For the whole set of income classes of any given alternative definition of income, the parameter b_{kl} reveals the elasticity of the k th taxable base with respect to that l th income concept.²

not rise as income rises to \$600. Even where there is a tax liability, two individuals, earning less than \$5,000 each, who use the Internal Revenue Service tax table and who are located at opposite ends of a given income bracket will pay an identical amount of tax; but the person on the upper side of the bracket has a lower average rate of taxation since his income is slightly higher. Nobody seriously contends that the federal income tax is not progressive because of these minor aberrations.

² In computing the elasticity coefficients, we have assumed that there are no errors of observation in the variables. The latent variable $\log \epsilon$ exists because of random shocks. We further assume that the distribution of $\log \epsilon$'s is independent and normal with mean of zero and the variance constant. While it is assumed that $\log \epsilon$'s are not autocorrelated, the data may, in fact, exhibit some interdependence. Where the logged values of the de-

Many different alternative tax bases are possible although only one usually exists in a given jurisdiction at any given time. Various possible alternatives analyzed in this paper are listed below. Only number 2 is the actual sales tax system now operative in Ohio. The most prominent exemptions in the Ohio law are: food to be consumed off the premises of the point of sale, certain farm products, occasional sales, articles becoming a component part of a finished product, school textbooks, motor fuel, tobacco products, gas, electricity, water, admissions, and all services. The other alternative tax bases listed below are hypothetical ones that the legislature has either not considered or, if considered, has rejected as an actual tax base. The elasticity measures under consideration are:

1. b_{C,Y_G} : the elasticity of total consumption with respect to gross income,
2. b_{C_1,Y_G} : the elasticity of actual taxable consumption (the tax base in actual operation in Ohio) with respect to gross income,
3. b_{C_2,Y_G} : the elasticity of taxable consumption with respect to gross income if food that is now taxable (i.e. food sold in restaurants, etc.) were excluded from C_1 ,
4. b_{C_3,Y_G} : the elasticity of taxable consumption with respect to gross income if only food for home consumption were exempt and no other exemptions were permitted,
5. b_{C_4,Y_G} : the elasticity of taxable consumption with respect to gross income if only nonfood items now exempt under the law were considered exempt and food for home consumption were not exempt,
6. b_{F_1,Y_G} : the elasticity of total food consumption with respect to gross income,
7. b_{F_2,Y_G} : the elasticity of taxable food with respect to gross income,
8. b_{F_3,Y_G} : the elasticity of nontaxable food (i.e. food for home consumption only) with respect to gross income, and
9. b_{F_4,Y_G} : the elasticity of nonfood exemptions with respect to gross income.

pendent variable do not occur as independent variables, the Durbin-Watson test can be used for determining the absence or presence of autocorrelation in the residuals. Unfortunately, the significance tables associated with this test [2] do not handle samples of less than 15. Since the sample size of income classes for this paper is less than 15, no test for autocorrelation was administered. On the above assumptions, then, we compute the elasticity of the k th tax base with respect to the l th income concept for the whole set of income classes by

$$(2) \quad b_{T_k, Y_l} = \frac{\sum_{j=1}^J (\log T_{jk} - \log \bar{T}_k)(\log Y_{jl} - \log \bar{Y}_l)}{\sum_{j=1}^J (\log Y_{jl} - \log \bar{Y}_l)^2}$$

where \bar{T}_k is the mean of the k th tax base and \bar{Y}_l is the mean of the l th income concept.

In addition to Y_G or gross income, these same alternative tax bases were used to calculate approximate elasticity coefficients for the whole range of income classes of Y_N or net income, Y_F or Fisher income, and Y_P or permanent income.

II. *The Empirical Evidence*

The sources of the basic statistics in this study are the Wharton School of Finance and Bureau of Labor Statistics reports on consumer income, expenditures, and savings [5]. The sample for this paper consists of 897 households in 9 different income brackets located in several Ohio cities.

TABLE 1—PERTINENT COMPONENTS OF CONSUMPTION: 897 OHIO HOUSEHOLDS

Income Class	Mean Consumption	Mean Taxable Consumption	Mean Total Food	Mean Taxable Food	Mean Nonfood Exemptions
Under \$1,000	\$1,212	\$ 426	\$ 402	\$ 55.6	\$ 439.6
\$1,000 to \$1,999	1,725	607	627	90.1	581.1
\$2,000 to \$2,999	2,605	1,024	843	149.6	887.6
\$3,000 to \$3,999	3,457	1,385	1,074	134.4	1132.4
\$4,000 to \$4,999	4,410	1,566	1,213	188.4	1419.4
\$5,000 to \$5,999	5,055	2,340	1,382	256.0	1589.0
\$6,000 to \$7,499	5,995	2,773	1,557	296.7	1961.7
\$7,500 to \$9,999	6,922	3,131	2,056	455.6	2190.6
\$10,000 and over	9,828	5,575	2,270	899.4	2882.4

Source: *Study of Consumer Expenditures* (University of Pennsylvania 1956).

Table 1 presents data on the actual taxable consumption base now in use along with other statistics which form the basis for calculating alternative tax bases. Table 2 contains the estimates on the basis of which comparisons and conclusions can be made as to the progressivity, proportionality, or regressivity of the actual and other possible alternative tax bases under various definitions of income. One assumption behind the elasticity coefficients of the alternative tax bases in Table 2 is that the demand for consumption items is of unit elasticity within the price range of plus or minus 3 per cent of the actual price of the various consumption items. If, for example, to develop any of our hypothetical tax bases we levy a tax on an item which in actual fact bears an exemption, the total amount spent on that item will remain constant after the tax is imposed. Then, bearing in mind that the legal rate of taxation is constant at 3 per cent, it is possible indirectly to obtain through the different elasticity coefficients, an estimate of the impact on the effective tax rates for different income concepts when alternative tax bases are employed.

Column 1 in Table 2 yields data on the elasticities of the actual and 8 other hypothetical tax bases for the whole set of income classes when the income concept is gross income. Comparison of each of the 8 hypothetical tax-base elasticity coefficients with the actual coefficient within the gross income column will give an estimate of the equity impact of each of various exemptions and nonexemptions on the existing tax structure.

A. Gross Income

In the gross income sample under examination the elasticity of consumption items now legally subject to the tax with respect to gross in-

TABLE 2—ELASTICITY COEFFICIENTS FOR 9 TAX BASES
AND 4 INCOME CONCEPTS

Gross Income	Net Income	Fisher Income	Permanent Income
$b_{C,Y_G} = .674$	$b_{C,Y_N} = .700$	$b_{C,Y_F} = 1.000$	$b_{C,Y_P} = 1.000$
$b_{C_1,Y_G} = .816$	$b_{C_1,Y_N} = .845$	$b_{C_1,Y_F} = 1.214$	$b_{C_1,Y_P} = 1.182$
$b_{C_2,Y_G} = .815$	$b_{C_2,Y_N} = .843$	$b_{C_2,Y_F} = 1.214$	$b_{C_2,Y_P} = 1.198$
$b_{C_3,Y_G} = .730$	$b_{C_3,Y_N} = .756$	$b_{C_3,Y_F} = 1.086$	$b_{C_3,Y_P} = 1.068$
$b_{C_4,Y_G} = .701$	$b_{C_4,Y_N} = .726$	$b_{C_4,Y_F} = 1.039$	$b_{C_4,Y_P} = 1.034$
$b_{F_1,Y_G} = .557$	$b_{F_1,Y_N} = .577$	$b_{F_1,Y_F} = .820$	$b_{F_1,Y_P} = .848$
$b_{F_2,Y_G} = .819$	$b_{F_2,Y_N} = .847$	$b_{F_2,Y_F} = 1.204$	$b_{F_2,Y_P} = 1.065$
$b_{F_3,Y_G} = .478$	$b_{F_3,Y_N} = .495$	$b_{F_3,Y_F} = .704$	$b_{F_3,Y_P} = .803$
$b_{F_4,Y_G} = .618$	$b_{F_4,Y_N} = .639$	$b_{F_4,Y_F} = .919$	$b_{F_4,Y_P} = .932$

Source: Based on data in *Study of Consumer Expenditures* (University of Pennsylvania 1956).

Symbols: For identification of symbols, see p. 989.

come, or b_{C_1,Y_G} , is less than one. Therefore, the tax must be regressive when gross income is used as the criterion of taxable capacity. The introduction of certain exemptions, however, makes the tax less regressive than it would be without these exemptions.

When the nontaxable food component of consumption is examined, $b_{F_3,Y_G} = .478$, the lowest elasticity coefficient in Table 2. This low magnitude for food which is now legally exempt permits the elasticity of actual taxable consumption to be higher than it would be if such food were made taxable. The elasticity of total food consumption on gross income or $b_{F_1,Y_G} = .557$. Since $b_{F_1,Y_G} > b_{F_3,Y_G}$, the exemption of food for

home consumption helps make the tax structure less regressive.

Since b_{F_2, Y_G} , the elasticity of the taxable-food part of consumption, is greater than the elasticity of actual taxable consumption, b_{C_1, Y_G} , it follows that b_{C_1, Y_G} is higher than if taxable food were exempt. Should the legislature exclude taxable food, which is food consumed in restaurants, etc., from the tax base, taxable consumption would become C_2 , and $b_{C_2, Y_G} < b_{C_1, Y_G}$. This kind of change would make the tax structure more regressive.

The elasticity of nonfood exemptions actually allowed by law with respect to gross income or $b_{F_4, Y_G} = .618$. Since $b_{C_1, Y_G} > b_{F_4, Y_G} > b_{F_3, Y_G}$, non-food exemptions make the tax less regressive, but they do not exercise as strong an influence as the food exemptions in pushing the tax toward progressivity.

In summary, if only nonfood items now exempt were to be exempt and food for home consumption were nonexempt, C_1 becomes C_4 . If only food for home consumption were in fact exempt and no other exemptions were permitted, C_1 becomes C_3 . Bearing in mind that as b_{C_1, Y_G} approaches one, the tax becomes less regressive, we have $b_{C_4, Y_G} < b_{C_3, Y_G} < b_{C_2, Y_G} < b_{C_1, Y_G}$.

B. *Net Income*

The second column in Table 2 shows various elasticity coefficients for the whole set of income classes when the income concept is net income. Intracolumnar and intercolumnar comparisons reveal the same relative pattern among the various elasticity coefficients as that which exists in column 1, but the net-income elasticity magnitudes are greater than their gross-income counterparts.

C. *Fisher Income*

The Fisher income concept used in this paper is actual consumption expenditures. Consumption is the destruction of utility and according to Fisher comes closest to measuring real income [1]. Sources of receipts are left untaxed while uses of receipts for consumption are taxed.³ The variation of measured gross or net income over time is greater than that for consumption. This must be the case for certain individuals, because the low-income families in the sample for this paper have average propensities to consume which are much greater than 1. These individuals are decreasing their assets and/or increasing their liabilities in order to consume. Past measured income must have been greater than past consumption in order to decrease assets currently, or the expectation that future consumption will be less than higher future income allows liabilities to be incurred currently.

³ See [4] for an extension of Fisher's idea.

If the Fisher income concept is taken as a measure of ability to pay, it follows that if a household spends \$1,000 on consumption goods, its taxable capacity and ability to pay is based on \$1,000 rather than current annual measured gross or net income; and it makes no difference whether the current measured income is greater or less than consumption. A radically different pattern of ratios and elasticity coefficients emerges when Fisher income is substituted for either gross or net income. The third column in Table 2 gives the elasticity coefficients with respect to Fisher income for the whole set of income classes. The coefficients of actual taxable consumption as well as for the alternative possible bases of C_2 , C_3 , C_4 , and F_2 are all greater than one. The tax for these bases, then, is progressive.

D. *Permanent Income*

Like Fisher income but unlike gross or net income, permanent income has a wider time horizon than one year. Like gross and net income, but unlike Fisher income, permanent income indirectly incorporates saving into the analysis as the residual between permanent income and permanent consumption [3]. As a result of Fisher's exclusion of saving, the slope of the regression of consumption on Fisher income is equal to one and greater than the slope of the regression of permanent consumption on permanent income.

Where the permanent income hypothesis is generally supported,⁴ we can let $C_P = \beta_P Y_P$ represent the simplified permanent consumption function. There is no divergence between the average propensity to consume and the marginal propensity to consume, and

$$\frac{d \log C_P}{d \log Y_P} = 1.$$

If the legal rate of taxation is constant, the tax would be proportional. When the legislature introduces the appropriate exemptions such as food and rent, so that

$$\frac{d \log C_{1P}}{d \log Y_P} > 1,$$

the tax becomes progressive. Permanent income and permanent consumption cannot be observed directly, for they are *ex ante* theoretical concepts, but they can be inferred from the receipts and expenditure patterns of households. The appendix to this paper shows how these permanent magnitudes were derived from cross-section data.

The fourth column of Table 2 presents the elasticity coefficients of various possible tax bases with respect to permanent income. The coeffi-

⁴ See, for example, Robert Eisner, "The Permanent Income Hypothesis: Comment," *Am. Econ. Rev.*, Dec. 1958, 48, 972-90.

cients are quite similar to those derived for Fisher income. When permanent income is used as the criterion to adjudge the equity effects of the sales tax, the alternative tax bases of C_1 , C_2 , C_3 , C_4 , and F_2 are all progressive.

III. *Conclusions*

For the whole set of income classes under observation the Ohio sales tax is regressive with respect to both gross and net measured annual income, although less so when net income is adopted as the criterion of taxable capacity. The empirical and derived data also support the hypothesis that the sales tax is progressive for the whole set of income classes when the Irving Fisher concept of income is used as the criterion on which to calculate the schedule of tax rates. The same conclusion holds when the permanent income concept is adopted. In all cases the exemption of food to be consumed off the premises of the place of sale makes the tax less regressive (or more progressive). The nonfood exemptions allowed under the law also makes the tax less regressive (or more progressive), but does not have as strong an impact on the rate structure as the food exemption. The legal provision that requires payment of the tax on food consumed on the premises has a slight positive effect in decreasing the regressivity or increasing the progressivity of the sales tax in the gross and net income cases.

APPENDIX

Following Friedman's analysis [3], we present the framework which yields the values of pertinent parameters from cross-section data. The purpose of this rather rough model is not to provide definitive results nor test the permanent income hypothesis, but rather to provide an apparatus from which rough comparisons can be made between the elasticity coefficients associated with measured income and those based on permanent income. We utilize the same notation employed in the text except that J does not include the highest income class of "\$10,000 and over," and the subscripts T and P are used to designate transitory and permanent magnitudes.

The heart of Friedman's permanent income hypothesis is contained in the following four equations:

$$(1) \quad C_P = \beta_P Y_P$$

$$(2) \quad Y = Y_F + Y_T$$

$$(3) \quad C = C_P + C_T$$

$$(4) \quad r_{C_T Y_T} = r_{C_T C_P} = r_{Y_T Y_P} = 0$$

where r is the coefficient of correlation.

In order more easily to convert the measured magnitudes into permanent estimates we assume that the relationship of measured consumption on measured income is linear. In our notation we have:

$$(5) \quad \sum_{j=1}^J C_j = \alpha + \beta \sum_{j=1}^J Y_{aj}$$

where $\beta = .695$, $r = .997$, and the standard deviation from regression is \$166.00.

We are not testing the permanent income hypothesis here, so now we assert that:

$$(6) \quad \frac{\sum_{j=1}^J Y_{Tj}}{J} = \frac{\sum_{j=1}^J C_{Tj}}{J} = 0$$

although for any j , $Y_T \neq 0$, and $C_T \neq 0$ except where $\bar{Y}_G = \bar{Y}_P$. When both the mean transitory income and mean transitory consumption are equal to zero, we have

$$(7) \quad \frac{\sum_{j=1}^J Y_{Gj}}{J} = \bar{Y}_P \quad \text{and}$$

$$(8) \quad \frac{\sum_{j=1}^J C_j}{J} = \bar{C}_P.$$

From the assumption in (6), it follows that

$$(9) \quad \frac{\sum_{j=1}^J C_j}{J} \cdot \frac{J}{\sum_{j=1}^J Y_{Gj}} = \beta_P = .8768$$

When $\beta_P > \beta$, then for any j where

$$(10) \quad Y_P > \bar{Y}_G, \quad \text{it follows that} \quad Y_G > Y_P, \quad \text{and where}$$

$$(11) \quad Y_P < \bar{Y}_G, \quad \text{it follows that} \quad Y_G < Y_P.$$

Inequalities (10) and (11) have serious implications with respect to measuring the equity effects of a tax when permanent rather than measured income is utilized as the criterion for calculating the elasticity of the tax base with respect to permanent income. Given the tax base and $\beta_P > \beta$, then the tax will be less regressive or more progressive if permanent rather than measured income is employed as the standard to adjudge the progressivity, proportionality, or regressivity of a tax.

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EMPLOYMENT, GROWTH, AND PRICE LEVELS THE JOINT ECONOMIC COMMITTEE REPORT

A Review Article

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On March 24, 1959, the Senate and House directed the Joint Economic Committee to conduct a large-scale investigation into the problems of maximum employment, an adequate rate of economic growth and the maintenance of price stability. On January 10, 1960, less than a year later, the Committee and the staff assembled for the purpose concluded its task, having published a Committee Report which with minority and supplemental views totaled 156 pages, a Staff Report (488 pages), 13 volumes of hearings (3486 pages) and 23 study papers (1364 pages).¹ It will be some time before the profession has managed to digest this enormous volume of material. This review is particularly concerned with the two central documents, the Committee and Staff Reports [2] [3].

I. The Committee Report

The Committee Report was not unanimous. Six members withheld approval; their minority statement complained that the report was needlessly partisan and in part unsubstantiated by the testimony of witnesses; objections to the Staff Report were also raised. In addition, three members added supplemental views; among these Senator Butler, for the minority, felt that its statement did not go far enough, whereas Representative Patman (whose emphasis on the need for lower interest rates was quite pronounced) felt similarly about the majority's report. For its recommendations and topics the report of the majority draws mainly, as might be expected, on the Staff Report. But the choice is a selective one, and emphases frequently differ. There is full agreement on a central issue, however: the importance of maintaining an adequate rate of growth.

The Committee Report states categorically: A high and sustained rate of growth is possible—with relatively full employment and a stable price level—provided proper policies are followed. The required policies are summarized in the report's 12-point series of recommendations. It is a mixed program. There is perhaps some tendency—evident in subsequent elaboration as well as in the list itself—towards a diminution of novelty if not of meaningful content, as

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¹ The complete list of hearings, studies and reports is in the bibliographical section of the following issues of the *American Economic Review*: Sept. 1959, p. 812; Dec. 1959, p. 1136; Mar. 1960, pp. 261-62; June 1960, p. 521.

it draws towards its close. Proposal 10 suggests that tariffs be reduced gradually. Number 11 is "Revise our farm policy, including more research to find new uses for farm products." The concluding proposal is noncontroversial: "Improve our foreign trade position."

Among its important recommendations, the report cites the proposal for a federally sponsored program of education as most important in contributing to an improved rate of growth. The program (adapted from the Staff Report) is a broad one, including not only school aid but provision for support of medical and scientific research, public and private apprenticeship programs, and the establishment of national productivity centers. The report accords top priority to this program in order to sustain a growth rate of 4.5 per cent over the next 15 years.

Some supporters of education may be distressed that such a program should be advocated principally in order to win a growth race. Moreover, it is a little surprising that the claims therefor should be associated with a time-span as short as 15 years, since the payoff from investment in education, in addition to its difficulties of measurement, is usually considered to be slow. In any event, a growth rate of 4.5 per cent annually clearly demands that education be given some assistance from economic policies in their more conventional form, and the Committee Report's proposals in this area have immediate professional interest.

Other proposals for promoting growth include the following: (1) monetary and fiscal policies permitting an expansion of money demand appropriate to the increase in output, (2) greater reliance on fiscal policy (in relation to monetary policy), and (3) strengthened application of the antitrust laws.

The first of these recommendations cites the fact that since 1953 the money supply has grown less rapidly than output and suggests—drawing on Milton Friedman's argument submitted at both 1958 and 1959 hearings—a steady year-to-year percentage increase in the money supply. It is a tribute to Friedman's persuasive powers that the Committee Report approves this proposal even though the Staff Report does not. But the similarity between the Committee Report and Friedman's views ends abruptly—this much seems clear, even if the report's outline of the scope of monetary policy is not—since Friedman had held that monetary policy ought in essence to involve no more than such expansion. That he would approve his own proposal in the setting supplied in the report seems rather improbable.

Defense spending receives prominent treatment within the review of fiscal policy (which the Committee Report regards as having "prompter and larger effects" [3, p. 19] than monetary policy). It is argued that an increase in defense spending contributed to the short-period excess demand in 1955, that a cutback contributed to the 1957-58 recession, that these were avoidable errors, that such errors have been "one of the major causes of economic instability" since the second world war [3, p. 20], and that defense spending could to some degree be manipulated in the interests of positive fiscal policy.

But what is most important is the proposal [3, p. 20] for stimulating growth through a combination of tight fiscal and easy monetary policy: run substantial budget surpluses in prosperous periods and use the proceeds for

debt retirement, thus encouraging investment at the same time as consumption is restrained. Changes in tax rates (income taxes in particular) are proposed as the principal discretionary fiscal tool.

Advocacy of this program suggests some confidence that investment will be responsive to easy money (and hence, to some degree at least, to low interest rates) and that it will not be inhibited by other aspects of the program such as the lowered consumption-investment ratio. Any failure of investment would make the program's results singularly unhappy ones. Yet, later, the Committee Report admits the gaps in our knowledge: "... further studies should be undertaken to determine what can be done to reduce the instability of plant and equipment investment. It may well be that it is impossible to stabilize these outlays, or that stabilization would lead to a lower average level" [3, p. 33]. It is also disturbing to read, at the beginning of Section 5 that "effects of a higher interest rate structure on spending are quite moderate."

Section 5 (Debt Management) is lengthy; it ranges over such topics as Treasury consultation with the banking community on new bond issues, oligopoly in the New York bond market, callable bonds, and the bills-only doctrine. The recurring theme is that interest rates on federal issues have been higher than was necessary. (The report refuses to approve lifting of the $4\frac{1}{4}$ per cent bond interest ceiling until "major reforms in fiscal, monetary and debt management policies are instituted.") But the complaint is not that too-high rates have restrained spending and growth; it is that the Treasury has in this way needlessly spent money to the benefit of interest-receivers. However important the problem of debt-service cost may be in other respects, it takes an effort of will to regard it as a central issue in the devising of policies suitable for growth, full employment and stable prices.

The Committee Report's recommendations propose also a reordering of priorities in government spending so as to place greater emphasis on programs "essential to the Nation's and the free world's defense, to promote the well-being of our people, and to contribute to a higher rate of economic growth" [3, p. 24]. In addition to education, the high-priority items include missiles, combat troops, foreign aid, research in science and medicine, natural resource conservation, and aid to distressed areas. The report insists that these proposals are perfectly consistent with its budget-surplus recommendation. The list of reduced-priority programs is, however, briefer: subsidies for agriculture and business and "other resource-wasting Government programs." The report submits two pages of familiar proposals for closing tax loopholes, and draws attention to the easing of financial and economic pressures that come with high growth of output. It does not add that programs designed to stimulate growth may by their nature generate new conflicts and pressures.

The Committee Report has been reviewed at some length since its form entitles it to ranking as the capstone of the entire study. Its content does not. The selection of ideas from the Staff Report is casual; instead of submitting them to proper intellectual discipline, the Committee Report is disposed to take excursions into topics only peripherally related to the central issues. The Staff Report must bear some responsibility for the nature of the Committee Report. Yet the latter does an injustice to the competent

economists who were assembled for the study. And it performs little service for those who have felt that recent policies, pursued out of a zeal to protect the dollar against further depreciation, may have had unfortunate side effects.

II. *The Staff Report*

The suggestion that there exists a natural harmony between growth, high employment and price stability in the proper order of things pervades the Committee Report. The Staff Report [2] is under no such illusion. Its principal theme is clear; it might be summed up as follows: Over-all monetary-fiscal controls are poor tools for the maintenance of price stability. Particularly in the manner of their application in recent years, the cost of so using them, in terms of lost output and growth, has been heavy. The mix of these policies should be altered, and new emphasis put on growth as a national objective.

The Staff Report submits three sets of proposals, the latter two concerned with long-run growth and price stability. The first set deals with monetary-fiscal reform: (1) better management of defense spending to reduce instability; (2) greater stress on discretionary fiscal policy—notably personal income tax changes—as a needed supplement to the automatic stabilizers; (3) more reliance on fiscal policy and less on monetary policy to achieve any given level of restraint; (4) within monetary policy, introduction of selective controls. This is an incomplete list; but presumably most of the remaining proposals, such as abandonment of the bills-only policy, are of somewhat less importance.

These proposals draw on argument later developed at length within the report. The monetary policy chapter stresses the deficiencies of monetary policy as a precise instrument of control. The fiscal policy chapter notes “the decline in fiscal restraint”: a declining ratio of budget surplus to GNP. It is suggested that underreliance on fiscal restraint (through reduction in total saving) and overreliance on monetary restraint have inhibited investment. The Staff Report does not follow the Committee in an explicit recommendation that tight fiscal and easy monetary policy be used in combination (the topic is briefly discussed in Chapters 8 and 9), although it may be argued that this is the only position logically consistent with the report’s proposals in entirety.

It will at once be apparent that neither the theme of the report nor any of the four proposals listed is new. These ideas have been commonly discussed in recent years and supported by many economists of impeccable professional reputation. What is new is that they should appear in combination in a form which offers some suggestion of official sanction. There has already been much angry criticism of the Staff Report, some of it to be expected of any such document which joins criticism of monetary policy to an attitude towards price level stability which—by the standards of recent official pronouncements—is downright casual. Moreover, “the Eckstein group” has not hesitated to pinpoint 1953 as the year the slowdown began, a choice which can hardly be expected to encourage Republican enthusiasm for the report. The authors of the report may have felt that the balance of opinion in government-sponsored documents needed evening up; that, at any rate, is the way it sometimes reads.

The 11 chapters in the Staff Report are uneven in length, style and quality. Chapter 1 is an introduction and summary. The proposals in Chapter 6 (Unemployment) are in the main familiar: adjustments in the unemployment system, a federal program for chronically distressed areas, and so on. Chapter 7 reviews some still more familiar ground in respect to agriculture; it adds little to the report. Chapter 11 deals with the United States in the world economy; its conclusions are not mentioned in Chapter 1, presumably because they deal with topics only peripherally related to the central interest of the report: the gold outflow, the competitive position of U.S. exports, and the use of the dollar as an international currency. Seven chapters thus remain for detailed consideration: three on growth, one each on fiscal and monetary policy, and two on the postwar inflation and policies to restrain market power.

A. Growth

The fact that three chapters are devoted to growth indicates the importance accorded it as a policy objective. Any accusation of failure to stress the importance of reasonable price stability would be quite unfair. But the references, although frequent, are sometimes faintly perfunctory, as though the authors felt that stress on this topic by others had already pushed it into the area of diminishing returns, and that a shift in the balance of emphasis would be desirable.

The genesis of the 4.5 per cent growth rate used in the Committee Report is interesting. Chapter 4 is a short but careful attempt to estimate the likely rate of growth through 1975. It stresses the uncertainties of projection, then suggests 4.5 per cent as the probable upper limit attainable with good management and good luck in respect to all the numerous factors involved. Assuming no serious depression, 3.4 per cent is offered as the other end of this range; an intermediate figure of 3.9 per cent is also mentioned. The upper-limit figure receives rather more attention in Chapter 1, and in the chapter summaries which preface the entire report, things come out like this: "Actually, we can enjoy an even higher rate of growth, 4.5 per cent per year, if we try" [2, p. xxvi]. This is not the tone of the chapter.

Considering the emphasis on growth, it is ironic that much of the material on this topic constitutes the report at its worst. Chapter 3 compares the 1947-52 period against 1953-59, to the detriment of the latter: the growth rate was lower (2.2 per cent as against 4.6) and the speed of recovery from recession was slower. It may well be that the post-1952 period was one in which official policies yielded a payoff in price stability that was small in comparison to the losses elsewhere, even on the basis of the marginal value scale of those disposed towards price stability. But the report pursues this topic elsewhere. In terms of policies for long-run growth, what is the meaning of performance during any such short period, or a comparison between two such periods? Moreover, the choice of periods is an open invitation to partisan dissent. The minority statement published with the Committee report not only dissents, but notes the dubious propriety of using the 1947-52 period, bounded as it was by two wars, for favorable comparison. The use of such figures ex-

poses the entire report to a charge, of which it is in the main quite innocent, of crude statistical manipulation. It does little to advance the affirmative argument of the Staff Report: a serious proposal for greater use of discretionary fiscal policy demands as much bipartisan support as can be mustered, and the choice of such a vinegary approach is puzzling.

Chapter 2, which reviews the significant factors in long-run development, is the basis for the Committee Report's proposals for education and research. The list in the Staff Report is lengthy; it is in fact comprehensive in the sense of listing almost everything that could possibly have been listed. It is submitted in support of quantitative rates of growth. Since scarcity is the fundamental issue behind the growth problem, it is surprising to find proposals which suggest that the scarcity principle is nowhere involved. In an economic report, it might be expected that the recommendations be quantitative, that the proposals be given some order of priority, and that some estimates be made of marginal rates of return. Of course, these are in actuality impossible demands; earlier in the chapter, the report admits as much (and so its assertion that school aid is "the single most important step" becomes a statement of faith). But if quantitative estimates are impossible, the meaningful content of these policy proposals as contributors to a 4.5 per cent growth rate is called into question.

B. Fiscal and Monetary Policy

The fiscal policy chapter is principally concerned with a historical review of federal government policies since the second world war. Otherwise it is confined to a few generalizations which it is suggested arise out of this experience, and a short but important series of proposals for the improvement of fiscal policy. Its generalizations include a recognition of the possible need to choose between full employment and price level stability; the chapter opts for greater emphasis on high employment. It is interesting that Chapter 1, which incorporates other parts of the chapter, omits this assertion, being content to argue that its proposals should "substantially reduce the present impasse between price level stability and a rate of growth commensurate with our true potential" [2, p. 26]. Perhaps it was felt that a blunt statement would oversimplify the issues involved; but Chapter 8 is left with only brief analysis in support of its forthright assertion. Elsewhere, it furnishes some basic arguments used in both Staff and Committee Reports: insufficient attention to the destabilizing impact of changes in defense orders, dwindling use of discretionary fiscal policy, and the need for strengthened automatic stabilizers. No attempt is made at an over-all survey of the uses and limitations of fiscal policy.

Chapter 9 (Monetary Policy and Debt Management) is a different matter. It is by far the most satisfactory part of the entire Staff Report in its attempt to furnish a comprehensive analytic background for the topic under discussion. The 115 pages of this impressive chapter incorporate a very considerable amount of recent thinking on matters related to monetary policy and debt management. Its viewpoints are sufficiently unequivocal and sufficiently un-

orthodox to assure an adequate supply of objections. Time and criticism may in due course possibly invalidate some of these viewpoints; but they are neither foolish nor superficial.

The treatment of general monetary controls in this chapter is not a kindly one. It argues that they are not general at all but apply most unevenly, and with a considerable lag. They have worked most severely on residential housing—largely in consequence of the interest ceilings on FHA and VA mortgages. The power of the Federal Reserve to restrict the total money supply has been severely limited by bank holdings of government debt; so much of this is short-term debt that the locking-in effect during periods of tight money is small. Financial institutions may have become more sensitive to interest rate changes; but they have also become more ingenious in economizing on cash balances during tight-money periods, thus speeding up the velocity of money.

To suggest that skill in mobilizing idle balances can be an offset to tight monetary policy is to argue that such policy may prove clumsy in its application, not that it is useless. Unless there is no practical limit on the extent to which idle balances can be activated, limitation of the total money supply is still of vital importance. This is the essential backing for the argument prominent in the latter half of the chapter, that (in association with fiscal policy) monetary policy *has* been effective in restriction, that it *has* held down output and growth in a misguided attempt to control price-level increases. Already-published criticisms complain that this is working both sides of the street.

Certainly the chapter would have been strengthened had it applied the same form of careful analysis earlier evident in order to demonstrate the consistency of the total argument. There is not much discussion, and what there is tends towards generalities. Thus [2, p. 396]: "It is quite possible that the sharp rises in interest rates in inflationary periods have been relatively ineffective in checking investment in the short run, while the gradual effects of the secular upward drift have weakened the general incentive to invest." The general incentive to invest, whatever it may be, presumably exists in the short run as well as the long. Even the secular period in question is rather short, unless something other than 1954-1959 is meant.

A reconciliation (if one is needed) might emphasize that lags in operation are still longer than is commonly supposed; there is a hint of this [2, p. 393]. That would make monetary policy both effective (in that tight money can restrain spending) and ineffective (in that it may do so only long after the need for restraint has passed). But Friedman has stressed the significance of lags in support of his own earlier-mentioned monetary policy proposal, e.g., his remarks [8]; if the Staff Report were to emphasize the length of lag, it might be regarded as veering towards the Friedman position—which in other respects it is not disposed to do. And there would remain the alleged unresponsiveness of investment and consumer durable spending to interest rate changes, on which considerable stress is laid. The chapter is cool towards the distinction between the interest rate and credit availability: "Doubtless there is something to it . . ." [2, p. 374].

Whatever its shortcomings, Chapter 9 remains a well-marshalled summary criticism of monetary and debt management policy. But by the nature of its topics, it can at best do little more than provide a favorable setting for the affirmative argument of the total Staff Report. Success in Chapter 9 even poses some threat to that argument. For to stress the deficiencies of monetary policy is to imply that fiscal policy merits more credit for the past and more emphasis for the future. But some cited objections to monetary policy (lag between times of need and application, uncertainty as to which way the wind is blowing) can easily be applied to fiscal policy. If it can be shown that the latter is equally clumsy and discriminatory in the small, then it may follow that the mild recessions of the 1950's indicate a degree of control about as good as can reasonably be expected; and that is not quite the conclusion toward which the report is directed.

In any event, the logic of the Staff Report position demands more than a simple advocacy of strength through growth. It requires some argument to rebut the official position—that is, analysis to demonstrate that the marginal gain in price stability resulting from recent policies was small and that the marginal loss in employment and output was large; it demands also a demonstration that this low rate of substitution, or something close to it, would persist over a significant range if effective demand were carefully relaxed. Finally, the argument may have to deal with the fact that (if the investment-consumption ratio is too low) full-employment output does not automatically provide a growth rate that is considered desirable as the result of peering over one's shoulder at an oncoming national competitor. These are all terribly difficult tasks, and even a moderate degree of success would be a major accomplishment.

C. Inflation; the Schultze Paper

Chapter 5 (Postwar Inflation) resembles the fiscal policy chapter in that its primary concern is with the historical record. But it submits even fewer recommendations, and its analytic note is more pronounced. This chapter together with the first three study papers [4] [5] [6] (notably the Schultze paper shortly to be discussed) represent a careful and thorough attempt to evaluate postwar price movements.

The chapter notes the concentrated quality of the 1955-58 price rise: within the wholesale price index, steel and machinery components accounted for two-thirds of the increase. Elsewhere, the principal increases were in construction and in services (notably medical services). Steel is the subject of a separate study paper by Eckstein and Fromm [5]. Its conclusions are cost-push in nature: market power possessed by both industry and union, plus alleged government pressure for wage settlement, yielded a wage-price interaction. (The chapter seems to put more emphasis on the 1955 automobile wage settlement as a factor in the 1956 steel agreement than does the study paper.) The indirect consequences are stressed: a cost-push into steel-using industries, the influence of steel wages as a pattern-setter.

Machinery prices in 1954-58 are explored in T. Wilson's short study paper [6], with the conclusion that the demand pressure of the 1955 investment

boom played the primary role. (But it is possible that this is an area in which the inability of price indices to catch quality improvements may be marked.) In services, pressure of demand is again considered the principal factor; the same verdict is applied, although with more caution, to construction.

Like all other important chapters in the Staff Report, Chapter 5 draws heavily on Charles L. Schultze's analysis of the 1955-57 rise in industrial prices [4]. This reliance is well founded, since the study provides a considerable argument against the use of over-all controls to restrain a particular kind of price-level increase. The Schultze study gives evidence of a gestation period much beyond the ten-month life of the Joint Economic Committee project. It is a measured and careful attempt to apply the literature on inflationary movements to the 1955-57 experience, and is itself a considerable addition to that literature.

Schultze effectively makes the point that it is impossible to "prove" either demand-pull or cost-push inflation by pointing to *ex post* material. If, for example, prices are "cost-determined" and wages are flexible, a surge of demand may have no immediate impact on prices. Instead, the output increase may raise productivity and profit margins. But industry members, bidding for extra inputs, will drive up wages; since prices are cost-determined, they too will increase. In such circumstances, to point to the precedence of wage increase over price increase as evidence of cost-push inflation is to oversimplify the process involved. But Schultze regards the distinction between demand-pull and cost-push as analytically meaningful. It reduces, however, to a difference as to the manner of price determination—whether prices and wages are flexible or cost-determined. ". . . the sensitivity of wages and prices to the state of demand proves to be the crucial factor" [4, p. 39]. To assume flexibility or sensitivity, after the fashion of pure-competition theory, is to incline towards demand-pull. Cost-push analysis considers prices to be regulated more by reference to some standard cost pattern and less by the state of current demand. The choice between these alternative assumptions as to the manner of price and wage determination is crucial if one takes some initial disturbance and seeks to trace out its consequences in terms of price-level behavior or the relation between prices and full employment.

The price rise in 1955-57 was small (about 3.5 per cent annually) for analytic purposes, but it was an increase in the rate of rise accompanied by a drop in the rate of output increase. It was disturbing both to a public uneasy over the threat of inflation and to economists who could not dismiss it, as they could earlier rises, as a simple manifestation of excess demand.

Schultze's name has already become associated with a particular explanation of this rise. Actually, there are three—or four—strands to his account of the background forces involved. Explanation 1 is the now-familiar particular-excess-demand account. Prices are inflexible against downward pressures. Consequently, any important shift in the pattern of demand will drive up prices among those industries favored by this shift; but there will be no corresponding price declines among those industries where demand has fallen. The average of prices will accordingly rise, even though there is no excess of demand in the aggregate, whenever demand shifts.

Explanation 2 is ancillary to No. 1. The demand-in-excess industries bid up wages and material costs in order to expand output. The demand-deficient industries thus experience a cost increase—they must match the wage increases brought about by the demand-in-excess industries—which they pass on as a price increase. Hence prices rise in these industries also.

Explanation 3 pertains to excess demand in the aggregate. Having earlier noted that where prices are primarily cost-determined, a surge of demand can yield a series of lagged cost increases, Schultze notes [4, p. 71], "... the rapid and quite generalized rise in monetary demand during the recovery of 1955 did leave a legacy of built-in price and wage increases which added to the selective inflationary pressures emanating, during the succeeding 2 years, from the capital goods sector of the economy."

Schultze devotes little time to Explanation 3, evidently feeling that he has little further that is new to contribute. His repeated cautions against the use of aggregative analysis when dealing with inflation show his disposition toward Explanations 1 and 2. But he also stresses a fourth element: the rising importance of overhead costs. It is quite possible that this may ultimately prove as significant a factor in pricing behavior as downward price rigidity.

The argument rests on evidence of a rising proportion of overhead costs to total costs. This ratio, as Schultze's statistics seem to indicate quite clearly, has been rising throughout the postwar period. Behind lies a shift of input proportions against direct labor: such labor is being replaced by a combination of new-type capital equipment and supervisory labor. Also, the ratio of relatively short-lived equipment to long-lived plant has risen substantially, thus increasing the depreciation element in fixed costs.

The rise in the ratio of fixed to total cost makes average cost (AC) more sensitive to output changes. Typically, capacity and minimum- AC levels of output have also been increased (it is evidently assumed that these two levels are close to one another). Until 1955, these increases were justified by corresponding increases in actual output. Thereafter, output increases failed to keep pace with the rightward-moving AC curve. Schultze thinks producers may have tried to recoup the consequent rise in AC through a rise in prices.

It is evident from the combination of Explanations 2 through 4—and in part contrary to the impression which Explanation 1 might convey—that Schultze's interpretation of price behavior leans heavily towards the influence of cost of production. In an introductory statement of the basic problem, he suggests that two major sets of factors demand consideration, namely (in a shortened version of his statement [4, p. 4]): (1) the impact of rising prices on aggregate demand, and (2) the impact of changes in demand on price. It is interesting that this interaction involves demand and price, not demand and supply. And the second is unorthodox, in so far as conventional theory indicates that the impact of a change in demand must *always* be to change price, save in particular instances far removed from the concern of the paper, such as the case of the long-run perfectly elastic supply curve derived from constant costs. The possibility which interests Schultze is that price will *not* change. If the critical interaction can be described as one in which demand and price are the elements, it is because price, in his analysis, is so closely associated

with supply: suppliers *make* the price; a change in demand *may* prompt them to change that price—or it may not. The two scissors-blades do not have equal cutting-power. Consequently, a demand-pull theorist may find Schultze's analysis unattractive, despite his insistence that all major inflations are demand-pull in nature, and that any cost-push movement must sooner or later be disciplined by the limiting factors of a fixed money stock or of a progressive tax system.

If Schultze's explanations are considered as generalizations regarding price behavior that apply over a wider period than 1955-57, then it follows that we are in serious inflationary trouble. The 1955 shifts in demand, however considerable, were well within the range to be expected with some frequency in an economy enjoying freedom of choice. Moreover, an emphasis on growth might well intensify such shifts. Our sole consolation is that price increases in the 1950's were fairly moderate—and here, the facts are more comforting than the logic. A price system that is basically cost-determined, but also flexible upward in the face of demand increases, has considerable inflationary potential; and that seems the system implicit in Schultze's explanations. But there may be some conflict between these explanations.

There is a basic shortcoming in the Schultze paper in that it does not utilize a consistent background theory of price determination. To insist that its author provide such a theory, in addition to his thorough exploration and analysis of a particular historical period, is to demand perfection. But this paper is a principal analytic prop for a major Congressional study, and it must be judged accordingly.

For his analytic background, Schultze relies [4, pp. 55-59] on the "modified full cost" price analysis of Heflebcwer, Lanzillotti, Gordon and others. But—as Schultze himself points out—this analysis provides only limited support. The full-cost price is commonly regarded as determined by the addition of some margin to prime cost or to full cost at some standard operating level. Because it does not recognize per-unit cost changes resulting from a departure from this operating level, it does not help Explanation 4 (rising AC). Schultze must thus support this explanation by the appeal, "It is not at all unlikely that these higher costs formed the basis for price increases." And he may be right, marginal analysis or no. The objection that producers will not dare to increase price in the face of intensified competitive pressure may not hold; there may be some trend towards the example of the regulated-price industries. But this is still in the area of speculation. It is important to know if Schultze's speculation is correct. If it is, then over-all monetary-fiscal policies are effective as price regulators—except that they work in reverse.

Absence of a consistent theory of price is more clearly evident in the conflict between Explanations 1 and 4: one account has producers increasing price when sales exceed their expectations, while the other has them doing so when sales fall below expectations. Indeed, much of the full-cost argument can be turned against Explanation 1. If increases in output (which do not press too hard against capacity) improve both labor productivity and profit margins—a point on which the paper lays considerable stress—then the in-

centive to exploit a rising market by increasing price is weakened. If the firm's goal is some target rate of return, expanded output means that this target is more easily reached or surpassed without price increase. Moreover, the statistical backing for Explanation 1 is not strong: Schultze's 48-commodity correlation between price changes and output changes is low. He gets a high correlation only by pulling out nine of the most uncooperative commodities. All that seems to come out of these statistics is that nearly all prices show a tendency to move upward, and that this tendency is a little more pronounced among commodities whose output is increasing most rapidly.

Explanation 2 can be attacked in somewhat comparable fashion. Input prices can be expected to rise only in those cases where demand-in-excess industries compete for inputs with demand-deficient industries *and* where the increase in the former's demand for inputs exceeds the decrease in the latter's—it being remembered that demand in the aggregate is not excessive. In 1955, the wage-rate increase in industries at the top of the increase-in-output range was nearly identical with the wage increase in industries at the bottom of this range. Schultze seems to regard this as evidence that demand-in-excess industries initiated the upward wage movement, which then spread throughout all of manufacturing. But the figures neither affirm nor deny this contention.

Perhaps, as Chapter 5 of the Staff Report points out, and as Samuelson and Solow have also suggested [1], there may be no one all-purpose price theory, so that each of Schultze's explanations is correct in its own place. But if such explanations are to become useful theories, we need an *ex ante* statement of the area within which each account can be expected to apply. So long as explanations are framed in general terms, without such a statement of the territorial limits of each, it seems appropriate to pit one against the other in search of inconsistencies. It will be a happier prospect for the 1960's if some of Schultze's hypotheses turn out to be wrong or to be limited in scope; and it will be a happier prospect for price theory if some tolerably uniform generalizations about price behavior can be sustained.

Whatever inner shortcomings, if any, there may be, the Schultze analysis remains a powerful reinforcement for the Staff Report. Yet a conclusion similar to that which was suggested with respect to the monetary policy analysis may once again apply: Schultze's paper advances the Staff Report thesis only so far; in some respects it may even be considered an embarrassment to that thesis. It is clear that over-all monetary restraint is a poor tool for dealing with the kind of price-level increase that Schultze describes. But the same conclusion applies to fiscal restraint. And a demonstration that price-level increases are so intractable may at least provide some justification for the degree of official stress which has been placed on the importance of this problem. Hence in order for the Staff Report to sustain its emphasis on growth, and at the same time to relieve public uneasiness that a growth program might intensify upward pressures on the price level, the Schultze paper would seem logically to demand of that report an adequate set of proposals for inflationary control. The Staff Report does submit proposals, in Chapter 10, now to be considered. As to their adequacy, there may be some division of opinion.

D. Anti-Inflationary Public Policies

Chapter 5 in the Staff Report concludes that policies for restraint of price level increases must be "diverse and flexible." Diversity and flexibility in public policy are to be sought after even as beauty in women; but the specifications are not easily drawn up in either circumstance. Chapter 10, at any rate, has limited success.

This chapter, which occupies just over 9 pages in the Staff Report, proposes a vigorous and expanded anti-trust program and a reduction in tariffs. Hopes are expressed that some benefit might accrue from an annual business-labor conference. Finally, there is some discussion of various stages of government intervention in the price-making process, policies which the chapter carefully warns must be considered only as a last resort. In this group, the most drastic measure cited is the establishment of an agency authorized to suspend proposed price increases and to hold hearings thereon. This is precisely the kind of central power now vested in agencies like the Interstate Commerce Commission, and its mention in a sense brings the argument of the chapter around full circle, since in its earlier one-page elaboration of the proposed antitrust program, the chapter suggests that Congress re-evaluate the policies of such agencies.

Perhaps it is true that [2, p. 439]: "By making the economy more competitive, antitrust activities serve not only to reduce inflation, they also help encourage a more rapid rate of growth." But this is an unsupported statement; it is not adequately sustained by vague associations between price level increases and market power. It is not so long ago, at the close of the second world war, that economists were wondering why prices rose less rapidly in the concentrated industries.

The question at issue is not the use of antitrust laws as instruments of social policy, but their use for particular objectives. The Sherman Act was not written to restrain the rate of price-level increase, nor to sustain a desired rate of growth in national income. It is not uncommon, when demanding such things of the antitrust laws, to take out insurance by expressing disappointment at their past performance (the Staff Report is no exception); but unreasonable demands can lead only to further disappointment. Simon Whitney's testimony before the Committee at the 1958 Hearings did not support the view that antitrust convictions bring about changes in pricing practices; and what is still more to the point, there is little support for it in Theodore J. Kreps' study paper written for this Committee project [7].

At the very least, it is doubtful whether any feasible stiffening of the anti-trust laws would significantly alter price behavior. An attack on conscious parallelism is an attack on a symptom. If it were to be pushed until it had a major impact on pricing practices, we simply do not know what the total consequences would be, except that to assume an impact confined to the manner of price-setting would be naive. As to this matter, rather more knowledge as to the sociology of price-setting than we now possess is needed.

The antitrust proposal is still more puzzling to interpret (except as a simple compulsion to say *something*) when it is recalled that (with the pos-

sible exception of construction) steel is the only industry discussed in Chapter 5 in which there is a close association between price increases and market power.

Of course, the Schultze particular-excess-demand argument persists: it is the downward inflexibility of prices in the demand-deficient industries that pushes up the price average. But it follows that these are the prices towards which antitrust action should be pointed. Yet the Staff Report is notably cautious about associating downward inflexibility with market power. There is no hesitation in the monetary policy chapter on this point; but there is a mild defense of wage inflexibility in Chapter 5 that might well be interpreted equally as a defense of price inflexibility. This caution seems wise. As to downward inflexibility, "monopolistic conspiracy" is a name, not a theory, and a temporary suspension of judgment seems appropriate. It is a phenomenon still to be explained. Perhaps in part it is an illusion; but while there may be less inflexibility than the published figures would indicate, there is also more of it than marginal price analysis would indicate.

If antitrust action will not work, then it follows that we know exceedingly little about proper measures for control of the price level except for such obvious (and just possibly sufficient) restraints as control over the total stock of money. And what is even more distressing, we know very little as to the quantitative terms of choice between the objectives listed in the title of the Committee's study.

In this respect, the Staff Report is unsatisfying. Chapter 1 asserts that much time and energy was spent in an investigation of the relation between growth and the price level; yet no summary conclusions are easily found. The report's strengths lie in the evaluation of monetary policy and in the Schultze analysis. It makes a fairly good case that discretionary fiscal policy has been neglected and monetary policy overworked. On growth, it is notably less successful. We are all in favor of growth; what we want to know is how much growth at what cost in terms of what.

There is a vast amount of useful analysis in the Staff Report, the hearings and the study papers. The central topics are difficult and complicated in the extreme. There are marks of haste and insufficient integration throughout the Staff Report; insufficient time is the obvious source of many of its shortcomings. Bluntly put, it was ridiculous to expect that a staff could be assembled, hearings held, study projects assigned, and the whole enormous resultant mass digested, in less than a year. Viewed against the background of this limiting factor, the Staff Report emerges as a remarkably good performance. But if a comprehensive and penetrating study was really wanted, why was not adequate time allocated for its preparation?

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COMMUNICATIONS

The Origins of the Contributors to the A.E.R. During the 'Fifties

A few years ago an analysis was made of the institutional affiliations of participants in the programs of the American Economic Association [2]. This note, an extension of that inquiry, reviews the institutional affiliations of contributors to the *American Economic Review* (apart from the Proceedings); and identifies the institutions at which the contributors obtained their terminal degrees.

There were 383 contributors to the AER in the last decade (1950-59). They produced 484 "titles" which encompassed 5,664 pages. An average of 14.79 pages and 1.26 titles per author was recorded.¹ The top 25 contributors accounted for 20 per cent of the total pages and 15 per cent of the total titles.²

A contribution, or title, was defined to include articles, review articles, notes, communications, and memorials. Ordinary book reviews were not counted. Since some contributions had more than one author and sometimes these authors were associated with different institutions it was decided to make a page count for the contributions of institutions instead of a "title" count.³ When a contribution was written by more than one author the number of pages was divided equally among the authors' institutions. Contributors were affiliated with 120 institutions. Table 1 shows the institutions in which contributions aggregating more than 100 pages had their origin during the last decade.

There was less concentration of authors in a small number of institutions in the case of the regular issues of the AER than in the case of the Proceedings [2]. Authors at four institutions had contributed about one-third of the papers in the case of the Proceedings. In this study authors at four institutions produced a little over one-fifth of the total.⁴ Both the University of California and the University of Chicago repeated in the top four. No one institution dominated the contributions. The University of California, the largest contributor, produced only 6.9 per cent of the total. Fustfeld's conclusion still holds and is reinforced for the location of a contributor:

The large universities provide their staff with more time for research, assistance of various types, and graduate students to dig up data. It would follow that people at such institutions would be discovering more, have

¹ These averages were the simple arithmetic means of the total pages and total titles divided by the total contributors. There were 276 authors who contributed only once.

² None of the top 25 contributors collaborated with each other, although 4 of the 25 collaborated with some of the other 358.

³ Over 90 per cent of the titles and pages were contributed by single individuals.

⁴ If titles contributed (even when computed by different methods) are used instead of pages the relative rankings of the institutions is slightly different and the percentage by

TABLE 1—INSTITUTIONS WITH WHICH AUTHORS WHO TOGETHER CONTRIBUTED 100 PAGES OR MORE TO THE AER, 1950-1959, WERE AFFILIATED^a

Institutions	Number of Pages	Per Cent of Total Pages
University of California	392	6.9
Massachusetts Institute of Technology	363	6.4
Stanford University	309	5.4
University of Chicago	218	3.8
University of Michigan	214	3.8
Federal Reserve System	200	3.5
Johns Hopkins University	199	3.5
University of California, Los Angeles	197	3.5
Harvard University	185	3.3
Yale University	164	2.9
University of Wisconsin	158	2.8
University of Pennsylvania	135	2.4
Princeton University	134	2.4
University of Illinois	133	2.3
Vanderbilt University	112	2.0
Northwestern University	111	2.0
Carnegie Institute of Technology	102	1.8
International Monetary Fund	100	1.8
	3,426	60.5

^a These statistics were computed by dividing the number of pages in each contribution by the number of authors and adding the results of these allocations according to the institutional affiliation of each author.

TABLE 2—TEN INSTITUTIONS FROM WHICH LARGEST NUMBER OF CONTRIBUTORS (1950-59) OBTAINED THEIR TERMINAL DEGREES

Institution	Number of Contributors	Per Cent of Total Sample of 287 Contributors
Harvard University	62	21.60
Columbia University	38	13.24
University of Chicago	34	11.85
University of California	20	7.00
University of Wisconsin	13	4.53
Yale University	12	4.18
University of Pennsylvania	11	3.83
University of Michigan	10	3.48
Cornell University	8	2.79
Stanford University	8	2.79

more time for reflection, and consequently be able to produce more than economists in a less favorable environment. It might also be argued that

the top four institutions is a little under one-fifth of the contributions. However, the method used in Table 1 appeared to be more informative because of the problem of satisfactorily allocating the divided titles.

the large universities select the best from the profession, and it should be expected that such people would have a better performance record than others.

The *Handbooks* of the A.E.A. [1] were used to locate the institutions at which contributors obtained their last, or terminal degrees. This was done for 287 of the 383 contributors to the regular issues of the AER, 1950-1959. A little over 75 per cent of the located contributors had their last listed student affiliation with 10 institutions. Table 2 shows the number of contributors from these institutions.

TABLE 3—THE RATIO OF THE PERCENTAGE OF THOSE WHO CONTRIBUTED TO THE PERCENTAGE OF THOSE PREPARING DISSERTATIONS FOR EACH OF 20 INSTITUTIONS^a

Institution	Value of the Ratio
University of California	2.92
Harvard University	2.25
Stanford University	2.15
Yale University	1.99
University of Michigan	1.58
University of Chicago	.92
Cornell University	.84
Ohio State University	.83
Johns Hopkins University	.72
Brookings Institution	.72
American University	.72
University of Pennsylvania	.70
Columbia University	.60
Princeton University	.60
University of Wisconsin	.51
University of Illinois	.42
Iowa State University	.42
University of Virginia	.30
New York University	.22
University of Minnesota	.11

^a Ratios were computed for only those schools with 1 per cent, or more, of the sample of potential contributors.

At first glance the top four institutions show a large dominating influence on the contributions to the AER. These institutions produced over 50 per cent of the contributors. Two facts should be noted. First, about one-third of all the contributors could not be identified as to their educational background. Second, it is necessary to consider the institutional distribution of former students in the population from which potential contributors come. This has been attempted by determining the percentage of candidates preparing doctoral dissertations in the top ten institutions listed in Table 2 between 1904 and 1950 [3]. It was found that students from these institutions represent about 70 per cent of the number of potential contributors

contained in the lists of those preparing dissertations at all recorded institutions in 1904-50.⁵

However, there were some interesting differences between the percentage of those preparing dissertations and those who contributed to the AER. For example, Columbia University had 22 per cent of those preparing dissertations and 13.24 per cent of those who contributed. In contrast, the corresponding figures for Harvard University were 9.6 and 21.6 per cent. In Table 3, the ratio of the percentage of those who contributed to the percentage of those who prepared doctoral dissertations is given for each of 20 institutions. A value of 1 would mean that the former students from a specific school were in the same proportion in the sample of the population of potential contributors as in the sample of the population of actual contributors. These ratios may only be suggestive.

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⁵ The authors are aware of the fact that the lists of candidates preparing doctoral dissertations which appear in the AER may fall short of completeness.

* The data for this note was gathered by Cleary, a recent graduate of the University of Virginia. Edwards, a graduate assistant of the Bureau of Population and Economic Research, had been his instructor and acted as technical advisor for the research.

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The First Two Decades of the American Economic Association: Comment

I will not pretend that I can add anything of importance to Dr. Coats's article on the early years of the Association. I think, however, that I can throw some light on the reason why the Association met at Chautauqua in the summer of 1892. Ely was engaged to deliver a short course of lectures as part of the educational program of the camp meeting. It occurred to him, very naturally, to kill two birds with one stone. I had just finished my first year of graduate study in economics at Johns Hopkins, and I decided to take in Ely's lectures at Chautauqua. (I may have been one of the first to take my graduate work in economics in this country instead of Germany.)

The Association met in Chicago during the summer of 1893 at the time of

the World's Fair. I attended this meeting also. I have suspected that Ely arranged for this meeting place also.

T. N. CARVER*

* The author, emeritus professor of economics, Harvard University, was president of the Association in 1916.

Competition and Growth—The Lesson of West Germany: Comment

In a recent article in this journal,¹ Professor Egon Sohmen discusses the problem of rapid economic growth in developed countries, with particular reference to West Germany. Sohmen contends that this country's successful recovery in the postwar years can be attributed to the renunciation of "post-Keynesian orthodoxy" and to its efforts to sustain a "more competitive economy." He wisely concedes, however, that West Germany's economy is far removed from the conditions of perfect competition. But at one point (p. 994) he refers to a "more efficient resource allocation"; and indeed if there is to be any point to his argument at all, he must mean that resources were allocated in West Germany much more in keeping with personal and private preferences, as expressed through a competitive pricing mechanism, and much less according to central government influence, than was true in other Western countries. But if this is truly Sohmen's thesis, it is one that is exceedingly difficult to reconcile with much that happened in West Germany during the period in question.

The facts of recovery are not in dispute. Between 1948 and 1957, West Germany achieved a high growth rate, which is all the more remarkable when one considers the serious structural problems which the country faced at the beginning of this period. Housing and productive facilities had suffered extensive destruction during the war. In addition, there had been a large influx of refugees into the area of the Federal Republic in the years immediately after the end of hostilities.

Many of the difficulties in the structure of production were not easily overcome. In some sectors of the economy, capacity shortages and bottlenecks persisted for a long time. Housing, especially in industrial areas, was painfully inadequate. A heavy volume of investment was needed, and it was of utmost importance that this be directed towards particular sectors and that it be restrained in less critical areas.

If Sohmen's argument were correct, private savings in sufficient volume would have flown smoothly into bottleneck sectors, because price rises in these sectors would have made investments especially profitable. As is well known, however, the volume of private voluntary savings was far from sufficient for the amount of capital formation needed, and with the low per capita income immediately after 1948 even very high rates of interest could scarcely have induced them. On the other hand, high-interest investible funds would have required substantial price rises in some important sectors of the

¹ *Am. Econ. Rev.*, Dec. 1959, 49, 986-1003.

economy. Such price rises were, however, undesirable. One example may suffice.

In housing, the government, acutely conscious of its refugee problem, did not leave matters to be settled by a free rise in rents. If rents had been allowed to rise sufficiently to make residential construction profitable for private funds, most of the income receivers in lower-income brackets would have been prevented from obtaining decent housing. For destitute refugees, however, this was an utmost necessity. Social considerations were of predominant importance for investments in this sector, something which is indicated by the existence of a large program called "Social Residential Construction" (*sozialer Wohnungsbau*).²

As Sohmen says, "West Germany made considerable and highly successful use of tax legislation in order to spur capital formation." It did indeed, but apparently Sohmen does not consider this centrally influenced program of resource allocation a sufficient departure from the principles of *laissez faire* to warrant the slightest attention, nor of sufficient importance to alter his conclusion that the evidence awarding credit for recovery to attempts at "strengthening competition" is "overwhelming."

West Germany's public authorities (federal, state and local government, the Equalization of Burden Fund [*Lastenausgleich*], and the social security system) either supplied or directed the bulk of all investible funds for net capital formation. It is estimated that between 1948 and 1957, of 213.5 billion DM of domestic net investment and increases in claims against foreigners and West Berlin, 92.8 billion DM—43.5 per cent—was furnished by public authorities (19.8 billion DM coming from the social security system). These funds were used for investments made directly by public authorities, or for loans or subsidies to private investors, or for participation with private investors. A conservative estimate is that between 35 and 40 billion DM was granted in loans and other forms to the private sector during the period considered. Large governmental savings were made possible by high tax yields and by relatively low defense expenditures. West Germany's defense expenditures plus occupation costs were much lower as a per cent of GNP than the figures for such countries as France and Britain.³

Government's influence on capital formation was not limited to the direct

² Between 1950 and 1957, 33.7 per cent of all investible funds in this sector were supplied directly by public authorities. If one adds tax exemptions granted for investments in real estate, the ratio is about 44 per cent.

³ Germany's defense expenditures as per cent of GNP were 7 per cent in 1935-36. The defense expenditures of West Germany (either own defense expenditures or contributions to occupational cost) as per cent of GNP were:

1948	5.4%	1952	5.6%	1956	2.8%
1949	5.6%	1953	4.3%	1957	3.0%
1950	4.5%	1954	3.9%	1958	2.8%
1951	5.1%	1955	3.4%		

Source: Deutsches Institut für Wirtschaftsforschung, *Die Deutsche Industrie im Kriege 1939-1945*, Berlin 1954, p. 17. Statistisches Bundesamt, *Statistisches Jahrbuch*, 1955 and 1959, Stuttgart.

influence just mentioned. An extensive and complicated system of tax exemptions influenced the direction of flow of investible funds, and probably to some degree also the volume of that flow.⁴ Many features of this system of tax incentives persist up to the present time. The tax exemptions granted between 1949 and 1957 for purposes of capital formation have been estimated at 28 billion DM.⁵ There can be no doubt that this was effective in changing and directing the preferences of private investors.⁶

In addition to fiscal policy measures, West German public authorities influenced resource allocation in other selective ways. Two cases will suffice. In 1952, under the "Investment Aid Law" business in general was forced (not induced by tax legislation) to supply one billion DM, which was channeled into bottleneck sectors, especially mining and the iron and steel industry. Occasionally monetary policy was also used selectively, particularly to favor exports. Between 1951 and 1957 more than 1.5 billion DM export drafts were each year rediscounted by the Central Bank at special rates, lower than the West German discount rate for internal transactions, in order to help business exporting to countries in which discount rates were lower than the domestic ones.

If one takes into consideration fiscal and monetary policy, the often-heard argument that high interest rates prevailed during most of the period of rapid growth seems unconvincing. Many interest-sensitive sectors could obtain low-cost funds from public authorities, e.g., residential construction. In other cases, borrowers were permitted to issue low-yield securities with tax exemptions. Depending on the marginal tax rate of a lender, the effective yield of such a security could be high. It is well known that in the West German capital market an intricate structure of effective interest rates existed—together with a nominal interest rate, which was substantially below the bank rate—all determined to a large extent by fiscal authorities. This disappeared only with the so-called "liberation of the capital market" in 1954-1955, with the increase in private investible funds made possible by rising income and a substantial reduction of fiscal interference in the capital market in 1955.

Most of the selective fiscal measures which were adopted in order to overcome the many and serious bottlenecks have now been abolished. West German authorities considered them as temporarily necessary for the quick restoration of structural balance. Whether West Germany would have regained this as speedily as it did without public influences is questionable. Had

⁴ Space limitations preclude any discussion of this system.

⁵ This figure includes the tax exemptions granted under paragraphs 7a to 7f, 10, 10a, 32a of the West German income tax law; further, paragraph 36 of the "Investment Aid Law," paragraphs 3 and 4 of the "Law to Favor Exports" and finally provisions under the "Law to Favor the Capital Market."

⁶ A clever man well acquainted with all tax exemption possibilities could substantially increase his profits after taxes. In 1957 West Germany was astonished to learn about the notorious Oetker case, a manufacturer of baking powder, who through a skillful use of all legal tax exemption possibilities managed to build up a whole fleet of freighters with an estimated value of 400 million DM. Profits channeled into ship building were tax favored. *Der Spiegel*, December 18, 1957, West German magazine.

resource allocation occurred entirely according to competitive free-market forces, funds might not have flown in volume into the sectors vital for future growth, but for a considerable time relatively unprofitable. The interferences with private preferences were deeply deplored by many Germans—particularly the absence of a “free” capital market.

West German postwar growth between 1948 and 1957 cannot be explained simply by a “high degree of competition.” It is necessary to take into consideration the many structural difficulties which the country faced and the public policy measures adopted to overcome them. These constituted a massive “interference” with competitive solutions.

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Competition and Growth—The Lesson of West Germany: Comment

The Keynesian problem typically arises where effective demand or the rate of increase of demand is insufficient to utilize fully all the available factors of production, and it may almost be defined as a situation where an increase in demand would lead to an increase in output with little or no rise in the price level. The postwar era in Europe has seldom been faced with this problem. It is therefore hardly surprising that, with the Keynesian problem of unemployment largely solved, a chief means of increasing output should be to improve the allocation of resources. Thus far it is difficult to disagree with Egon Sohmen [2]. However, his main thesis—that the rapid growth achieved in the West German economy can be attributed firstly to the degree of competitiveness achieved by virtue of Allied “trust busting” and secondly to the orthodox monetary policies pursued by West German governments—is much more difficult to accept.

In view of all the clamor about the German “miracle” of postwar growth, it would be well to keep this achievement in proper perspective. The few figures in Table 1 suggest that, swift as the growth in real gross national product was, it is not relatively so striking when allowances are made for increases in population and working force.¹

The third column indicates how much of industrial growth was not attributable to the increase in the labor force. Here Germany's performance appears much less spectacular, and on this basis she ranks lower than Austria, France and Italy.

In Sohmen's judgment the linkage of greater “competition” with “more efficient resource allocation” and hence with growth, is quite firm.

Monocausal explanations of unusual phenomena are always suspect, and

¹ Reliable figures are available only from 1950 onward. As Sohmen notes, the antitrust and anticartel policies are rapidly fading, and so 1957 was chosen as the terminal year. The important years 1948-49 are not included, whereas the Korean boom period is, but this span seems to be the most suitable one. All growth rates are compound ones.

TABLE 1—VARIOUS INDICATORS OF RATES OF GROWTH, 1950-1957

Country	(1) Average Annual Rate of Growth of Real Gross National Product	(2) Average Annual Rate of Growth of GNP per capita	(3) Average Annual Rate of Growth of Indus- trial Production per Industrial Employee
West Germany	8.0	6.8	5.0
Greece	7.4	6.5	—
Austria	6.7	6.4	5.6
Turkey	6.3	3.5	—
Italy	5.8	5.2	6.9
OEEC Countries	4.7	3.8	3.9
Switzerland	5.3	3.9	—
France	4.8	3.9	5.6
Netherlands	5.0	3.6	3.5
Canada	4.6	1.9	3.7
Portugal ^a	4.4	3.5	—
United States	3.7	1.9	2.1
Sweden	3.3	2.4	3.0
Belgium	3.3	2.8	—
United Kingdom	2.5	2.3	1.9
Norway	—	—	4.7
Denmark	2.2	1.5	—
Ireland	1.5	2.0	1.4

^a 1952-1957.

Source: OEEC [5. Jan. 1960]. A dash (—) indicates that data were not available, for one reason or another.

rightly so. With all due respect to all other factors, biological, sociological and metaphysical, the conclusion is nevertheless inescapable that only more efficient resource allocation can explain the difference between West Germany's rate of growth and that of other countries in a comparable state of development [2, pp. 993-94].

The hypothesis that the attempt at strengthening competition deserves principal credit is here advanced with all due caution, but the evidence in its favor seems overwhelming [2, p. 994].

As we know, it is a tricky thing to bring about more competition, especially in a world where rather large firms abound. Whether or not changes in the market structure alone will intensify competition is still a wide open question. (Nor is there yet a wholly satisfactory measure for "competitive" behavior and conditions.) If competition in Germany had been strengthened, it would be of greatest interest to have this demonstrated scientifically, especially since so much of Sohmen's hypothesis rests on it.

For evidence on this point, Sohmen simply refers to the Allied policies, as described in one study [6]. Although it has become customary to minimize the effects of these policies, Sohmen offers no new evidence to set against, say, Wallich's and Hansen's conclusions.² Sohmen apparently holds that it

² "Taking all measures together, one cannot help concluding that the effort of the United States to remake Germany in its own antitrust image has met with rather limited success"

was the increase in competition, not the level of it, that spurred growth in Germany and might do so in other countries. (He could hardly contend that competition in Germany during this period was in some sense greater than in, say, the United States.) In the present instance, this seems to mean that rather a little increase (so far as can be judged) in competition brought about rather a lot of growth. This is an ambitious hypothesis, and deserves the most careful research. In particular, it would be helpful to know in which markets the increased competition was correlated with greater growth. Most of all, one would want to know just how—by what mechanism—the enhancing of competition is linked to greater investment and output.

Sohmen relies heavily on the method of argument which eliminates all possibilities until the favored explanation is left. This is an adequate method only if all possibilities are covered and the ones rejected are eliminated for sound reasons. In this article it seems doubtful that Sohmen has fulfilled either of these conditions. His list of alternative explanations is rather incomplete, including foreign aid, "German enthusiasm for hard work, the docility of German workers and the weakness of German unions that made a low wage level possible" [2, p. 991]. Nowhere does he come to grips with either Wallich's [3], Hansen's [1], or the British survey team's [6] array of reasons.³ It can hardly be said that increased competition is the sole remaining element, when the flow of immigrants, the relative fortunes, misfortunes and distresses of rival economies, the special buoyancies of a genuine reconstruction, and various structural changes within the economy remain to be considered.

For example, Sohmen neglects the rather important factor shown in Table 2, that the working force steadily increased as a proportion of the total population. During roughly the same period the figures for the United Kingdom show an increase in working population to total population of only about one per cent. A relative increase in this proportion would, on its own, go a long way to explain Western Germany's relatively faster rate of growth, and bears out column 3 of Table 1. If this point is to fit the Sohmen thesis it must be shown to stem from the increase in competition rather than from other factors, such as the influx of refugees and changes in social attitudes to the employment of women.

[3, p. 383]. Also: "The deconcentration of the commercial banks has probably been the least effective of the major Allied efforts" [3, p. 387]. As for Hansen: "Germany historically is par excellence the country of cartels, and while the American Military Government made some effort to weaken the hold of cartels, the effort has met with relatively little success, and such success as there was appears to be rapidly vanishing" [1, p. 12-137].

³ Wallich, who stresses three broad areas of "factors": (1) the conditions of the cold war, (2) geography, resources, immigrants, and the like, (3) policies of the Allies and Germans, concludes, "A great many circumstances and events beyond German control helped, and the respective roles played by good management and good luck are not easy to unravel" [3, p. 3].

Hansen, in de-emphasizing even the role of German "low-pressure" economic policy, stresses the "underlying real factors," most of which Sohmen does not bring in [1, p. 17].

The O.E.E.S. team gave a major emphasis to such deliberate policies as investment allowances and the famous investment assessments and the relatively major role of *nonmarket* investment in contrast to those who stress the beneficial effects on investment allocation of monetary policy alone [6, p. 59-69].

TABLE 2—WESTERN GERMANY'S INCREASE IN LABOR FORCE

Year	Ratio of Working Population to Total Population	Ratio of Numbers Employed to Total Population
1949	31.4	28.8
1950	32.3	28.9
1951	33.1	30.1
1952	34.3	31.4
1953	34.9	32.3
1954	35.9	33.4
1955	36.7	34.9
1956	37.7	36.2
1957	38.1	36.8

Source: Deutsche Bundesbank [4] and United Nations [7].

Sohmen acknowledges that the policy on investment allowances (surely an interference with market parameters) helped stimulate investment, but mentions it only when disassociating this tax policy from functional finance. When one adds the Reconstruction Loan Corporation, the investment assessments, and other legislation affecting the capital market [6, pp. 59-69], one must ask if there were not significant deviations from "free market" policies on investment.

On the whole a few general conclusions seem to follow from the varying views put forward by Wallich, Hansen, Sohmen and others. These are that an open economy must be wary of inflation; that high investment will probably bring rapid growth; and that a large and prolonged flow of immigrants will soon, if work is found for them, enhance the GNP though not necessarily GNP per capita. These conclusions are scarcely revolutionary.

Sohmen's digs at "post-Keynesian orthodoxy"—whatever this means—seem largely irrelevant to the main issues of trust-busting and competitiveness which he raises. An amplification of his views on post-Keynesian policies in general would be welcome. We offer no alternative single explanation in this paper for the German boom. The burden of proof lies, we think, upon Sohmen to disprove the more familiar explanations and to establish more fully the importance of his "monocausal" connection. Nonetheless, in re-opening this fascinating and complex area of controversy, Sohmen has boldly and lucidly challenged a consensus which may have needed some skepticism.

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Competition and Growth—The Lesson of West Germany: Comment

Sohmen explains the German growth by reference to the forces of competition. Few would deny that competition may spur growth. But Sohmen's dismissal of another—and some would argue more powerful—factor cannot be left unchallenged. This is simply the very high ratio of capital formation to gross national product in the Federal Republic.¹ If the United Kingdom and the Federal Republic are taken as two economies in similar stages of economic development, the difference is striking. Over the years 1950 to 1958 inclusive, gross domestic fixed capital formation (in machinery and equipment) took 8.4 per cent of the United Kingdom's gross national product at factor cost in 1954 prices, and no less than 13.1 per cent of that of the Federal Republic. It would be surprising if this factor alone did not account for most, if not all, of the difference between the rates of growth in the two countries.

These figures conceal the really important ones, viz. the amounts of resources devoted to *net* investment. One would expect that the ratio of replacement investment to gross investment would be much lower in Germany than in the United Kingdom since the war, simply because of much greater destruction and dismantling of existing equipment during and immediately after the war. The data support this view. In the years 1954 to 1958, the proportion of replacement investment in gross investment in Germany averaged 42 per cent, and in the United Kingdom 54 per cent [2] [3]. Thus a given volume of real resources devoted to fixed capital formation in these years produced a much larger *addition* to existing production facilities in Germany than in the United Kingdom. One would guess that this was an even stronger tendency in the earlier years of the 1950's.

How was it done? One can conveniently distinguish between the factors that *caused* this high rate of capital formation, and those that *permitted* it. The former are obvious—the need to reconstruct, the desire to satisfy the compelling export demand for the sort of goods Germany can supply, the common urge of businessmen to control larger rather than smaller units, and so on.

¹ Sohmen's implied refutation of this view by taking Norway as an example of a country with an even higher rate of capital formation but a lower rate of economic expansion, of course, fails. As a matter of policy, the Norwegian authorities, faced with an economy possessing limited natural resources, have deliberately promoted investment in shipbuilding and public utilities, both with a high capital-output ratio. If there had been other more "profitable" outlets, they would doubtless have been preferred. The German economy was fortunate in possessing such outlets at a time when world markets, especially in Europe itself, were clamoring for just such products as Germany had always produced.

These are common to both the United Kingdom and Germany, though no doubt operating with different strengths at different times. It is not clear why Erhardian competition alone should have been a potent factor.

Much more important in explaining the difference between the two economies are the factors which *permitted* the high level of capital formation in Germany. What other demands on the GNP were lower in Germany than in the United Kingdom? Three spring to mind—nonproductive (social) investment, especially housing and construction, defense, and personal consumption. Sohmen looks at these demands on Germany's GNP and argues that the first was very large (and in particular larger than in the United Kingdom), the second was admittedly lower than in the United Kingdom (although not all that much), and the third was not low by German standards (which of course proves nothing).

To take nonproductive investment first. The OEEC tables of national accounts do not distinguish between residential and other construction, but these two together tended to fluctuate around 10 per cent of Germany's GNP at 1954 prices, compared with about 7 per cent in the United Kingdom. This difference arises mainly from the very different housing needs in the two countries.

When we come to defense expenditure, a similar quantitative difference occurs, but in the opposite direction. Over the years 1950 to 1958 inclusive, defense expenditure accounted for 4 per cent of Germany's GNP, and 7.7 per cent of the United Kingdom's. Sohmen admits to a difference of this order, but apparently believes it to be of no importance. This is understandable since of course it tends to undermine his case.

If we add together the demands made on the economy by the needs of construction and defense, we find that between 1950 and 1958 the annual average in West Germany stood at 13.6 per cent of GNP, and in the United Kingdom at 14.5 per cent. Thus these two "unproductive" demands required one per cent more of available resources each year on average in the United Kingdom than in West Germany. Had these additional resources instead been available for investment in machinery and equipment in the United Kingdom, the level of capital formation there could each year have been more than one-eighth higher.

This is not a very large difference. The crux of the matter comes when we examine the proportion of resources used for private consumption in the two countries. Between 1950 and 1958 inclusive, private consumption in West Germany amounted to 59.4 per cent of the GNP at market prices. In the United Kingdom, it amounted to 67.2 per cent. I imagine that Sohmen believes he has answered this point by his sentence: "... consumption was not unusually low in West Germany by prewar German standards" [1, p. 992]. This is true—in 1936, the Nazi government allowed private consumption to absorb only 59 per cent of the GNP. But it is also irrelevant. The point is simply that German consumption levels are very low by European standards. The only other European economy where consumption expenditure accounts for so low a proportion of the GNP is the Netherlands (60.5 per cent in 1958). The rest range from Norway (62 per cent) to Austria, France and

Italy (each 66 per cent) up to Portugal (83 per cent) [5, pp. xii-xxviii]. It is this very low average propensity to consume which is clearly of supreme importance in explaining the ability of the German economy to invest such a high proportion of its resources, and consequently to grow at such a fast rate.

Sohmen dismisses this view, and adds that in any case it would require its holder to accept Say's Law. This is of course a nonsequitur. If autonomous factors are strong enough—and in postwar Germany they have been—then freeing resources by restricting consumption will increase investment, not depress it.

This is neither to condemn nor to praise the fact of a low consumption level as such. It was simply a means to an end. But it was also the result of other factors. The most important of these has been the low share of wages and salaries in the national income. Between 1950 and 1958 inclusive, the share of wages and salaries, after the receipt of public transfer payments and the payment of taxes on incomes and insurance contributions, in the GNP at current prices averaged 58 per cent in the United Kingdom, and just over 47 per cent in West Germany [2, p. 5] [3, p. 15].

It is not difficult to find a reason for this low share in West Germany. There is a clue in the rise in this share in the last few years, and especially in 1957, 1958 and 1959. These are the years when the level of unemployment began to approach what most other countries would regard as tolerable. Until these years, the German trade unions were in a very weak bargaining position. The currency reform of June 1948 denuced them of funds (along with all other holders of liquid balances), so that they were quite unable to finance strikes. The steady inflow of refugees from the East, coupled with and contributing to the high level of unemployment, greatly undermined their bargaining position. And finally the German neurosis about inflation no doubt damped down any ardor left. It is not surprising, therefore, that wages and salaries should have been so low a proportion of national income.²

My conclusion therefore is that Sohmen's case is illfounded, and misleading both as a lesson (which it was his aim to provide) for policy and as a guide to the future of West Germany. Now that full employment has been reached and the inflow of labor from the East has dropped, the share of wages and salaries in the national income is likely to rise, and so too is the proportion of consumption expenditure. It will then become much more difficult to induce German consumers to abstain from present consumption so nobly as they have done in the past. If German firms continue to possess the same urge to expand (which has in the past had little to do with Erhardian competition, and much more to do with ample labor supplies and very strong overseas demand) then the authorities will be faced with the familiar "British" problem of excess demand. It will then become very difficult for the German economy to main-

² Sohmen's arguments trying to demonstrate that German trade unions were on the contrary, militant and successful [1, pp. 991-92] are extremely feeble. Indeed his admission [1, p. 991] of their "self-restraint in matters of wage policy" is precisely my point here.

tain its rate of growth of the early 1950's. This is already the case in 1959-60.

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Competition and Growth—The Lesson of West Germany: Reply

It is highly gratifying to be able to conclude after reading the three comments that the principal theses of my article remain virtually unscathed. This is not to deny the merits of my critics, nor to assert that this exchange will settle the complex issues involved. The present discussion should amplify and clarify a number of points that could only be inadequately discussed in the limited space of a journal article.

1. A remarkable human tendency to explain what happened *ex post facto* as having been bound to happen under the given circumstances frequently makes people advance certain factors as *causes* of the high German growth rate which before and during the event were almost unanimously accepted as formidable *obstacles* to its realization. It seems at least as natural to assume that the millions of destitute refugees were, on balance, as much a drain upon as a gain to the German economy, and to hold that their urgent needs should have made the achievement of a high rate of investment, a prerequisite for their useful employment in an economy starved of capital, considerably more difficult.

Apart from the fact stressed by MacBean and Shepherd (Table 1) that part of the increase in industrial production was due to the growth of the labor force, the more modest, but still remarkable, increase of labor productivity in industry—an average of 5 per cent per year from 1950 to 1957—also reveals that the explanation of the German growth rate on the basis of the "special buoyancies of a genuine reconstruction" (p. 1020) is not as well founded as commonly believed, at least for the years after 1950. While they stress the growth of the *total* labor force, MacBean and Shepherd fail to add that one of the main reasons for their result is the much faster relative growth rate of employment in *industry* at the expense of other sectors.¹ The

¹ This was stressed in my article [4, p. 990, n. 13].

observation that much of West Germany's growth is explained by a swift reallocation of resources on a large scale (with labor productivity *within* each sector increasing at an impressive, but not exceptionally high rate) is surely not in conflict with my basic thesis.

But even the view that a substantial increase in the total available labor force must necessarily produce the high aggregate growth rates observed in West Germany need not, I believe, be refuted in detail. The degree of flexibility required to perform this task successfully is precisely what needs explanation. Further explanation is also needed for the increase in the ratio of working to total population. I suggest that this increase reflects the fact that incentives to work have increased at a remarkable rate, a factor that is surely not independent of West Germany's economic policies.

2. Roskamp as well as MacBean and Shepherd dispute my contention that the West German economy was highly competitive during the crucial years. Roskamp's disagreement is mainly due to his belief that competition necessarily implies consumers' sovereignty. At least in theory, an economy can conform to the model of perfect competition when a large proportion of the means of production is collectively owned, or when the exact composition of final demand is determined dictatorially by planners' preferences. Similarly, neither sumptuary taxes nor governmental subsidies need make the market for the commodities concerned any less competitive, if we follow the usual definition of the degree of competition as the degree of control of individual producers over the prices of their products. On the other hand, it is also possible (and true more frequently than not) that an economy may be ridden with imperfections while its consumers enjoy perfect freedom of choice. Roskamp's examples do not, therefore, in any way contradict my basic thesis.

While there is not a single passage in my article that would suggest that the West-German government does not take an active role in economic affairs, it is nevertheless generally admitted that there has been much less government intervention in West Germany than in most other European countries during the postwar reconstruction period. An important characteristic of West German policies has been, moreover, an emphasis on *marktkonforme* measures rather than direct controls; in other words, a preference for the carrot rather than the stick. In many instances mentioned by Roskamp, government interference only tended to correct serious maladjustments whose direct elimination would have been politically unfeasible. This can be said of the subsidies for residential construction in conjunction with the taboo concerning rent controls, for example, or of the investment aid for the basic industries where price ceilings continued after 1948. These measures undoubtedly tended to guide resources more closely *toward* the uses indicated by consumers' preferences rather than away from them.

One can hardly argue (this argument is liable to be read into Roskamp's comment) that export promotion has accelerated West Germany's rate of growth. An economy endeavoring rapid reconstruction would obviously do better to keep its resources at home (or even to borrow abroad) rather than artificially to encourage their exportation.²

² Incidentally, if Opie had added the export surpluses to what he calls the "unproductive"

MacBean and Shepherd contend that I have not established my claim that competition (on whose definition we seem to see eye to eye) was stronger in West Germany than elsewhere. I do not see any more hope of establishing *quantitative* measures of degrees of competition than they do. Let me repeat once more, however, that measures whose radicalism is without precedent were undertaken in West Germany to break up concentrations of market power and to prohibit restraints of trade [4, p. 987]. In addition, West Germany's commercial policies are generally recognized to be among the most liberal in the world, and the country has enjoyed the rare experience of having a supremely energetic Minister of Economic Affairs who has usually thrown his weight in the balance whenever competition appeared to be seriously threatened. The behavior of labor unions was generally such that labor markets may not have been too different from what they would have been had competitive conditions prevailed. It seems difficult to maintain that all this did *not* significantly enhance competition. At least labor markets must be judged to be significantly less competitive in the United States.

The three contributions of my critics are not the first instance in which I have detected a misunderstanding of the true significance of efficient resource allocation in a competitive economy. In the eyes of many people, competition seems to be associated merely with the choice of one particular point among those available *on* a society's production possibility locus. What counts at least as much is that allocative distortions can make an economy operate far *below* this locus as a result of imperfections in the markets for factors and intermediate goods, even though all resources are employed to the fullest extent.

3. The role of investment is the issue most hotly contested by all four critics. On reading the three comments, one is liable to conclude that West Germany was the European country whose government attempted most forcefully of all to stimulate capital formation by artificial incentives, or where a larger proportion of investment was directly or indirectly controlled by the government than elsewhere. For anybody with a knowledge of the European scene, this conclusion (which was not intended by my critics, I hope) is patently absurd. With few exceptions, other governments have exercised more direct control over the allocation of investment funds. West Germany, in contrast to many other European countries, did not launch an extensive nationalization program after the second world war. Germany did not have a Monnet Plan, or a Capital Issues Committee of the British type; nor did any prominent German economist find cause to complain, as Roy Harrod did in England, that "the Government . . . has put into operation excessive capital programmes" [2, p. 29]. It is for the simple reason that most other governments have practiced much tighter systems of investment control, and that many of them have fared rather badly with them (some with lower, some even with higher *ex post* investment ratios than West Germany) that I did not present this aspect as a distinct factor in the German growth process.

claims on the national product, construction and defense, his conclusion that the United Kingdom was in a less favorable position than West Germany with respect to the availability of resources for domestic capital formation would have been reversed.

The observations of my critics make rather strange reading in the light of the severe criticism of German investment policies by the economists in the U. N. Economic Commission for Europe:

The lack of balance in the expansion can be explained by the lack of any coherent investment policy. In western Germany, the determination of the direction of investment has been left very largely to individual entrepreneurs. . . . Fixed investment, at 20 per cent of total available resources, has been relatively high, but the shares of transport, mining, steel works, public utilities and agriculture in the total have been abnormally low.

Thus if the Government of western Germany is to get rid of its depressed areas it will, as is now generally admitted, have to assume considerably greater responsibility than hitherto for redirecting investment into channels where it can fructify for the benefit of the community [5, 1954, p. 151].

For proper perspective, Table 1 presents average ratios of gross domestic fixed investment to gross national product for the OEEC countries, the

TABLE 1—AVERAGE RATIOS OF GROSS DOMESTIC FIXED INVESTMENT TO GROSS NATIONAL PRODUCT, 1950-1957 (PER CENT)

Norway	28.2	Denmark	17.5
Iceland	26.9	France	16.7
Canada	23.2	Belgium	16.3
Switzerland	23.1	United States	16.6
Netherlands	22.0	Greece	14.5
Luxembourg	21.2	Ireland	14.5
Austria	21.0	Portugal	14.0
West Germany	20.6	United Kingdom	14.0
Italy	19.7	Turkey	13.8
Sweden	19.3		

Source: OEEC [3].

United States and Canada. It shows that the German ratio is matched or surpassed by a number of countries. If the British ratio has been abnormally low, it was certainly not for lack of effort. While Opie expounds on the low West German consumption levels, we remember that "austerity" was the catchword of British, not of German, economic policy for many years, and that nobody in Germany has felt moved at any time during Erhard's tenure of office to wonder "Are These Hardships Necessary?" The statistics seem to show to the careless observer that the British people frolicked frivolously during the postwar years by devouring the lion's share of its national product, but the impression of most residents of the United Kingdom was, on the contrary, that they were living miserably.³ Germans, on the other hand,

³ "For several years the British people have been accustomed to see their consumption rise by much less than the national output" [5, 1950, p. 155].

"Here at home, as the forties ended, Britain was living on a ration standard of three ounces of butter and one egg a week." "Farewell to the Fifties," *The Economist*, December 26, 1959, pp. 1219-21.

remember a substantial rise in their living standards precisely after the currency reform of 1948. Consumption in West Germany *did* rise phenomenally after 1948: The average annual increase in real per capita consumption between 1950 and 1959 was 6.3 per cent. The average rate of increase for Britain, on the other hand, was 1.8 per cent. On two occasions, 1950/51 and 1951/52, per capita consumption in the United Kingdom actually *fell* by 1.9 and 1 per cent, respectively [3]. In spite of the upsurge of personal consumption in West Germany, its proportion to gross national product remained low and was even falling slightly. For the share of personal consump-

TABLE 2—AVERAGE ANNUAL RATE OF INCREASE OF REAL HOURLY WAGE RATES
(Index of wage rates divided by cost-of-living index)

	1950-1953 (per cent)	1950-1957 (per cent)
France	5.2	5.9
West Germany	6.0	5.3
Canada	5.2	3.7
Sweden	3.8	3.6
Belgium	2.6	3.4
Netherlands	1.8	2.9
United States	2.1	2.7
Austria	4.0	2.5
Denmark	2.8	2.2
Norway	2.9	2.1
United Kingdom	0.5	2.1
Italy	0.6	1.4
Switzerland	0.0	0.4
Ireland	0.6	0.14

Source: OEEC [3].

tion to fall, it was not necessary to "induce German consumers [nobly] to abstain from present consumption," as Opie puts it (p. 1024). All that was required was for the gross national product to rise even more sharply and for the well-established Duesenberry-Modigliani-Friedman "ratchet effect" to occur.

With some astonishment I read that I was "trying to demonstrate that German trade unions were . . . militant and successful" (Opie, p. 1024 n.). I wrote, on the contrary, that "it cannot be disputed that unions did not bargain for higher wages with quite as much determination as they did in other western countries. Whatever the reason . . . the result would have been the same had it indeed been weakness. . . ." [4, p. 997]. I continued with an unblushing endorsement of Henry Simons' position on labor unions and thought I had made it fairly obvious that I considered the behavior of West German unions during the recovery period reasonably close to a competitive pattern. As to the alleged lack of success of German workers in achieving wage increases, Table 2 shows comparative figures on the development of real wages in some important countries. One need not argue weakness to explain the

fact that German workers were more than lukewarm about engaging in labor disputes in view of the remarkable increase in their earnings.⁴

I had used the example of Norway, the country with the highest investment ratio in the Western world, to point out that a high level of capital formation may fail to guarantee a satisfactory rate of growth if resources are misallocated sufficiently seriously [4, p. 992]. According to Opie, this example "of course, fails" (p. 1022 n.). Again, natural factors rather than policies explain the phenomenon. In this case, a shortage of natural resources is invoked. One wonders why Switzerland, for example, a country with even more limited natural resources, could achieve a higher growth rate with only four-fifths of the Norwegian investment ratio during the 1950's.

4. My critics profess not to appreciate the bearing of my remarks on "post-Keynesian orthodoxy" on the issues under dispute. The past twelve years have seen a continued campaign of condemnation of the West German government for its alleged failure to engage in sufficiently expansionary policies. Opie explicitly concurs in this criticism by remarking that only in 1957, 1958, and 1959 did the level of unemployment in West Germany begin to "approach what most other countries would regard as tolerable," and by poking fun at "the German neurosis about inflation" (p. 1024). At the same time he maintains, however, that "if autonomous factors are strong enough—and in postwar Germany they have been—then freeing resources by restricting consumption will increase investment, not depress it" (p. 1024; my italics). MacBean and Shepherd assure us likewise that "the postwar era in Europe has seldom been faced with [the Keynesian] problem" and that, presumably also in Germany, "the Keynesian problem of unemployment [was] largely solved" (p. 1018). These assertions may again be contrasted with what the U. N. Economic Commission for Europe had to say during the event [5, 1949, pp. iv, v, 67; 5, 1950, p. 150].

Judged by all the available evidence, "post-Keynesian orthodoxy," which I would define as the advocacy of expansionary monetary and fiscal policies to secure full employment under any and all circumstances, all too frequently combined with little regard for the allocation of resources, has already proved beyond doubt to be an unambiguous failure, although I am fully aware that, having acquired some of the characteristics of a genuine ideology, it is assured of a long lease on life. To avoid the danger of being misunderstood, I want to emphasize as forcefully as possible that opposition to "post-Keynesian" remedies does not, of course, imply an endorsement of unemployment as an alternative. First of all, changes in the institutional setting and the structure of markets are to some extent alternative means of countercyclical policy.

⁴ Opie's claim that the share of wages and salaries in West Germany's national income is abnormally low is based on a comparison with the United Kingdom. I stressed [4, pp. 991-92] that while the share of wages and salaries before deductions is lower in Germany than in the United Kingdom, it has been the second highest in Europe. To quote the share after deductions gives a somewhat misleading impression since the social security system is more highly developed (and, be it noted, strongly endorsed by the union movement) in West Germany than in most other countries. The bulk of the deductions therefore has to be regarded as a legitimate part of workers' real incomes.

With very few exceptions, they go unrecognized in the standard presentations of the Keynesian framework, or are forgotten, at any rate, when it comes to policy recommendations. Secondly, it should have become obvious that expansionary monetary and fiscal action may under certain conditions become badly blunted and will in the long run lead to serious allocative maladjustments in some institutional settings widely found in the real world. Greater market pressure on administered prices and the elimination or at least softening of oligopolistic warfare in labor markets—in short, measures to intensify competition—are not only, I submit, indispensable conditions for the efficacy of the customary macroeconomic policy tools, but will themselves go a long way in damping cyclical fluctuations.

Neo-liberal policies have constantly increased in popular appeal over an extended period (not only in Germany) and are now being endorsed by many who were formerly among their harshest critics. Earlier than I would have dared to predict, the ideological metamorphosis of German socialism sketched in my article [4, pp. 1001-2] has produced one of the most remarkable documents of our time, the *Godesberger Programm*, drafted and accepted by an overwhelming majority at an extraordinary party congress in November 1959. I would like to close with a few quotations from it:

Free consumers' choice and free choice of one's place of work are important foundations, free competition and free entrepreneurial initiative are important elements of social-democratic economic policy. . . . Totalitarian economic regimentation destroys freedom. The Social Democratic Party therefore endorses free markets wherever genuine competition exists. Where markets become dominated by individuals or groups, on the other hand, a variety of measures may be called for to preserve freedom in the economy. Competition as much as possible, planning as much as necessary! [1, pp. 13-14].

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International Trade and Economic Expansion: Comment

In a recent article in the *American Economic Review*, Mr. J. Bhagwati¹ examined the effects of economic expansion on the terms of trade of an ex-

¹ J. Bhagwati, "International Trade and Economic Expansion," *Am. Econ. Rev.*, Dec. 1958, 48, 941-53.

panding economy. The analysis ran in terms of two countries (I and II) and two commodities (X and Y), and it was assumed that only one of the countries (I) experienced growth. Bhagwati concluded that "... in order for the terms of trade to turn adverse to the growing country it is not sufficient that the income effects of the expansion should be unfavorable and should create an increased demand for imports . . ." (p. 945). If the foreign elasticity of the supply of exports (r_m) should be negative and sufficiently large in magnitude, it is possible, according to Bhagwati, for the terms of trade to turn in favor of the expanding country.

Bhagwati's conclusion is correct, within the confines of the model, as a general statement covering cases of stable and unstable equilibrium in foreign trade. If the analysis is restricted, however, to cases in which foreign trade equilibrium is stable, the negative values r_m can take on are not sufficiently large to turn the terms of trade in I's (the growing country's) favor when the income effects of expansion create an increased demand for imports. The remainder of this note is an amplification of the last statement and is a supplement to Bhagwati's analysis.

From a well-known relationship we know that country II's elasticity of demand for X with respect to the terms of trade (e_{dx}) minus unity equals II's elasticity of supply of Y with respect to the terms of trade; i.e., $e_{dx} - 1 = r_m$. The familiar stability criterion is, $e_{dx} + e_{dy} \geq 1$, where e_{dy} stands for country I's elasticity of demand for Y with respect to the terms of trade. We can substitute $(1 + r_m)$ for e_{dx} and rearrange the stability criterion to read, $r_m + e_{dy} \geq 0$. Country I's elasticity of demand for Y with respect to the terms of trade (e_{dy}) involves the substitution and income effects of a change in terms of trade on consumption of Y in I and the effect of a change in the terms of trade on the production of Y in I. It involves, in other words, Bhagwati's ϵ , σ , and E_{DY}' . We can lump together these effects and obtain the change in imports of Y stemming from a change in the terms of trade, dM_t , in the following expression:

$$(1) \quad dM_t = -e_{dy} \cdot \frac{M}{p} \cdot dp,$$

where E_{DY} is the output elasticity of demand for Y goods and E_{SY} is the output elasticity of supply of Y goods, and p representing a worsening of country I's terms of trade.

We can now proceed to determine the effect of economic expansion in country I on its terms of trade. Following Bhagwati we write the income and output effects of expansion on the demand for imports, dM_E , as

$$(2) \quad dM_E = (C \cdot E_{DY} - Y \cdot E_{SY}) \frac{dK}{K},$$

where E_{DY} is the output elasticity of demand for Y goods and E_{SY} is the output elasticity of supply of Y goods (both at constant terms of trade), C is the initial quantity of importables consumed in I, Y is the initial production of importables, and $\frac{dK}{K}$ is the expansion of I's productive capacity. The

sum of equations (1) and (2) gives us the total change in the import demand for Y goods in country I, dM_D , which must in equilibrium equal the change in the supply of Y goods from country II, dM_S , which can be expressed as:

$$(3) \quad dM_S = \frac{M}{p} \cdot r_m \cdot dp.$$

Setting (1) + (2) = (3) and solving for dp , we obtain:

$$(4) \quad dp = \frac{(C \cdot E_{DY} - Y \cdot E_{SY}) \frac{dK}{K}}{\frac{M}{p} (r_m + e_{dy})}.$$

If $C \cdot E_{DY} > Y \cdot E_{SY}$, the terms of trade will turn against country I unless the sum of r_m and e_{dy} is negative. But we have already shown that r_m and e_{dy} must sum to zero or greater if we consider only those cases involving stable equilibrium. Thus, in the case of single-country expansion, starting from a stable equilibrium, the change in that country's terms of trade depends solely on the relative magnitudes of $C \cdot E_{DY}$ and $Y \cdot E_{SY}$.²

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² An alternative way of expressing the conclusion reached above is as follows: The denominator of Bhagwati's equation (7) (p. 945) is the amount by which a deterioration of I's terms of trade improves its trade balance; if the denominator is negative, so that a deterioration in the terms of trade worsens the balance, then the initial equilibrium was unstable.

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BOOK REVIEWS

General Economics; Methodology

International Economic Papers, No. 9. Edited by A. T. PEACOCK, R. TURVEY, W. F. STOLPER and E. HENDERSON. New York: The Macmillan Company, 1959. Pp. vii, 244. \$4.75; \$3.92 for A.E.A. members.

This marks the ninth volume in the series of fine translations sponsored by the International Economic Association. Like its predecessors it carries the distinctive blue cover and maintains the clear printing standards to which we have grown accustomed—along with a relatively high price. A translation of reviews of Scandinavian books is a new feature. This could initiate another worth-while service by the imaginative editors of the publication.

Contents in these collections, like a blind date (from a select list, however), are usually unpredictable though always of more than passing interest. Once again the offerings go back and forth in time, requiring some perspective in fitting the pieces into a proper setting.

First is an essay by Francesco Guicciardini, an unknown to all but very few prospective readers. Described as a Florentine historian and statesman (1483-1540) he is represented by a 12-page selection titled *Two Discourses on Progressive Taxation of Land Incomes*. Because of his remarkable quality of analysis, considering its date, this article is likely to be most welcome to historians of economic thought, and intriguing to all in view of its pro and con method of argument. Almost all the commonplace political, sociological, economic, and moral judgments seem to be mustered here, on the one side or the other, reminding one of Bastiat's *Petition from the Manufacturers of Candles*. It is a melancholy thought that standards of debate seem not to have improved much through time.

Next comes an 18-page chapter from Karl Schlesinger's book *Theorie der Geld*, published originally in 1914. Of the author, an Austrian banker, Schumpeter has written that this is another instance confirming that "in our field first-class performance is neither a necessary nor a sufficient condition of success." While the analysis by now is slightly "dated"—though the clarity of ideas and position mark it as a strong effort compared to even much current thinking on the influence of money on prices—the translation adds materially to our knowledge of earlier work of continental authors. In Howard Ellis' excellent and rather complete account of *German Monetary Theory, 1905-1933*, Schlesinger gets only a passing nod, for, Ellis writes, "the difficulties of a mathematical treatment in a foreign tongue" prevented his utilizing the volume which, he advises, is "well worth the attention of mathematical economists" (p. 175n).

The translation, titled "Basic Principles of the Money Economy," is concerned with the medium-of-exchange function of money and argues stoutly for a clear-visaged quantity theory (p. 23). Good theorist that he was, Schlesinger

builds up to the macro statement from the micro analysis of spending units. In advance of his time, and not unlike D. H. Robertson, he constructs a sequence in which one day's money receipts are not disposed of until the following day. There is also the acute perception, in our day associated with Whitin and Baumol, that cash-balance requirements will rise less slowly than projected expenditures, on a probabilistic argument (p. 24). Attention is also devoted to the discontinuities in the payments stream as a money-demand determinant, with the final conclusion, in good quantity theory fashion, that "the existence of money has very little influence on the conditions of exchange equilibrium" (p. 38).

While Schlesinger's work merits attention from monetary theorists, especially to contrast it with Fisher's great work of the same period, this collection of papers is likely to be recalled mainly because of Ragnar Frisch's lengthy and elegant exposition *On Welfare Theory and Pareto Regions*, running to some 53 pages of show-stealing space. Frisch develops Paretan welfare theory by separating problems of whether optimal positions exist which satisfy "some preassigned set of conditions," from the question whether an economy can accomplish such ends; these are typed as *selection* versus *realization* problems, a classification which should prove fruitful for the theory. Much of the analysis, in terms of set theory, is devoted to isolating local as against global maxima. Frisch concludes at one point that, like the statistical testing of hypotheses, "Pareto-optimality is a principle of *negation*, not one of affirmation," and that its assistance "consists in asserting that certain economic régimes do *not* satisfy certain optimality criteria" (p. 74).

While welfare theorists will want to study this article carefully, the entire relevance of stationary welfare analysis becomes quite obscure when one turns to the next but one essay in the collection, by Siro Lombardini, concerned with the efficiency of investment in underdeveloped areas. Here all the old enigmas, of time and change, of uncertainty, of external economies, confront the analyst. It is hard to reconcile the "new" welfare analysis with the vital topical welfare issues confronting "underdeveloped" nations today. Lombardini's analysis is a well-reasoned introduction to these difficult mundane matters that literally defy the embrace of the developed welfare theory.

Somewhat special is the article on "The Market Economy and Roman Catholic Thought," by Daniel Villey of the University of Paris, written "at the instance of Professor Wilhelm Röpke" (p. 92n.). Villey argues that "very few Roman Catholic theologians really know what liberalism is or understand the working of a market economy" (p. 94). Considering that Villey never discusses precisely the various shades of liberalism, market structures, or market economies, it is possible that he is too severe in limiting the imprecision to theologians alone. When Villey asks Catholics "to choose" the market system (p. 123) an American is apt to forget that such ill-defined selections do agitate countries such as Italy or France. While the article is a valuable one in a borderland area of economics it would have more merit if Villey helped us know what sort of "market economy" he is talking about, with what rules, monetary system, and governmental bounds. While Villey seems to be talking about a very limited degree of governmental intervention,

it would be interesting to know how he would react to Lombardini's observation that: "Entrepreneurship, of course, must not be equated with private enterprise" (p. 135).

The collection closes with an article demonstrating Walther G. Hoffmann's deft capacity for organizing statistical data and drawing important conclusions from them on labor productivity, relative wages, and capital intensity ("On Forecasting Productivity Changes in an Expanding Economy"). Hoffmann gives detailed consideration to differences of productivity in the separate branches of industry, and different rates of productivity growth within the structure (p. 149); he concludes that "any prediction of the average productivity of the industry groups . . . also gives indications about probable productivity changes in separate branches, and *vice versa*" (p. 171). Does this mean more than that averages when used with care can be useful and informative in predictive work? Space prevents a detailed discussion of his findings which, I think, should be directly related to many recent growth models.

Book reviews from Scandinavian journals over the years 1951-58 involve some 13 works, and require the final 71 pages in which the editors seek to draw attention to various contributions and to illustrate that "in some countries at least, reviewing has not become a lost art" (Preface). It is possible to express skepticism at the last remark and to question the need for some of the reviews when book notes might have served just as well. Considering Erik Lundberg's enthusiasm for Erik Dahmén's work on *Entrepreneurial Activity in Swedish Industry* some consideration to future translation and inclusion in the series would appear in order.

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Price and Allocation Theory; Income and Employment Theory; Related Empirical Studies; History of Economic Thought

Business Behavior, Value and Growth. By WILLIAM J. BAUMOL. New York: The Macmillan Company, 1959. Pp. xiv, 164. \$4.75.

Strategy and Market Structure: Competition, Oligopoly, and the Theory of Games. By MARTIN SHUBIK. New York: John Wiley & Sons, Inc., 1959. Pp. xviii, 387.

It is one of the cardinal sins of reviewing to criticize an author for failing to write the book the reviewer would have written under the same title. But it is going too far in practicing the opposing virtue to refrain from commenting on the failure of an author to write the book he himself has announced in his introductory remarks. In the judgment of this reviewer, both W. J. Baumol and Martin Shubik have failed, in different ways, to meet the goal both set forth: a new understanding of oligopolistic markets. Readers will find something worth thinking about in both these books, but not much that advances our understanding of the behavior of firms in markets where numbers are few.

Business Behavior, Value and Growth consists of two distinct parts, with only the loosest connection between them. The first is entitled "On the Static Theory of Oligopoly." Two of its seven short chapters introduce the problem and summarize the classic discussions of oligopoly; the third is something of a digression on the character of the capital market and its implications for competition between large and small firms. The central part of Baumol's argument is contained in the chapters entitled "On Oligopolistic Interdependence in Practice," "The Revenue Maximization Hypothesis," and "A Static Oligopoly Model." The final chapter of the first part draws some conclusions about observable business behavior from the preceding argument. In a word, Baumol's theory offers us a model of a (quasi?) monopoly maximizing sales revenue subject to the constraint of maintaining an "acceptable" level of profits. The structure of the market in which it operates and the behavior of its rivals are nowhere present in the discussion; interdependence among rivals selling in the same market, which theorists have usually taken to be the essence of oligopoly and students of industrial organization have regarded as one of the important problems in the study of any concrete market, has simply vanished. The arguments Baumol gives for neglecting interdependence (Ch. 4) have some merit in supporting his contention that in "*day-to-day decision-making*, . . . [it] plays only a small role" (p. 27, emphasis in original), but none in dismissing its importance for what Baumol calls "crucial decisions." Perhaps the mere mention of the famous 1931 increase in cigarette prices will indicate what has been assumed away.¹ And, of course, the assumption of constrained sales maximization in itself makes the interdependence problem no more tractable than that of profit maximization.

If not a new theory of oligopoly, we are at least given a new theory of the firm; what can we say of it? Baumol's large firm uses the margin provided by the difference between "acceptable" profit and maximum profit to maintain as large dollar sales as possible, e.g., by advertising, other kinds of sales effort, concessions to distributors, product improvement, etc. The essence of competition is rivalry for sales, or market share. This is not an implausible hypothesis; but a dozen equally plausible ones can be invented. Baumol's, to be sure, has the virtue of simplicity as well, and others would be more complex. Yet it is not a compelling hypothesis, either because of the deductions it supports, or its direct agreement with observation. Many other hypotheses about business behavior will lead to the conclusion that various forms of nonprice competition among large firms are more likely than price competition; in particular, those which emphasize interdependence and the importance of business reactions to ignorance and uncertainty. Nor is sales-maximization as a behavior rule in general consistent with the existence of market-sharing agreements, and devices which work toward market-sharing.

Of course, the definition of the "acceptable" level of profit may become crucial here, and as the author himself observes, high enough levels could lead to the same behavior as profit maximization. The discussion of what

¹ See W. H. Nicholls, *Price Policies in the Cigarette Industry*, Nashville 1951, pp. 78-88 and 167-81.

determines the "acceptable" level points to the conclusion that appropriate conditions in the capital market will lead to the normal competitive level of profits as the acceptable level, but is too sketchy to support it firmly. Some of the assumptions which appear to lie behind the discussion of the capital market in Chapter 4 are curious, and at least one of them is inconsistent with the proposition that large firms get their capital in an effectively competitive market. The upward slope of the average gross (and net) profit rates can only be justified by Baumol's arguments if the large firms do not sell shares in the public market. If they do, owners of less profitable small firms will withdraw funds from their own businesses to buy them, and this movement will tend to equilibrate the profit rates between the two classes. If they do not, the capital market is not competitive. Further, the argument omits the possibility that there are profitable opportunities for small owner-managed firms which are not open to large firms, since the latter could not provide owner-management. The restaurant industry provides an example of this possibility; so do many service trades and, possibly, some high-style and specialty manufacturing industries.

The only connection between the first part of the book and the second, "On the Theory of Economic Growth," lies in Chapter 10, "Oligopoly and Economic Growth." This argues that big firms with professional managements have various built-in pressures toward high and steady growth, of which sales maximization is only one. Here again the arguments are plausible, but not compelling, and empirical evidence is absent. Why did the steel industry respond to the built-in pressures for growth less rapidly in the first postwar decade than many other large-firm industries? Or why did the Aluminum Company respond so cautiously to growth pressures in the 'twenties? If the same forces are absent in small, competitive firms, why has there not been a great increase in concentration over time in the U.S. economy, rather than the nearly stable situation which the best students of the problems have found?

The rest of Part II presents a simple aggregate growth model, and some observations on policy related to it. The chief feature of the model is that it makes the rate of addition to capital (long-term investment) dependent on the level of income, not on the rate of change of income, as do Harrod-Domar models. This leads to a more comfortable view of the stability of the growth process, since the possibility of "falling off" the growth path into violent oscillations does not arise. This is not the place for a labored discussion of the usefulness of very simple aggregate growth models, and the realism of policy recommendations based on the mathematics of their solutions. It is hard to disagree with Baumol's contention that policies which raise the long-term growth rate of an economy (his "expansion effort") are likely to be more important than those which merely increase its initial capital stock. But even at this level of generality, it is worth observing that in most underdeveloped countries these two factors are more likely to be inter-related than independent, and some minimum level and composition of initial capital may be necessary to secure any change in expansion effort. Be this as it may, the discussion of this and other points suggested by the growth

model has no bearing on the particular views of business behavior set forth earlier, and seems equally applicable to a world of profit-maximizing competitors operating under Schumpeterian conditions of innovation and its consequent dynamic changes as it does to Baumol's revenue-maximizing oligopolists.

Strategy and Market Structure also fails to give us much new understanding of oligopolistic markets, but not because it turns out to be about something else. As his subtitle tells us, Shubik's book is an essay in the application of the concepts and techniques of the theory of games to the problems of oligopoly and competition. The heart of the book lies in the two chapters (10 and 11) in which a new dynamic theory of oligopoly is set forth. The first part of the book (Ch. 1-7) is a discussion of the static theory of oligopoly in game theory terms. It covers largely familiar ground, but unfortunately the level and style of the exposition is such as to make this neither a suitable introduction to the material for the novice nor a neat, elegant summary of the literature for the experts. Chapters 8 and 9 are preliminary to the dynamic analysis; and, again, those familiar with the theory of games will not find them necessary to an appreciation of the main argument as set forth in the two following chapters.

In these chapters Shubik develops a model of a duopoly market in which the asset positions and financing plans of the participants, as well as their market moves over a number of periods are taken into account. He terms this the analysis of "economic ruin games," and uses it to examine the stability of asymmetrical market structures; i.e., the circumstances under which it would be likely that one duopolist would drive the other out, or those under which a new entrant might challenge the position of a single seller. While the model is not formally generalized from duopoly to oligopoly, some aspects of the more general problem are examined, including the stability problem. Shubik rightly contends that all this represents a large step toward relevance in the application of game theory to market problems. But how much more remains to be done is shown by the discussion of the automobile and cigarette markets in Chapter 12, and of public policy toward competition in Chapter 13, the final chapters of the book. In neither of these is anything said that specifically reflects new insights provided by game theory.

The gap between the simple model of two sellers with constant demand and cost functions and different asset structures analyzed at the end of Chapter 12 and the kind of model that would be needed for a formal discussion of the automobile market along similar lines is enormous. It reflects at the same time both the complexity of the tools of game theory and their weakness. To take into account the whole set of strategic possibilities open to the half dozen domestic rivals in the automobile market would require a game model of tremendous size, about which almost nothing could be said. Yet, at present, the structure of the theory seems to require that all the complexities be admitted into the model; no methods of aggregation, so to speak, which could drastically reduce the combinations that required consideration seem to be available. This view of the matter—if it is correct—does not imply that Shubik's model-building is a misguided effort. Quite the

contrary; it suggests to the reviewer that what is needed is more such effort, precisely with the purpose of uncovering new simplifications which will make the use of game-theoretical concepts more fruitful. But this view does indicate that the author's attempt to leap from model-building to market analysis is premature. The student of actual markets must continue for some time to use his present miscellaneous kit of theoretical tools, inadequate as it is, rather than plan to trade it in for a neatly graded set of game-theoretic models, guaranteed to fit all problems.

CARL KAYSEN

Harvard University

An Introduction to the Theory of Interest. By JOSEPH W. CONARD. Berkeley and Los Angeles: University of California Press, 1959. Pp. xx, 375. \$7.50.

Joseph W. Conard's *Introduction to the Theory of Interest* was begun as a doctoral dissertation at the University of California (Berkeley), and still bears traces of that fact. An ambitious plan to expand it into a complete treatise could not be carried out, and it remains, by its author's admission, a fragment. During the excessively long period between its completion and publication, several important contributions have been made to the literature in its field. Yet these three strikes against it do not seriously impair its total quality and usefulness. It is a work of genuine scholarship which will take a permanent place in the literature on interest theory.

A brief introductory chapter poses the basic problem for interest theory in terms proposed by Schumpeter: How could interest (even) exist in a circular flow economy, in which the value of each good derives on the one hand from the value of its current services, and on the other is wholly imputable to the factors that produced it, recognizing that capital goods are like other produced goods, whose whole value should be ultimately imputable to the "original" factors, land and labor?

This introduction leads naturally to Part I, which considers the "nonmonetary" interest theories of Böhm-Bawerk, Fisher, Knight, and Ramsey. Conard's presentation of these theories impressed me with its thoroughness, subtlety, sophistication, and its ability to drive to the heart of most issues. The only exception I have noted is with respect to his treatment of Fisher's "opportunity curve," Keynes' "MEC," and Lerner's "MEI," where Conard recognizes most of the issues, but does not give them what I would consider definitive resolution. On the whole, however, I cannot imagine a better place to send a graduate student for a clear exposition and exegesis of the classical nonmonetary theories of interest.

Part II turns to the "monetary" theories. It begins with a brilliant chapter on "Money Rates, Own-Rates, and Real Rates of Interest," which settles a great many issues raised by Keynes, Lerner, and Fisher. This is, however, a sidetrack from the main argument. The balance of the second part does essentially two things: (a) it reviews the loanable funds vs. liquidity preference argument; and (b) it places the static determination of the rate of interest in what Conard calls a "general equilibrium" context (which I would prefer to call the context of a "closed macroeconomic system").

The loanable funds vs. liquidity preference argument is skillfully done, arriving at conclusions essentially identical with those which have been reached by others during the long interval between Conard's writing and publication.¹ Conard's analysis is subject to some (though not all) of the complaints which I have elsewhere made about Tsiang's and will not repeat here.² I did find disconcerting the following definitions of stock and flow analysis (p. 158): ". . . the supplies and demands involved may be conceived in terms either of stock or flow. That is, one may think of the demand and the supply for securities at a moment of time, or may think in terms of the quantities that would be bought and offered over a period of time." (Surely this is not the difference. A stock is measured in units that make no reference to time—e.g., tons or dollars; while flows are expressed in units that necessarily involve time—e.g., tons per hour or per month, dollars per week or per year. Either one can be defined as of an instant of time or as an average over a period of time.) Conard's later discussion shows that he really knows better; but there remains a slight muddiness in the matter that prevents his reaching what (to me, at least) are completely sharp and correct formulations.

His setting of interest theory in a general macroeconomic context reviews familiar ground, but reviews it well. The principal extensions (shared with Patinkin and others) involve an expression for the demand for idle balances in the form

$$\begin{aligned} M_2/p &= L_2(r, We) \\ We &= M/p + R/p + A \end{aligned}$$

where M_2 is idle balances, p the price level, r the interest rate, We the real volume of wealth, M the total money stock, R the value of outstanding bonds, and A the quantity of real assets. This formulation introduces considerable complication, although it makes little difference for most predictions.

Part III relates to the term structure of interest rates, including both the theory and its empirical testing for the United States, 1951-54. Space does not permit consideration of his treatment of this subject; but I cannot omit praise for his excellent statement on pages 298-99 of the theoretical relationship between "the" rate of interest in general interest theory and the term structure of rates.

Conard's study surely makes important contributions to interest theory. It does not provide the last word on this thorny subject; but it covers a number of important aspects, and in a way that fully merits the attention that this book will receive.

GARDNER ACKLEY

University of Michigan

¹ Especially, W. L. Smith, "Monetary Theories of the Rate of Interest: A Dynamic Analysis," *Rev. Econ. and Stat.*, Feb. 1958, 40, 15-21; and (in part) S. C. Tsiang, "Liquidity Preference and Loanable Funds Theories of Interest," this *Review*, Sept. 1956, 46, 539-64.

² "Liquidity Preference and Loanable Funds Theories of Interest: Comment," this *Review*, Sept. 1957, 47, 662-73.

Theory of Markets. By TUN THIN. Harvard Economic Studies CXIV. Cambridge: Harvard University Press, 1960. Pp. viii, 120. \$5.00.

Prospective readers should heed the author's caution (p. v) that he is addressing economists "who have already been initiated into the theories of markets" and who consequently have mastered such standard works as those by Chamberlin and Triffin. The book probably will appeal most to the initiates who prefer close study to general reading. It is the admirably terse style, more than content, which forces a sustained studious effort. Its compactness results from the employment of frequent textual references to a profusion of serially numbered paragraphs, equations and hypotheses. This abbreviated presentation necessitates either a tenacious memory or a willingness continually to turn back and forth in search for indispensable information and verification.

The *Theory of Markets* contains an analysis of the essential influences of price on profit under varied market arrangements. It surveys the flood of studies on perfect and imperfect competition which began in the 'thirties and which so quickly became a vital part of microeconomics. In no other sense is it a theory of markets. The author's chief interest, however, is not in historic interpretation or even in criticism, but in a serious endeavour to introduce his own principal innovation.

Probably at the outset Tun Thin also intended, as a secondary contribution, to settle the controversy about the classification of markets in terms of elasticity coefficients which started with Triffin and which has continued so inconclusively up to now. After his introductory discussion of market structures he compares Triffin's and Fellner's definitions. Then he replaces them with his own selections, which however satisfy him only within bounds because "different qualifications have to be made to make them applicable to different markets" (p. 35). His symbolic representation of perfect competition, for instance, uses two coefficients similar to Fellner's but further restricts the variables with four sets of equalities and inequalities. This added burden evidently led him to realize that the value of symbols as market definitions vanishes when their untidiness increases sufficiently.

Although Tun Thin touches all well-known varieties of perfect and imperfect competition, his predominant concern is with oligopoly. To start his oligopolistic section he puts the prior results of Cournot, Smithies, Chamberlin, Stackelberg, and Fellner into comparable mathematical notation. Then he emphasizes, as others have done, how each writer's characteristic conjectural variation provides a side condition necessary to determine the best price. This introduces his own oligopoly analysis which fills almost half the book, and which he rightly regards as his primary innovation. He describes it as "akin to Game Theory." In it he substitutes for the earlier economists' fixed rules an assortment of strategies from which the oligopolists can pick "according to the kind of situation they are faced with" (p. 58). Ever since game theory arrived, its successful application to oligopoly has been looked forward to; the hitch has been to apply it in a way so that something different from a methodical restatement of the oligopoly problem results, particularly in such a manner that fresh and significant conclusions follow.

Tun Thin's game-theory attempt begins with a drastically simplified duopoly model which assumes (1) either a direct or an indirect relation between the firms' profits and (2) either increasing or decreasing monotonic profit functions. The first assumption yields six cases which he displays as matrices of non-zero-sum max-max games. From each matrix he ascertains, by means of a graphical solution, only the *directions* in which the oligopolists ought to change their prices, rather than the usual equilibrium prices. This is, the author says, the "second best solution," yet one which still gives answers where information is incomplete. Later chapters moderate the initial hypotheses and enlarge the model to include four sellers. In the final pages he illustrates both "fixed conjecture" and "multiple conjecture" problems with servomechanisms. Whatever defects (e.g., irrelevance) a reader finds, he must nonetheless feel obliged to Tun Thin for having made some headway in a field with few guideposts. In fact the present volume is the single extended link of game theory with oligopoly outside Martin Shubik's recent *Strategy and Market Structure*.

R. S. HOWEY

The University of Kansas

Einkommensverteilung und technischer Fortschritt. By JÜRG NIEHANS, GOTTFRIED BOMBACH, and ALFRED E. OTT. Berlin: Duncker & Humblot, 1959. Pp. 202. DM 24.80.

The theoretical problems posed by the interrelationship of technical progress, economic growth and income distribution have been widely discussed in American and British economic literature. The contributions of Nicholas Kaldor, Robert Solow, Sidney Weintraub and others to this discussion are relatively well known. Among German economists, particularly those who have some familiarity with mathematical techniques, there has also been a debate on the problems of distribution, production and growth theory. The present volume is a collection of essays encompassing the most recent German contribution to this discussion.

The first and longest essay, by Jürg Niehans, is divided into two main sections. The first part develops a Keynesian theoretical model and the second shows how the model may be used to resolve various macroeconomic problems. Among the applications of the model examined are the effects of changes in wages, technical progress, taxes and savings habits upon prices, production and income distribution. The magnitudes incorporated by Niehans into his closed economy model are: the price level, the wage level, nominal and real gross national product, nominal and real wage income, nominal and real capital costs, business income, consumption expenditures of wage earners and entrepreneurs and the state of expectations. Conspicuously absent from the model is any detailed consideration of the money market. Niehans assumes a stable interest-rate policy and a completely elastic money supply. These extreme Keynesian assumptions are made to ease the working of the model and are justified by the author's belief that the direction of money-market influences, in any case, is self-evident. For me, these assumptions severely constrict the usefulness of the model. Theoretical and empirical develop-

ments of recent years have tended to show that the left-hand side of the equation of exchange is nowhere near as predictable and passive in response to the right-hand side as was formerly thought.

In the second essay, Gottfried Bombach assesses the respective merits and weaknesses of various aspects of distribution theory. He discusses, for example, several ways of classifying income distribution, a number of distributive-share income models, the relationship between marginal analysis and distribution theory, and short-run versus long-run income distribution models. In the course of his analysis he points out clearly and correctly that analytical systems in which the capital coefficient is important can adequately handle only long-run movements. Short-run fluctuations of capital coefficients are thus a phenomenon that can only be explained by a theory of the cycle which accounts for fluctuations in the level of productive investment. It is on this basis that Bombach objects to some of the work by W. Krelle and M. Kalecki on problems of income distribution.

The third and final essay, by Alfred E. Ott, is concerned with the difficulties of including technical progress in the production function when working with growth models. Ott's central thesis is that the older growth theory of the Harrod-Domar type is by no means as limited and unrealistic as the more recent work of Champernowne, Kaldor, Power and Solow would seem to imply. Theories of the Harrod type, he argues, can adequately explain the relative constancy of labor's share of income, the constancy of capital coefficients, and the increase of worker productivity and capital intensity. In the course of the development of this thesis, Ott carefully examines the relevance of the Cobb-Douglas and Leontief production functions for growth theory. He favors the Cobb-Douglas function and observes that this production function is basic to Harrod-type models when dealing with the economic realities of labor-saving technical advances.

All three studies are of the highest caliber. The use of literary, graphical and mathematical techniques is well integrated in each study. These essays should be well received, particularly by specialists in growth theory.

JOHN J. KLEIN

Fordham University

Marktform und Verhaltensweise. By ALFRED E. OTT. Stuttgart: Gustav Fischer Verlag, 1959. Pp. 153. DM 26.—.

The first half of this book is given over to description and critical evaluation of the various systems of market classification of recent decades. These the author divides into the "morphologic," and those determined with "the aid of the concept of elasticity." In both categories the major difficulty is the definition of oligopoly—the drawing of the line few-many if the criterion is market data, or of dependence-interdependence if the criterion is cross-elasticity.

With respect to the cross-elasticity criterion, Ott argues, first, for retention of the "group" concept, which—following Joan Robinson—he would define by an "intra-industry elasticity of substitution distinctly greater than the

extra-industrial" (pp. 39, 80); and second, that the values of the various kinds of elasticity (own, cross, penetration) are consequences of the primary market data. The latter—number of rivals and degree of (product) differentiation—are therefore the proper criteria of classification.

Moving to behavior patterns, Ott describes the "older theories" (Sting, Frisch, Erich Schneider)—older, that is, than game theory. Ott's own rather complicated classification is based on the "measure" to which an entrepreneur takes into account the "actions-parameters" of others, and his "idea" of their constancy. Despite the subjective nature of these criteria, Ott concludes that in all market types except heterogeneous oligopoly, form and behavior patterns are strictly coordinate. For the rest, the author is concerned with the peculiar difficulties of determining strategy in oligopolies with undifferentiated products and in bilateral monopolies. In this connection, he summarizes the findings of game theory with respect, first, to the problems of maximization, and second, to formation of coalitions.

It is difficult to find fault with Ott's classifications and major conclusions—with the important exception of his treatment of oligopoly. In the first place he distinguishes the two market forms, oligopoly with and without differentiation (Table 12, p. 119); but only in the latter is the entrepreneur's situation unambiguously "interdependence," whereas with differentiation, the oligopolist's situation is "interdependence/independence" and his behavior pattern "conjectural *or* autonomous" strategy (my italics). But if differentiation is so pronounced that it implies "independence" is not the situation essentially monopoly? Thus Ott might better have omitted this market type in the case of which form does not strictly determine behavior.

Second, the specific problem of oligopoly, according to Ott, is that the oligopolist can determine his best strategy only if he knows the behavior patterns of his rivals (p. 120). Ott seems to imply that polypolists and monopolists do not need to guess what will be the behavior of others. But they do. The difference here is of degree, namely, that uncertainty is reduced if numbers—whether of rivals, or of market opposites—are large. The characteristic problem of oligopoly—and of bilateral monopoly—lies elsewhere, namely, that in these markets the entrepreneur anticipates reactions of rivals or market opposites specifically to his own actions. Their "actions-parameters" are functions of his decisions, as his of theirs. Incidentally coalitions are not, as Ott thinks (p. 121), impossible in bilateral monopoly. There is always danger that the "countervailing" powers may get together—management and labor, for instance, against the consumer.

The principal merit of this book is its critical survey of recent and contemporary market theory. The American reader will also be grateful for the opportunity to make or renew acquaintance with important continental theorists, particularly Stackelberg and Schneider. The book contains several unimportant errors, and has no index. But an appendix gives the most important systems of classification.

C. W. EFROYMSON

Butler University

Ocherki planovogo tsenoobrazovaniia v SSSR. (Essays on Planned Price Formation in the USSR.) By SМ. YA. TURETSKII. Moscow: Gospolitizdat, 1959. Pp. 499. Rbl. 13.

In this comprehensive treatise, Professor Turetskii of Moscow University undertakes to analyze the principal features and problems of the Soviet price system. Separate chapters are devoted to each of the following topics: the role of value and price in the Soviet economy, a survey of the various types of prices used in the USSR, prices in heavy industry, prices of agricultural products, prices in light industry, freight rates, and retail prices. Turetskii considers the functions of prices in the various sectors of the economy and the problems encountered in trying to set prices "correctly." Throughout, he is primarily concerned, as the book's title indicates, with how prices can be used as an instrument of control in the Soviet planned economy.

The principal functions which he distinguishes for prices in the control and direction of the economy are the following: (1) to guide choices by producers among alternative inputs and among alternative outputs; (2) to promote the efficient operation of Soviet enterprises; (3) to secure the proper relationship between the money incomes and expenditures of households, assuring the distribution of consumer goods' output and avoiding repressed inflation; (4) to guide choices by households among alternative consumers' goods; (5) to provide adequate incentives to collective farmers, through the appropriate relationship of agricultural procurement prices, on the one hand, and retail prices and the prices of industrial inputs into agriculture, on the other; and (6) to guide the use of transportation facilities and the location of economic activity.

To achieve these ends, Turetskii says, prices cannot be based on "socially necessary labor time" alone. Instead, three factors must be considered in setting "economically sound" prices: (1) the level of "socially necessary" production expenses; (2) the relationship of supply and demand (including the possibilities for substitution); and (3) the nature of certain commodities (such as basic foodstuffs and strategic raw materials) which require special treatment in price-setting. Thus Turetskii specifically condemns fixing relative prices on the basis of relative costs (or the relationship of costs plus a "normal" profit), stressing the need to consider demand and scarcity. However, he envisions price ordinarily adjusting the demand for a commodity to a given supply of it, neglecting the implications for future output adjustments of the resulting diversity in cost-price ratios. This orientation corresponds to his view of the price system as a device for executing the desires of those who plan and control the economy, rather than as an autonomous force influencing them in their decisions. It is unfortunate that, as a consequence, this extensive treatise does not deal either with the role which prices play in planning decisions (as distinct from their effect on enterprise managers and households) or with the respective roles in the allocation of producers' goods of prices and of allocation orders in physical terms.

Illustrative of the problems in price formation to which Turetskii devotes considerable attention is that of the dual role of price as cost to the buyer and as revenue to the seller. It is desirable that buyers pay the same price

for a given commodity regardless of the identity of the seller (in order that the costs and the performance of buying enterprises be comparable). A uniform selling price would achieve this, but, inasmuch as the costs of producers vary greatly, a uniform selling price would mean a wide variation in their profitability, ranging from high profits for the lowest-cost producers to losses for the highest-cost producers. Although such a variation in profitability is attributable in many industries to such factors as differences in the location or quality of raw materials or in the kind of machinery the enterprise has, rather than to differences in managerial performance, Soviet doctrine holds that wide variation in profitability among enterprises producing the same product is undesirable. Managerial efficiency is promoted, it is argued, by setting prices so that each enterprise will earn a "normal" profit (say 5 or 10 per cent above cost, as this is calculated in Soviet industry) if it operates efficiently, i.e., meets its plan. Both above-normal profits and below-normal profits (or planned losses) encourage laxity and inefficiency on the part of enterprise managers, according to Soviet writers (for reasons which are not fully explained). Hence the task of the price-setters is to fix a single buying price for customers but differentiated selling prices for sellers. One way of achieving this, which Turetskii recommends, is to have a central sales organization in the industry buy from producing enterprises at different prices and resell to buyers at a single price.

Despite its failure to consider such questions as the role of prices in planning decisions and Soviet foreign trade prices, this book is an important addition to the scanty literature on the Soviet price system. Within its carefully defined scope, it provides the most extensive discussion of price formation in the USSR yet published. Turetskii analyzes critically the questions which he takes up, not limiting himself to the definition of Soviet concepts and the description of Soviet practices, as do some Soviet writers. In support of his analysis, Turetskii presents much statistical material showing actual cost and price relationships in the Soviet economy (rather than illustrative, hypothetical figures of the kind found in many Soviet books). The many examples he cites provide incidental information on various Soviet industries, on turnover tax rates, on economic administration, and on other matters of interest to students of the Soviet economy.

MORRIS BORNSTEIN

The University of Michigan

- Zakon stoimosti i yego ispol'zovaniye v narodnom khozyaystve SSSR.* (The Law of Value and Its Utilization in the Economy of the USSR.) Edited by Y. A. KRONROD. USSR Academy of Sciences, Institute of Economics. Moscow: Gospolitizdat, 1959. Pp. 516. [Referred to below as Book I.]
- Zakon stoimosti i yego rol' pri sotsializme.* (The Law of Value and Its Role Under Socialism.) Edited by N. A. TSAGOLOV. Lomonosov Moscow State University, Faculty of Economics. Moscow: Gosplanizdat, 1959. Pp. 336. [Referred to below as Book II.]

Everyone who has read Marx beyond his *Capital* knows that for him the law of value was the "formula for the modern slavery of the proletariat, in-

stead of being, as M. Proudhon would have it, the 'revolutionary theory' of the proletariat's emancipation."¹ On the other hand, socialist economics appeared to Marx somewhat like the management of modern municipal water works (the output of water is so abundant that its value withers away: it costs more to charge a price to the consumer than not to charge it; yet, to prevent disproportions in consumption, the distribution of water must be centrally planned). Marx never assumed a socialism with basic scarcities, with labor productivity lower than under capitalism, with a sweated wage system, squeezing capital accumulation, etc. Stalin, having "creatively developed" Marx, decreed that socialism existed in the USSR despite that country's underdevelopment and that the law of value was not only coherent with, but had to be made the correction factor in the rationalization of the centralized plans.

It is pure politics (and moral choice, too) to side in this instance with Stalin against Marx and to pretend that Marx's foresight of the future was wrong, while Stalin's arbitrariness was right. Yet, this is explicitly what Soviet economists had to do in the books under review in order, in their turn, to "creatively develop" Stalin. Their real problem was clearly foreseen by Marx in *Misère de la philosophie* as follows: "It is important to insist upon this point: that which determines value is not the time in which a thing is produced, but the minimum time in which it is susceptible of being produced; and this minimum is revealed in competition. Suppose for a while that there is no competition and, consequently, no means to ascertain the minimum of labor necessary for the production of a given commodity; what will happen? Six hours' labor would have to be put into the production of an article in order to have the right to exact in exchange six times as much labor from another worker who spent only one hour to produce an identical product."² Precisely this irrationality arose from Stalin's planning, and it was the factor that caused eighty-two leading economists to assemble in two conferences—at the Academy of Sciences in 1957 (Book I) and at Moscow University in 1958 (Book II)—and to publish their papers and comments, which, we are sorry to observe, still contain no remedy for the ailments of Stalinism.

To be true, not all conferees caught up with their problem or were even aware that it had been pointed out by their teacher long ago. In Book I two problems are discussed: (1) how to explain and exonerate before Marx the fact that commodity production, a moneyed exchange economy, and the law of value exist at all under socialism; and (2) how the law of value relates, or should relate, to the centralized pricing practiced in the USSR. Book II introduces an additional problem (3): how the law of value relates, and should relate, to capital allocation under centralized planning. Problem (1) was disposed of in categorical terms, with a minority deriving the law of value from the division of labor and a majority maintaining that it stems from the differentiation of property rights.

Problems (2) and (3), however, split the participants into three interesting schools of thought. A sturdy group that included Z. Atlas, M. Kolganov,

¹ *Marx-Engels Gesamtausgabe*, Frankfurt 1932, Vol. 1 (6), p. 133.

² *Ibid.*, p. 146.

V. Sobol, D. Kondrashev, A. Pashkov, L. Vaag *et al.* proposed to make a general rule of what it said had always been a common Soviet practice: that all prices be made proportional to prime costs and that the equalization of the rate of profit in all lines of production be made the criterion of the allocation of resources. This is the "price-of-production" formula of Volume III of *Capital*; it came in for criticism on the ground that "it would restore capitalism": resources would follow the lead of higher profits, rather than flow into the unprofitable employments which may be of interest to the society as a whole.

Another school, which included such theoretical luminaries as Y. Kronrod, M. Bor, A. Bachurin and P. Mstislavsky and which pledged its allegiance to S. Strumilin (who was not present at the conferences), came out with the doctrine that prices should be made proportional to labor content of commodities and that the criterion for resource allocation should be the equalization of the rate of surplus value. Recipes almost identical with this one were proposed in their time by Rodbertus and Proudhon; and Marx, while spurning them, used to pepper his verdict—"utopia"—with some rather unquotable epithets. Strumilin proposes to measure value by wages. This would mean, however, that prices determine values, rather than values determine prices.

The third school, led by K. Ostrovitianov and L. Gatovsky, who presumably represented the Communist Party's official standpoint, took a reserved position, hesitatingly critical of the other two schools. According to this group, improvements, rather than radical changes, are needed in the present mixed economic system; the law of value must not be allowed to become the sole allocator of resources, but rather must be "mastered" and "utilized" by the state in a monopolistic fashion. The planners must continue to grope around for appropriate adjustments of value ratios to create incentives for the fulfillment of the Communist Party's plans.

VSEVOLOD HOLUBNYCHY

Columbia University

Lectures on Economic Principles. Volumes II and III. By DENNIS H. ROBERTSON. London: Staples Press Ltd.; New York: John de Graff, distributor, 1958 and 1959. Pp. 162; 164. \$3.50; \$3.50.

Robertson used to give these lectures to undergraduates at Cambridge who had already had some economics. Volume I, which was reviewed in the December 1958 issue of this journal, puts value theory in a nutshell. Volume II does the same for the theory of distribution. In the case of Volume III, which deals with virtually the whole of macroeconomics, the nutshell is too small; yet, Robertson succeeds in touching on almost everything; the only obvious omission is appraisal of Keynesian economics as a whole (as distinct from particular ideas). The lectures embody all the virtues associated with Robertson's name—clarity, sophistication, wit, wisdom, urbanity. One envies the teacher who had students capable of absorbing such compact discussion at one hearing; one envies even more the students privileged to learn from such a lecturer.

RENDIGS FELS

Vanderbilt University

The Rise of American Economic Thought. By HENRY W. SPIEGEL. Philadelphia: Chilton, 1960. Pp. viii, 202. \$5.00.

Professor Spiegel's judicious selection of original materials in American economic thought starts with our colonial beginnings and ends with the founding of the American Economic Association in September 1885. In its field, this volume is the first collection of its kind. Excerpted authors include such of the famous as Benjamin Franklin, Alexander Hamilton, Thomas Jefferson, Frederick List, Henry C. Carey, Henry George, Francis Walker, and Simon Newcomb, as well as such lesser-known figures, shelved only in the better libraries, as Matthew Carey, Daniel Raymond, Alexander H. Everett, Jacob Newton Cardozo, Thomas Cooper, Francis Bowen, and Alexander Del Mar. If used in conjunction with Joseph Dorfman's invaluable *The Economic Mind in American Civilization*, this small book can serve as the basis for a well-organized course in American economic thought up to 1885. After that date economic materials become more copious and more available. In particular the works of the institutionalists are in any good library and some of them—Commons as well as Veblen—have been reprinted in paperback versions.

Spiegel's introductory comments to each selection are invariably clear and informative. Taken together they amount to a brief sketch of the development of economic thinking in this country. With the selections themselves I have no quarrel. Occasionally, however, Spiegel's editorial decisions do raise questions. Why, for example, in the instance of John McVickar does a page and a half of comment precede a single paragraph of excerpt? Again, Spiegel allots only four pages to John Bates Clark, almost evenly divided between the editor's comments and a selection from *The Philosophy of Wealth*, and a substantial seven pages to the much less important Alexander Del Mar. It might be said in general that longer excerpts from the authors would give students a better flavor of their characteristic mode of argument.

As they reveal themselves in this collection, what are the characteristic features of early American economic thought? One is a pragmatic emphasis upon economic policy. In the eyes of most of these writers, there is little question that the appropriate function of economics is to furnish the proper recipes for private and public economic action. Not until Francis Walker did a leading economist insist upon his subject's scientific neutrality. Commercial policy was one important field handled by almost everyone. In the late 18th and 19th centuries most American economic writers were automatically protectionist, although there is an interesting shift from the bounties advocated by Hamilton to the outright protective tariffs favored by Henry Carey. Under the protection of tariffs and with the stimulus of internal *laissez faire*, the country could rapidly industrialize and cease to depend upon foreigners. By 1816, even the agrarian Jefferson had come to the reluctant conclusion that English and French violations of international law compelled this country to protect its manufacturers as well as its farmers.

Much more than English writers, American economists or dabblers in the subject cherished a close alliance with religion. All the way from Cotton Mather to John Bates Clark, Americans took innocent joy in the thought that, as Wayland put it, economics could be best understood as "a systematic

arrangement of the laws which God has established." *Laissez faire*, supply and demand, and the unequal distribution of the world's goods, all earned inclusion among God's ordinances.

Finally, there is the relationship of American economics to economic analysis. This collection reaffirms the inaccuracy of descriptions of American economic thought as barren of analytical innovation. John Rae anticipated a good deal of Austrian capital theory. Simon Newcomb's societary circulation embodied a genuinely novel notion. Alexander Hamilton has a fair claim to the infant industry argument. Willard Phillips' "circles" come fairly close to von Thünen's location theory. Francis Walker's critique of the wages fund helped overthrow it in its English home. Nevertheless, American economic texts in the 19th century were organized and based largely upon McCulloch and Say, themselves little more than popularizers of the English classics. Where dissent occurred it was largely on the doctrines of free trade, population, and rent. Seldom indeed did the critics of classical doctrine rise to the theoretical level of their opponents. In large part the answers were those of practical men whose experience contradicted received theories. Although a great deal in Benjamin Franklin anticipates Malthus, most later American writers on economic affairs were staunchly anti-Malthusian on the eminently solid grounds of American experience with vast fertile lands and relatively small numbers of people. As for Ricardian rent theory, its theoretical subtleties won scant sympathy and little understanding. To many Americans, the theory of differential rent rested upon the peculiar land tenure arrangements of England and the special situation of an overpopulated island.

In short, what Americans did not copy from the English classics in their textbooks they tended to criticize out of the belief that America was different. The heavy reliance upon the English classics both positively and negatively does imply a warning to teachers of American economic thought. Before students are allowed into courses on this topic, they should take at least a semester of work in classical English and French economics. The relatively slight emphasis upon doctrinal history in many places may have the unfortunate effect of limiting the best use of this valuable volume.

ROBERT LEKACHMAN

Barnard College, Columbia University

Economic History; Economic Development; National Economies

Studies in the Industrial Revolution. Essays Presented to T. S. Ashton. Edited by L. S. PRESSNELL. New York: Oxford University Press; London: The Athlone Press, University of London, 1960. Pp. xii, 350. \$6.75.

A fine and fitting tribute has been paid to Professor Ashton by the contributors to this volume. The essays display the same respect for facts that we find in his work, and perhaps more important, the same respect for the human beings who faced the facts of their time. The time is mainly that of the 18th and early 19th centuries and the approach is one common to British economic historians who, as Herbert Heaton once put it, feel that "... they can be most useful by intensively tilling their little island garden or draining its fens." Each essay is a product of original and creative research and each in its way

illuminates topics with which Ashton was concerned. Together they bear witness to his influence and to the high quality of workmanship he inspired in others.

Of the twelve contributions, seven focus more or less sharply on individual or group action in a limited sector of the economy; the remainder are more broadly interpretive in their analysis of various aspects of industrial change. In the first category, T. C. Barker writes of advances in river improvement and canal building before James Brindley appeared on the scene, and Brindley's predecessors are given credit long overdue. The tribulations of Isaac Wilkinson, potfounder, are described by W. H. Chaloner in a biographical essay which places the achievements of Isaac's sons, John and William, in clearer perspective. H. Heaton's "A Yorkshire Mechanic Abroad," a delightfully written account of the achievements of a small entrepreneur without capital but of considerable technical and administrative competence, tells us much about technological change and market conditions in the mid-19th century textile trades of England's North Country, France and northern Italy. The response of labor and management to changes in industrial conditions is the theme of two essays, J. De L. Mann's "Clothiers and Weavers in Wiltshire during the Eighteenth Century" and A. J. Taylor's comparative view of the subcontract system in the British coal industry. On banking, D. M. Joslin analyzes new evidence on the activities of London bankers in wartime (1739-84) and R. S. Sayers, in his discussion of the return to gold in 1925, links monetary theory and history in his examination of the issues surrounding the making of this momentous decision. Failure to take proper account of the risks arising from London's inability to shift the burdens of cyclical adjustment to other countries is laid to neglect to long-term developments. "A little more systematic study of monetary history might have been useful in 1925, as well as now" (p. 327).

In the remaining essays, greater emphasis is placed on the relationship between industrial progress in England and changes in other sectors and areas which helped to shape its course. L. S. Presnell takes up anew the debate on the significance of the rate of interest in the 18th century. In his closely reasoned analysis, he stresses the distinction between real and monetary rates of interest, considers the implications of the switching of resources between money and other assets, and examines in some detail the significance of rate changes for various forms of investment. He gives at best a qualified support of Ashton's views on the question, although perhaps something more is suggested by the observation that "A higher rate of interest possibly concealed the operation of a substantial pair of scissors, reduced profit expectations being one blade, a higher cost of borrowing the other" (p. 209). J. D. Chambers' study of population changes in Nottingham in the 18th century draws attention to the importance of rising urban centers which take on a momentum of their own as industrialization sustains rather than initiates a strong upward trend of population growth; in this view the city appears as a determinant of national development rather than as an end-product of industrial change.

In his review of agricultural change in the period 1660-1760, A. H. John

provides a wealth of material on farming costs and on agricultural adjustment to price-changes in the different regions of the country and in the major divisions of agricultural activity. The failure of agricultural output to respond to the upward swing in prices which began in the middle of the 18th century is regarded as particularly significant in the shift of resources to industrial and commercial rather than to agricultural expansion. "... this failure of agriculture to respond to demands made upon it might well have been a contributory factor in launching England upon an industrial revolution" (p. 155).

Moving farther afield, E. F. Söderlund examines the impact of British industrialism on the Swedish iron industry, and J. Potter takes up a similar theme with respect to the response of United States to industrialism. The absence of revolutionary changes in the Swedish iron industry and its ability to maintain a position as a major export industry without coal deposits provides a striking contrast to British experience. Potter discusses in detail the statistics of trade between Great Britain and the United States in the period 1815-60 and makes it clear that no account of the Industrial Revolution in the older country can be complete without full consideration of the stimulus provided by the commerce of the closely integrated North Atlantic economy of this period.

Written by specialists very much at home with their subject matter, the essays leave little room for criticism on points of detail. Nevertheless as a group they raise the larger question of the status and direction of research and teaching in economic history. The present division of the subject into the two sharply delineated categories of conventional economic history and growth theory must be a matter of increasing concern. It is doubtful that historical data however presented are applicable to growth study as presently viewed, and it is clear on the other hand that the apparatus of the growth theorist has little meaning for the economic historian. The very quality of the essays underlines the need for the same skilled craftsmanship in the nebulous area between trend analysis and empirical study of the long period. In a volume so rich in insights into process one might have expected closer reference to Ashton's interest in the relations of theory and history. Presnell and Sayers touch on this theme, but in the main the essays leave unexplored the problems arising from the divorce between "two ways of thinking" about long-period change.

The volume contains a complete bibliography of Ashton's academic writing along with a number of illustrations. The editing is above reproach.

W. T. EASTERBROOK

University of Toronto

Commercial Crisis and Change in England, 1600-1642. By B. E. SUPPLE. New York and Cambridge, Eng.: Cambridge University Press, 1959. Pp. xii, 296. \$7.50.

Although seventeenth-century England was still primarily an agricultural country, its economy had become very vulnerable to disturbance in international trade, because it depended to a large extent on one article of export (cloth) and one market (northwestern Europe). Any decline in the foreign

demand for English cloth was likely to cause a depression which then spread rapidly from the higher to the lower stages, thus creating widespread distress and unemployment. As sales abroad shrank in volume, exporters curtailed their purchases from the clothiers whose stocks would not move, tying up working capital. As a result they were eventually forced to stop buying wool and giving work to spinners and weavers. Unlike modern depressions, such a slump was, of course, exclusively commercial in character: it was an inventory crisis, but it had monetary aspects, because it was usually accompanied by "scarcity of money" due to an outflow of specie and a slowing down of the velocity of circulation. Sometimes bad harvests or the plague came in to augment the sufferings of the poor and to bring them to the brink of starvation.

Professor Supple's purpose is to examine how the crisis in the woollen industry under the first two Stuarts, especially the depression of 1620-1624, affected government policy and influenced the thought of the mercantilist pamphleteers, the economic advisers of the period. He argues convincingly that the government's chief concern was simply to maintain public order, to prevent bread riots, and to provide relief by strict enforcement of the poor law. Although the government ill-advisedly supported Cockayne's project, it was in general rather wary of mercantilist prescriptions or grandiose schemes which might miscarry and make the situation worse. Even plans to enhance the currency, that is, to devalue the pound sterling, were rejected in the face of fierce opposition in certain quarters. Half-hearted attempts to control the bullion market and prevent species outflows also failed and their failure brought discredit to Malynes and others who had advocated such a policy. In the end, the government resigned itself to letting the depression run its course while taking measures to alleviate distress. The battle of pamphlets among the early mercantilists ended with the rout of the bullionists and the victory of Thomas Mun who was able to show, to the satisfaction of his contemporaries, that an unfavorable balance of trade was at the root of all evils and that economic policy should consist in promoting the export trade instead of establishing a system of exchange control.

Supple's analysis is sound; his conclusions are in general acceptable. However, he is not always the first to have made certain points, and he is regretably stingy in giving due credit to preceding writers. Usually he refers to them only when he disagrees, but he omits to mention them when he gives exactly the same interpretation. Thus, Jacob Viner is taken to task thrice in critical footnotes, but his contributions are passed over in silence. Eli F. Heckscher and this reviewer receive the same treatment. E. A. J. Johnson is entirely ignored. Only Astrid Friis fares a bit better and gets at least an honorable mention in the preface. More generosity would not have detracted from the value of Supple's work. On the contrary, it would have strengthened confidence in the soundness of his analysis and interpretations.

It is not that the book lacks originality, since it gives more emphasis than earlier studies to the influence of stark reality—a disastrous slump—upon thought and policy. Besides, Supple has been diligent in digging up new material, such as Mun's reports as a member of the Commission on Trade (1622). These reports already embody all the ideas which he later put in his

book, *England's Treasure by Foreign Trade*. Like the depression of the 1930's, the "decay of trade" in the years 1620-1624 spurred economists to search for remedies.

A few points remain debatable. For instance, it is questionable whether, as Supple contends, the mercantilists' aim was simply to retain rather than to acquire and accumulate treasure. Misselden must have realized that enhancement (devaluation) would have boosted exports, since he knew perfectly well that a low exchange rate for the pound sterling was bound to increase the price of all foreign wares and to lower the price of English cloth sold abroad in local currency. This is a matter of simple arithmetic. Besides, Misselden's opponent, Malynes, belabored this point. In agreement with Viner, the reviewer maintains that Malynes was hopelessly confused in some of his analysis. Furthermore, his doctrines contained a strong flavor of scholasticism.

On the whole, Supple's book has the great merit of studying early mercantilism from a pristine point of view. It is a valuable contribution to economic history as well as to the history of economic thought, but it supplements earlier works without replacing them.

RAYMOND DE ROOVER

Boston College

Theory and Policy of Accounting Prices. By A. QAYUM. Amsterdam: North-Holland Publishing Co., 1960. Pp. xi, 123. \$3.50.

This is the twentieth volume of "Contributions to Economic Analysis," a series which includes Strotz and Tinbergen among its editors, and which has presented such well-known works as Theil's *Linear Aggregation of Economic Relations*, Klein and Goldberger's *An Econometric Model of the United States, 1929-1952*, and Tinbergen's *Design of Development*.

Qayum elaborates in this book ideas first proposed by Tinbergen, who was his former teacher and has guided him in its writing. What Tinbergen has stated rather generally, Qayum has narrowed and formalized. The book abounds in symbols and equations, though the mathematics is in fact not difficult, being restricted with few exceptions to first-order differentiation and simple algebra. However, the reader will find that many steps are omitted in deriving formulas.

Qayum maintains that the fundamental cause of underdevelopment is "structural disequilibrium"—a systematic distortion of prices throughout the economy, following from an excessive wage rate (one above the full-employment marginal productivity of labor), and from an unnaturally low interest rate. The results are a high level of continuing unemployment characteristic of underdeveloped societies, and the prevalence of socially uneconomic techniques of production which use too much capital and too little labor. This misallocation of resources constricts national output, and therefore also savings and growth.

The author attributes the nonequilibrium wage rate chiefly to government reluctance to let wages fall to a natural level near or below subsistence, and the artificially low interest rate to a misguided government idea that this will

stimulate development, often encouraged by low-interest though limited loans from international agencies.

To correct these conditions, Qayum following Tinbergen proposes indirect manipulation of private decision-making through carefully measured taxes on capital-use and subsidies for labor-use. This would lead to increased social output from the reallocation of resources more truly in accord with their scarcity or abundance, while preserving freedom (in contrast with direct government planning or price-setting) and marshalling the pre-eminent forces of private initiative.

"Accounting prices" signify the estimates of what the marginal productivities of uncommitted capital and labor would be if fully employed in the technologies best for such resource endowments. The government would leave all prices to the market, including those of factors, but would establish a subsidy rate per unit of labor equal to the difference between the market and the accounting wage rates, and a tax per unit of capital equal to the difference between the market and the accounting interest rates. Each enterpriser in the "accounting sector" would receive from the government (or pay) a net sum equal to the difference between his labor force times the subsidy rate and his capital-use times the tax rate. The "accounting sector" would consist of all production with new investment, excluding activity in agriculture and handicrafts. Qayum's reason for excluding the latter sectors seems to be the seriously imperfect economic calculation typical of self-employed units in such activities; he suggests that the government attack their problems directly through technical advice, etc. A further qualification of the program is that only *unskilled* labor-use would be subsidized, since labor acquires skills through the equivalent of investment, so that skilled labor might well contain an amount of capital that would call for an implicit interest tax approximately equal to the implicit subsidy for use of its unskilled content. The program as a whole would eventually render itself unnecessary: with the full employment of labor and the growth and ultimately correct use of capital stock, each factor's market and accounting prices would move steadily toward coincidence.

The largest part of the book is given to deriving formulas for estimating accounting prices and their effects on national output, on savings, on the government budget, and on the price level. One may wonder how quantitative estimates of these variables can be obtained without empirical data. Qayum evolves the estimates by treating the economy as one process turning out a single good, having a simple Cobb-Douglas production function $P = L^\alpha K^{1-\alpha}$, with P as national product, and L and K as the total quantities of homogeneous labor and capital in use. The parameters to be specified are then the initial percentage unemployment of the labor force, the constant growth rates of labor and capital, the depreciation rate, the duration of time since the introduction of the accounting price policy, and the value of α (Qayum posits .75). With these values given, we can derive, say, the total subsidy (as a fraction of national product) required to bring the entire labor force into use.

Some errors and contradictions appear in the book, chiefly of a formal kind. In one set of equations (p. 37), national saving is derived as net, though it is clearly intended to be gross. Measures of the gains and costs from the

introduction of accounting price policy are derived from formulas which assume that the nonequilibrium market prices of labor and capital equal their marginal productivities for enterprisers, though this could not be true, at least for capital, which is then in excess demand. The proposed rule that already existing plants should be exempt from a newly introduced tax-subsidy program, to protect them from obsolescence and to prevent their abandonment, which Qayum says would follow, indicates a confusion of private with social losses; the exemption would avert only the loss of nominal and exaggerated capital value, and would sacrifice the presumed benefit of shifting to a more labor-intensive use of the old plants (assuming any flexibility of factor proportions). The assumption throughout that enterprisers in underdeveloped economies are rational, foresighted, and progressive, and that their invisible guidance along the course of economic development is thwarted only or chiefly by the existence of artificial factor prices, is in effect denied by the author when he says of the capital markets in such areas (p. 3), ". . . due to lack of initiative and enterprise, the demand is very low even for the small amount of capital which is available."

The author implies that his model is operational. But it is not true that the economy has a single production function and two homogeneous factors. The estimation of specific accounting prices requires working out a picture of the economy's future in considerable detail. It involves a problem in general equilibrium and optimization, taking into account changing demands, alternative and changing technologies, external economies, and shifting supplies of imperfectly substitutable inputs. Qayum recognizes this in Chapter 6, which treats accounting prices as a problem in linear programming on the assumption of discrete processes, in contrast to that of a continuous production function. Here he states (p. 69) ". . . there is little possibility of the derivation of the accounting prices from discrete processes of production in the near future." Yet he implies in the remainder of the book that such estimates can be made more easily when the production function is continuous. What is true is that it is easier to use calculus to differentiate the general form of a production function than it is to solve a linear programming problem; but this is only abstract model building.

Tinbergen, in discussing accounting prices in *Design of Development*, has suggested that rough approximations of their magnitudes could be made which would at least have the virtue of improving on nonequilibrium market factor prices. For example, the accounting price of unskilled labor might be judged to be 80 per cent of the market wage, in the light of existing unemployment. A corresponding subsidy policy could bring full or at least fuller employment through a shift toward techniques with a higher ratio of labor to capital.

This possible result brings into the open a critical premise of accounting price theory and policy: that an underdeveloped economy promotes its most rapid growth by assuring continuous use of all its existing resources, through adopting techniques which reflect its current high ratio of unskilled labor to capital. Along with this proposition should be considered the question whether the profit-oriented judgments and energies of individual enterprisers,

as found in such societies today, and the functioning of free markets corrected for imperfections, provide an adequate engine for growth. The theory of general equilibrium, as embodied in the policy of accounting prices, is primarily a theory of comparative statics, and the usual inferences from it may conflict with the underlying processes of optimal growth and development. It may be more advantageous for a society to use (within limits) modern processes which transform its productive factors, even at the cost of temporary unemployment, than to seek out those methods which find full use for its resources in their existing forms.

AMOR GOSFIELD

Cornell College (Iowa)

The Stages of Economic Growth: A Non-Communist Manifesto. By W. W. Rostow. Cambridge: Cambridge University Press, 1960. Pp. x, 179. \$3.75; paper edition, \$1.45.

There is a peculiar, baffling charm to Professor Rostow's set of lectures. In them history is made simple. The world is a set of nations, or societies (the words are used interchangeably); each one—though a separate creature—goes through a sequence of five stages as its economy develops. The stages have oddly memorable names: traditional society, preconditions to take-off, take-off, drive to maturity, and the age of high mass-consumption. Each stage has a few general characteristics, sketched out in five brief chapters. History, it is said, fits this pattern, resembling a rather jerky horse race (in which a tragic photo finish is just now in danger of developing between the two front runners).

From economic history, the lecturer's interest shifts to the great questions of international politics. Here nations appear as the eternal, indestructible unit in human affairs. They existed in the traditional society, they thrive under the "reactive nationalism" of the transition, and they form the power blocs of the next world. The historical pattern of war and peace reflects in a complex way their relative stages of development. Most exciting is said to be the view of the future which this historical taxonomy can give. Since many great nations are moving toward economic maturity, the game of international power politics will grow more complex and competitive. In this situation, the West must persuade Russia to abandon her pretensions to world religion and, as simply one great power, to accept international inspection of nuclear disarmament. A peaceful world of mature nations can then move on together to see what lies beyond the age of high mass-consumption. As a contribution to this great act of persuasion, Lecture 10 proposes the stages-of-growth sequence as a liberal alternative to Marxism.

Simplicity, relevance, and a high sincerity are important sources of the widespread appeal of these lectures. But there are other sources as well. There are on every page bold assertions immense claims, sweeping insights, unsubstantiated by analysis or documentation. When a professor speaks thus to students, he excites them to look further into the material, to take more advanced courses, even to dip into that "more conventional treatise" where "the views presented here might have been elaborated . . . at greater length,

in greater detail, and with greater professional refinement" (p. ix). How many times, from high-spirited undergraduate days on, must Rostow, like his more inhibited colleagues, have found that such ideas, resembling the other pleasures, Burns wrote about,

... are like poppies spread—
You seize the flow'r, its bloom is shed;
Or like the snow falls in the river—
A moment white—then melts forever.

Surely Rostow has scattered snow liberally here upon the surface of what appears to be rather shallow water. One cannot be sure of the depth, of course, since the current is swift and the author darts like a dragon fly over the dazzling expanse. The applause of economic policy-makers and journalists to his performance betokens an important pedagogical accomplishment. And it is not enough for academic critics, themselves unmindful of the fable of the fox and the grapes, to remind Rostow of the fable of the frog and the ox.

One's chief regret is that, for all his boldness and bravado, Rostow has remained tied to an extremely conventional historical framework. One sees, gleaming up through the water a broken image of the school of Schmolter. The notion of stages is a primitive aid to thought, itself an obsolete stage in the economic historian's grasp of a complex and continuous process. And there is surely an ultimate level on which the world must be seen not as a checkerboard of national case studies, but as a single, developing international industrial society if we are ever to understand its past or to come to terms with its future.

WILLIAM N. PARKER

University of North Carolina

Southern Tradition and Regional Progress. By WILLIAM H. NICHOLLS.
Chapel Hill: University of North Carolina Press, 1960. Pp. xviii, 202.
\$5.00.

The South could do with more leaders—native, trained, experienced, of good will—like the author of this sensible book. Native because in the present scene of collective sectional resentment the "outside" critic is suspect. He is unable to understand the South's soul. His inspection is superficial, his recommendations are inapplicable. His mechanics do not reckon with that undefinable something of which Southerners are supremely aware even if they do not cherish it. As of old, the prophet lacks honor in his own country if he runs counter to popular mores. He is not called a fool, but a knave. In the proportion that his advice is penetrating he is a traitor. His very acquaintance, coupled with adverse comment, makes him immortal. He holds up his own people to derision.

Happily the cry for Barabbas subsides somewhat and stripes begin to be withheld. The Supreme Court is different from Pilate. More and more agree that the secret of the South is resolvable into ignorance, poverty, underemployment, undeveloped resources, overrepresentation of rural electorates, narrow economic opportunity that breeds fierce racial jealousy, and dema-

gogues feeding on exploitation of several sorts. Progress commences to convince the South against itself. Industry, highways, electric power, government installations, TVA planning, army experience that interchanged populations, wage and hour legislation substituting competence for self-pity, all relax tension with the blessed emollient of income. Original sin begins to be dispelled—heaven help us!—by kilowatts and compact cars.

Professor Nicholls, recently president of the Southern Economic Association, and chairman of his department in Vanderbilt University, discusses Southern tradition, prejudice, inertia in five thoroughly informed chapters. Given the desire for betterment, what inhibitions lie in agrarian values, rigidity of the social structure, undemocratic politics, decline of social responsibility, and conformity of thought and behavior? In viewing this parade of negatives the author has the advantage not only of hominy in his diet but also of an extensive knowledge of agricultural economics; he has had a tour of duty with the President's Council of Economic Advisers, has participated in development work in other backward regions, and has made field studies in the South itself. He has both patriotism and perspective, both compassion and candor. If one must confine himself to a single historical survey of the South's culture, this descriptive and analytical section, with all main elements comprehended, must stand high on the list of eligibles. The absence of democracy is a sorrow, and the abdication of aristocracy has made the misfortune worse.

But this wise guide does not stop with deploring. His regrets are but preface to his program for regeneration. If "the South's poverty problem is centered in its nonfarm Negro population and in its farm families, both white and Negro," then "solution . . . requires an acceleration of the rate of industrial-urban development throughout the region." The recommended means are state industrial development commissions devoting funds to research and planning, not to advertising labor docility; a TVA power policy that ceases to handicap private industrial expansion in favor of omnivorous defense projects; federal labor-market information that embraces rural underemployment; more generous public support for educational and health facilities (remember our projected aid in this department to the Congo); encouragement of migration from low-income rural areas unlikely to attract industries affording a good standard of living; credit and technical services to permit those who remain on the land to embark in intensive farm enterprises. Local initiative must refuse to shun outside aid, financial and supervisory; what is threatened by these helps is not the independence of the people, but the dominance of disingenuous, if indigenous, masters. These remedies remind of the advice of Mathew and Henry C. Carey more than a century ago—the South must join plough, loom, and anvil.

Too many talk of the South—its demands and responses—as though all of its people were white. But black and brown have this author's solicitude on every page. And only together can Negroes and Whites advance from custom to contract.

BROADUS MITCHELL

Hofstra College

Comparisons of the United States and Soviet Economies: Parts I, II, and III, Papers Submitted by Panelists Appearing before the Subcommittee on Economic Statistics, Joint Economic Committee, 86th Congress, 1st Session (Washington: Supt. Docs., 1959. Pp. xii, 376; ix, 166; vii, 69. \$1.00; 45¢; 25¢); *Hearings before the Joint Economic Committee, November 13-20, 1959*, Joint Economic Committee, 86th Congress, 1st Session (Washington: Supt. Docs., 1960. Pp. iv, 292. 70¢); *Supplemental Statement on Costs and Benefits to the Soviet Union of Its Bloc and Pact System: Comparisons with the Western Alliance System*, Joint Economic Committee, 86th Congress, 2nd Session (Washington: Supt. Docs., 1960. Pp. iv, 50. 20¢).

These five volumes represent the third published effort in five years by the Joint Economic Committee to get at the problems posed by the growing economic strength of the Soviet Union. The present study commences with twenty-odd prepared papers, essentially technical in character, submitted by panelists who are identifiable for the most part as economists and as specialists on the Soviet Union. These are followed by seven somewhat polyglot papers under the subtitle of "Evaluation of the Russian Economic Threat by Private Policymakers," submitted by representatives of various organizations. The prehearings materials conclude with three papers on "Summary and Policy Implications." The hearings proper consist largely of remarks by the same panelists but include as well a prepared statement by Allen Dulles. The supplementary volume on the Soviet bloc is submitted by the CIA and State and Defense Departments in response to a request made by Senator Javits in the course of the hearings.

What is of principal interest here to the professional reader is contained in the papers submitted by specialists. These are on the whole exceptionally competent and well-conceived expositions of various aspects of the Soviet economy. Some, e.g., the papers of Hans Heymann and Robert Campbell on the general problems of U.S.-Soviet comparisons, are intelligent restatements of knowledge that is more or less common among specialists in the area. A second group of papers, e.g., Warren Eason on labor force and Ernest Williams on transportation, derive from earlier writings by their authors but are none the less useful on that account. A third and quite large group represent summaries of the findings of extensive and prolonged studies of which complete versions have not yet been published.

Of the last group, two papers are especially noteworthy: one by Herbert Levine on Soviet planning and another by D. Gale Johnson and Arcadius Kahan on Soviet Agriculture. Levine's paper, particularly if taken together with J. M. Montias' recent article in this journal, is much the most illuminating explanation of Soviet planning yet to appear. Levine stresses more heavily than does Montias the "forcing" of technical coefficients as a device for obtaining consistency in the material balances and discounts the importance of iterative procedures, but the difference looks to be more expositional than substantive. The Johnson-Kahan essay presents new indexes of Soviet agricultural output and various measures of inputs and productivity. While these findings appear broadly consistent with those of previous studies,

they are considerably more detailed and more comprehensive in the time periods covered. The reader should note that the statements of these two contributors in the hearings include additional material: a comparison of their output estimates with a recently released official index and a summary of their estimates of real per capita earnings in agriculture.

The subject of agriculture is perhaps the most effectively dealt with in the entire proceedings. Johnson and Kahan on outputs and inputs, Nancy Nimitz on prices and costs, and Lazar Volin on general organizational and policy matters, cover Soviet agricultural problems fully and penetratingly, though it would have been well if Miss Nimitz had had space enough to argue her important hypotheses on the stimulating effects of recent agricultural price reforms. Similarly complementary papers are provided by Robert Allen, Franklyn Holzman, and Henry Aubrey, on Soviet foreign economic activities. Other individual papers too numerous to list are equally competent.

The technical papers assembled here constitute, then, an exceedingly useful collection of readings. Especially in view of the outdated by the post-Stalin changes of older text materials, these essays will undoubtedly be drawn on heavily for classroom assignments. Other readers will find in them a handy (though not tightly organized, entirely comprehensive, or fully documented) survey of our present knowledge of the Soviet economy.

Viewed as a whole and as an effort to render counsel on a problem of national policy, these proceedings are considerably less satisfactory. The central question at issue, clearly if not altogether explicitly, is the size and significance of the threat to this country created by the relatively high rate of Soviet growth. That the Soviet growth rate does currently exceed ours, by better than 2 to 1 measured by GNP or industrial production, and that it will probably continue to exceed ours, is generally agreed (see, among others, papers by Warren Nutter, Morris Bornstein, and Allen Dulles). The greatly preponderant view expressed here, however, is that this differential is no cause for alarm, or, perhaps more accurately, not so much cause as one might think or less cause than other things one might name.

This effort to minimize the significance of growth rates is not in the main the responsibility of the panelists who contribute the technical papers (exceptions are cited below.) Either on instructions from the sponsors of the proceedings or as the automatic consequence of the narrowness of their topics, the specialists for the most part restrict themselves to the providing of facts. The drawing of inferences is left to the nonspecialists.

Among the latter, Howard C. Petersen, here representing the Committee for Economic Development, undertakes the single systematic and extended appraisal of the significance of growth rates that appears in these volumes. Petersen argues roughly as follows: Economic capacity is not determining ("not decisive") for the volume of power-related expenditures because either the United States or the USSR could increase such expenditures if, as a matter of policy, it chose to do so. The Russians *may* not use the power for mischief which they possess, and their economic successes *may* lead to favorable internal political changes. The loyalties of uncommitted peoples are less likely to be affected by relative growth rates in the United States and

USSR than by those of China and India (this presumably is not meant to be reassuring). In any case, accelerating our growth rate is likely to be expensive: we now spend about \$75 billion a year for growth-affecting purposes (net investment, education, and research); as a first approximation, it would appear necessary to at least double such outlays in order to double our growth rate. Whether outlays that affect growth are assumed also to maximize growth, or whether it is assumed that all such outlays would necessarily be increased in equal proportion, is not explained.

In the first of the three summary papers, Willard Thorpe, who does very neatly summarize the submitted papers, relies for his policy inferences largely upon Petersen, though he does not accept all of Petersen's arguments. W. W. Rostow opens his summary paper with the statement: "I believe I speak for virtually all the panelists [about 4 out of almost 30] who addressed themselves to the implications of their analyses for American policy when I say this: Our dangers do not lie primarily in the size of the Soviet economy or its overall rate of growth." Rostow's reasons for deprecating growth rates are much like Petersen's, but, unlike Petersen, he is greatly concerned that we improve our present allocation of U.S. output.

Among the panelists responsible for the technical papers, only Heymann deals at any length with the implications of the growth-rate differential, and he also plays down its significance. In the hearings (p. 42), Heymann makes the further observation that, once this nation faces up to its real challenges, "growth will follow automatically, and enthusiastically, without anyone doing anything directly about it." Joseph Berliner in his remarks in the hearings (p. 142) explicitly endorses Petersen's position.

The foregoing resumé is unfair in that virtually all of the contributors cited concede some significance to growth rates, and most of them do in fact accept the need for accelerating U.S. growth. But this significance is allowed expressly as a concession, and the conclusion driven home is that growth is not of "primary" or "decisive" consequence. That the committee members so understand the point of the discussion is clear from the hearings (see, e.g., Representative Curtis' remarks on page 23, Representative Bolling's on page 41).

The contrary view is not entirely unrepresented. Bornstein in his prepared paper gives a brief but ungrudging statement of the significance of growth rates. Gerhard Colm, representing the National Planning Association, treats with quite even emphasis the problems of growth and other problems that confront us, and asserts both the need for and the feasibility of accelerated growth. Harry Schwartz, in the third of the summary papers, expresses his alarm vigorously, but he relies for argumentation (as adherents of this view may be too prone to do) on the sheer impact on the beholder of statistics of projected outputs. Allen Dulles, who has elsewhere shown concern with growth, makes in his prepared statement only a passing reference to the possibility of a "dangerous" narrowing of the gap between the two economies; the questioning set off by this word does not do much to establish wherein the danger lies. There are a few other expressions of alarm scattered through the hearings (see especially the testimony of Evsey Domar).

Nowhere, however, is there a brief for this position comparable to Petersen's. The dominant voice throughout remains that of participants who are effectively unalarmed.

A possible explanation of at least part of this odd imbalance begins to emerge clearly only at the last session of the hearings. The significance of growth rates, their role as success symbols aside, presumably relates to the long run. Domar in his testimony (pp. 245 ff.) raises considerations which are explicitly or necessarily long run: "What will the situation be if and when Soviet output matches ours, and then exceeds it, say, by some 50 per cent?" ". . . Economic growth and technological and scientific progress are longrun processes: The school children of today are the scientists of tomorrow, and their research of tomorrow forms the basis of growth in years to come." Schwartz (p. 254) picks up the same thought in criticizing Dulles for restricting his projections to 1970: ". . . there is no reason to suppose that the world will end in 1970." "We have children who, we hope, will live beyond that date." The point is evidently acknowledged: Thorpe, reiterating his earlier argument that growth is not consequential for foreign economic competition, adds here (p. 255) the phrase, "at least in the short run." Rostow (p. 270) declares that 10 years "is about as far ahead as I can look operationally."

From this exchange it appears likely that a crucial but unstated point at issue throughout these lengthy proceedings was the appropriate time horizon for the planning of national policies. Had this been brought out earlier, it would at least have made clearer where real disagreement lay. It might have permitted as well an expressed agreement on a conclusion to which many of the participants on both sides of the argument implicitly assent: we face not one danger but two. For the near future, the danger is that we may allocate too little of our output to the uses that determine our security and our influence in the world. In the long run, if our growth rate continues to lag as it has in the recent past, we may reach a point where reallocation alone will no longer meet our needs. By that time, of course, it will be too late to look to our rate of growth.

Whatever purpose may be served by disputing which of the two is the greater danger, it scarcely appears wise to magnify one by minimizing the other.

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Statistical Methods; Econometrics; Social Accounting

Economic Arithmetic. By ROBIN MARRIS. London: Macmillan and Co. Ltd., 1958. Pp. xvii, 349. \$4.50.

The declared purpose of Mr. Marris's book is to provide an explication of the broad statistical descriptions of the British economy now available, and to aid the reader in integrating economic theory with the statistical techniques by which that theory is applied. Marris carries out his plan in three parts: Part I is a study of official British national income and expenditure statistics, Part II is given over mainly to an exposition of the rudiments of time series

and regression analysis, while Part III is a long and detailed discussion of the formal properties of index numbers and their application to the measurement of changes in family and national real income.

Marris opens his explanation of the nature and sources of British national income statistics by sketching the main features of the economic process that generates the data. This introductory material is followed by a lengthy reading of the important income aggregates that are annually published in the official government Blue Book entitled *National Income and Expenditure*. Although Marris follows closely the organization of the Blue Book, his exposition does not utilize a single statistic from that source. This is an unfortunate omission, for it has the double effect of making the subject seem unnecessarily abstract and his explanation less valuable than it really is. His work is helped, however, by an ingenious diagram that schematically depicts the relations between the aggregates discussed in the text. Marris's work would have been further helped had he taken the space to write down these relations algebraically. As it is, though, American students who possess a knowledge of national income accounting will find the first three chapters of this book a satisfactory guide to British social accounting practice.

The remaining five-sevenths of the book is devoted to a number of problems concerning the manipulation and interpretation of economic data. Part II opens with a very nice chapter on the value of simple inspection of the data—which turns out not to be an entirely simple matter. Marris then goes on to explain, in the following two chapters, the basic aspects of time series analysis and the least-squares technique of fitting regression equations. Although he develops these subjects in a routine fashion, his warning that little can be inferred from the computations made unless the investigator has "a reasonably correct idea of the nature of the system which is governing data" will be of especial help to beginning students. Marris closes this section with a mention of the method of simultaneous equations.

Part III furnishes a good example of how economic and statistical theory can be usefully combined. Marris begins this section by verbally developing the general nature of index numbers, and follows with a carefully done algebraic exposition of their formal properties. He then combines the theory of consumer choice with index number theory and explains the problem of measuring changes in family real income. He ends the book with a long and excellent chapter on the economic and statistical questions involved in measuring changes in real national product. Though no final solutions to the many problems connected with this difficult question are provided, this chapter is valuable because it is thoughtfully worked out and intelligently written.

PAUL WELLS

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Economic Systems; Planning and Reform; Cooperation

Beyond the Welfare State. By GUNNAR MYRDAL. New Haven: Yale University Press, 1960. Pp. xii, 287. \$4.50.

"Economic Planning and Its International Implications," the subtitle of this book by the well-known Swedish economist, best describes its contents.

The book primarily concerns the past growth and present tendencies of the welfare states of the rich Western powers. The incidence of their policies upon the poorer countries of the non-Soviet world is assessed.

Myrdal's main subject is the continuing evolution of Western welfare states towards his ultimate ideal of being "the people's home." He stresses the ways in which, through a series of *ad hoc* government interventions, a "created harmony" and "social convergence" have already come into being without conscious plan. Hence many once necessary statutes, in labor matters for example, are now anachronistic. He looks forward to the day when much of this legalistic and detailed intervention by the state can be replaced by a new infrastructure of cooperative associations and local boards of citizens.

Unfortunately, in some countries such as the United States, national planning has not yet caught up with this need for coordination and simplification, but still takes the form of regimentation and tinkering by state organs. However, in some of the "advanced" welfare states, the habit of cooperating and bargaining in the collective organizations below the state level is well developed. In these relatively established and homogeneous societies the citizenry participate increasingly in an evolving form of economic democracy.

Some of these views may seem to Americans like a description of Utopia. My own feeling is that Myrdal's description of the rich welfare states applies only perhaps to Scandinavia, Benelux, West Germany and Great Britain. Surely the United States does not exemplify the planned economy—defined as *coordinated* economic intervention—Myrdal attributes to his welfare states. He admits, too, that the United States is something of a special case, not because it is big or "young," but because white and black immigration has made it heterogeneous. To this he attributes the lack of citizen participation in a coordinating infrastructure and the prevalence of political bossism and labor union gangsterism.

As for the underdeveloped countries, Myrdal explains why, compared with the rich welfare states, they practice so much less economic planning although officially they are so much more in favor of it. He stresses their need, not only for technical and capital assistance, but especially for more assured markets at more stable prices in the rich western countries. He regrets that the inevitable nationalism that accompanies the welfare state has made international economic integration more difficult than ever. But if the world economy is to be restored, it will not be through a return to some automatic money standard but through a more sincere use of new and established international agencies. In this respect he deplores the comparative failure of UNESCO and the IMF, the obsolete attitudes of the ILO, and the too modest scope of the IBRD. He places the blame for these inadequacies primarily upon the government officials of the rich western nations. Here one senses, between the lines, the frustrated international civil servant.

Some of Myrdal's best and perhaps most original pages concern the psychological reactions of people in the western countries to economic planning, foreign aid, and to various institutions that affect their economic lives. Some of these insights were a revelation to this reviewer. They serve as a valuable reminder that economics is after all a social science.

This book is based upon various lectures and articles to academicians and intellectual laymen. So the presentation is in very general terms and is not directed primarily at economists. Perhaps for the same reason there is considerable repetition. Occasionally one would prefer a little less heavenly sublimeness and a few more earthly facts. But many American economists will find much that is novel and provocative, especially if they have not read other recent publications by the same author, and we can all envy his notable command of the English language.

STEPHEN ENKE

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A Humane Economy—The Social Framework of the Free Market. By WILHELM RÖPKE. Chicago: Henry Regnery Co., 1960. Pp. 213. \$5.00.

On rare occasions, at just the time when a productive stream of human history seems to have reached a dead-end, a book has appeared with the power to regenerate tradition and to free men anew for positive, productive action. Wilhelm Röpke, internationally acclaimed economist, whose theories-in-practice have been credited with the revitalization of the West German Republic, has written such a book.

The very title of Professor Röpke's study is enough to give twentieth century man new hope; for during the last fifty years we have been apparently caught in the dilemma of having to choose between the arbitrary tyranny of the totalitarian system and the free, but coldly impersonal, working of old-fashioned *laissez faire*.

In *A Humane Economy* Röpke has provided, not a middle-of-the-road compromise between two rival ideologies, but a new synthesis which transcends them—an economics which liberates the whole man both from the degradation of slave labor and from the selfishness of unbridled materialism.

So says the jacket. Can one book do all this? The first chapter is a "re-appraisal after fifteen years" of trends and events in the world economy. In general, it concludes that the "road to serfdom" has flowered into an international, interlocking highway system. Unfortunately the economic, social, philosophical and other ideas are mixed with personal opinions of the author without being too well integrated by analysis. Hence there is some tendency toward overgeneralizations. For example, it is said that in the West today "the dominating and prevailing opinion is completely atheistic" (p. 9) and Marshall Plan aid was "tailored to the needs of socialist countries" (p. 22); but, fortunately, "there is no point now of continuing this list of embarrassed evasions and absurdities because they have long been answered by the facts" (p. 25). However, the conclusion is sound. "The decision on the ultimate destiny of the market economy, with its admirable mechanism of supply and demand, lies, in other words, beyond supply and demand."

The second chapter is a long critique of modern mass society and an acute analysis of recent worldwide population growth trends. These are provocative and bristle with interesting though often isolated ideas. But even this long

chapter seems too short to do justice to its main arguments which include a case against the "deplorable" decline of chivalry and rural life (which are assumed to be closely interrelated) and the rise of the new trinity of advertising, socialism and boredom. This curious equation might well have been further explained.

Third, there is a long chapter on the market system, beginning with an idealized version of classical economics. Again, an otherwise strong case for 19th century liberalism is marred by oversimplifications. For example, the words totalitarianism, socialism, nationalism, collectivism, communism, and mass society are used more or less interchangeably. The real struggle between the "free world" and communism is seen to be on the spiritual, not economic, level whereas communism's main weapon is said to be in production and living standards (p. 111). How this curious conclusion can be derived from a reading of Marxian and classical economic philosophy is not entirely clear.

The central issue is stated, "What will happen when these individualist motives induce people to do things which are manifestly harmful to others? . . . Is it enough to appeal to people's enlightened self-interest to make them realize that they serve their own best advantage by submitting to the discipline of the market and of competition? The answer is decidedly in the negative. . . . The market economy is not enough" (pp. 122-23).

Granted the market economy is not enough. Where is the answer sought? It is in "Self-discipline, a sense of justice, honesty, fairness, chivalry, moderation, public spirit, respect for human dignity, firm ethical norms—all of these are things which people must possess before they go to market and compete with each other. These are the indispensable supports which preserve both market and competition from degeneration. Family, church, genuine communities, and tradition are their sources" (p. 125). In describing and analyzing these nonmarket factors Röpke is at his best and the arguments are eloquent and instructive. But the conclusions are indefinite. There are many interesting side-trips into ideas of Dickens, Macaulay, C. W. Eliot, Pascal, Montesquieu and others but no attempt to integrate them.

The advertisement promises a "new synthesis which transcends rival ideologies." Possibly the word synthesis means something else in English than its German equivalent because no synthesis in the accepted English sense ever appears. Röpke does state clearly some basic, philosophical issues but the "synthesis" usually takes the form of eloquent defense of one position and a strongly worded attack on the other. Sometimes, however, the "synthesis" takes the form of stating both sides of a controversial question and then agreeing with each side in turn without explaining how the debate comes out. For example, on page 121 the author states, "Any attempt to base an economic order on a morality considerably higher than the common man's must end up in compulsion," while on page 130 he says, "Every society should have a small but influential group of leaders who feel themselves to be the whole community's guardians of inviolable norms and values. . . ."

Another long chapter is devoted to the twin dangers of the welfare state and chronic inflation. We are exhorted to resist these. As in the section on the

market system, the discussion runs somewhat afield with many interesting observations on everything from Greek plays to American advertising. Although Röpke stated earlier that the task of economics is "to make the logic of things heard in the midst of the passions and interests of public life to weigh everything and assign it its due place" (p. 149-50), he finally concludes that neither economic theory nor statistics is of any use and even national budgeting is futile (p. 253).

In the concluding chapter the issue is restated in terms of two kinds of social thought, "centrism" and "decentrism." The good decentrist "thinks in terms of human beings and also knows and respects history" (p. 229). The bad centrist, however, "misuses big words such as freedom, justice, rights of man, or others to the point of empty phraseology, who poses as a paragon of virtue and stoops to use his moralism as a political weapon" (p. 229-30).

This book is not easy to evaluate because it was written in German and translated into English. A reviewer's criticism should be directed toward the substance of a book rather than the appearance and hence should concentrate on the underlying thoughts as expressed by the author, not the words in some foreign language edition as chosen by a translator. The book is interesting and stimulating; but overtones and innuendoes play so much of a part that this review must consider them to be intended by the author rather than the translator. The book abounds with anecdotes, side comments, brilliant insights, and over-simplifications to such an extent that the main theme is often lost. If any conclusion can be drawn from the morass of opinions it is that solutions to all problems can be found in the unfathomable soundness of the "right" way as against the wrong or "left" way of doing anything.

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Zagadnienia ekonomii politycznej socjalizmu. (Problems of Political Economy of Socialism.) By OSCAR LANGE and others. 2nd edition. Warsaw, Poland: Książka i Wiedza, 1959. Pp. 622. zł 30.

This is a joint work of Oscar Lange and twelve other leading Polish economists (most of them lecturing at the University of Warsaw). It is the first attempt at theorizing on the basis of the experiences of 15 years in the Polish socialist economy. The book should be of interest to any Western economist, if only for the reason that the socialist economic system is relatively young and, in consequence, theoretical syntheses of it are still very few. Marx and Engels dealt almost exclusively with the then existing capitalist system and much the same could be said about the major part of the works of Lenin. Owing to the specific situation of the USSR in the interwar and the immediate postwar years there are not, at present, many general theoretical treatments of the working of the socialist economy in that country; and it is only during the last decade that Stalin's "Economic Problems of Socialism in the USSR" has appeared, and some theory has been included in the well-known economic textbook published by the Soviet Academy of Sciences. Some theoretical papers have been published lately by Strumilin, Ostrovitianov, Kronrod, Kantorovich and a few others.

The present Polish work does not attempt to formulate a complete theory of political economy of socialism based on Polish practical experience.¹ Nor does it, as a matter of fact, regard any of its findings as clear-cut or final. Socialism, as a new economic and social system, is undergoing changes. The work of the Polish economists furnishes a good example of this. Its first edition contained two papers by Oscar Lange ("Political Economy of Socialism" and "Fundamental Problems of the Period of Building Socialism") and, further, a paper on socialist industrialization (Rutkowski), on the influence of agricultural development upon the growth of national income (Herer), on certain problems of dynamics of a socialist economy (Kalecki), on conditions of a general equilibrium between production and consumption in a socialist economy (Laski), on models of a socialist economy (Bobrowski), on certain problems of prices in a socialist economy (Brus), on problems of economic choice in planning and on the problem of prices (B. Minc), on prices of agricultural products (Pohorille), on wages in a socialist economy (Morecka), and finally, on costs and optimal production in a socialist economy (Fiszel).

The first edition printed in 10,000 copies, became out of print after six months which would seem a record for a country of some 29 million population. Yet, 10,000 copies of the second edition published in December 1959, went out of supply by July 1960. What, however, is more interesting is that, because of developments during 1959, not only did the eleven papers of the first edition have to be supplemented in the second by the addition of two more (on "Calculating the Economic Effectiveness of Investment," by Rakowski and on "Problems of Planning Economic Development," by Pajestka), but in addition some of the views expressed by authors in their first-edition papers had to be, here and there, modified in the second edition.

Moreover, the ideas expressed by the authors on several fundamental economic problems are not uniform. A good example is furnished by two fairly divergent theories on prices in a socialist economy presented by Brus and Minc. Their views differ widely—and so do certain other theories advanced by their colleagues. The book as a whole gives the student of economics an opportunity to assess the difficulties of theoretical generalization applied to a national economy the socialist development of which has been going on for one and a half decades under specific conditions (the location of the country close to Central Europe, a certain level of economic development achieved before the second world war, the unbelievable destruction caused by war and by the German occupation, the difficulties of development during the "cold war" period, etc.). The experiences of other socialist countries must necessarily have been somewhat different, according to the stage reached in their economic development at the time of change in their economic and social systems, according to the domestic and other conditions accompanying their early socialist development (e.g., the threat of German aggression looming upon the USSR in the 'thirties), etc. No wonder that, despite the uniform Marxist-Leninist ideol-

¹ Lange published a few months ago the first volume of his "Political Economy" (*Ekonomia polityczna*), Warszawa 1959. This is an introduction to a major three-volume systematic treatise of Marxist political economy. The French translation was to be published at the end of 1960, the English one is somewhat delayed.

ogy, socialist development in practice has had to vary among the different countries and that, consequently, every attempt at theorizing about this practice must be made, at least in its first formulation, with a great deal of caution as well as with a perfectly open mind.

In his first paper, on the "political economy of socialism," Lange states clearly that, as a result of the past practical experience of the USSR, China, Yugoslavia, Poland and other socialist countries "the situation is slowly becoming ripe for a synthetic, theoretical analysis of the principles of political economy of socialism." "Of course," he goes on, "such a synthesis is bound to have a preliminary and temporary character only. There is much discussion among Polish economists, but there also are many divergences of views as far as those matters are concerned. This is quite natural if one takes into account the still very raw stage of development of these problems. We are by no means going to be surprised if these opinions, as far as certain problems are concerned, undergo change as a consequence of further studies or of further practical experiences within the socialist economy."

These experiences are multiple, and constant theorizing about them is bound to lead to more new and new findings of a general character. It is obvious that factual differences between the stages of development in the building of the new system may reflect these differing experiences and, consequently, that divergent theories may be produced *at a given moment*. There would be nothing new in this, as exactly the same thing happened in the 17th and the 18th centuries when a variety of experience with capitalism led to different and steadily evolving theories concerning the then developing economic and social system. Gradually, of course, the differences with regard to certain specific points will be smoothed down.

Thus the two editions of the Polish *Zagadnienia ekonomii politycznej socjalizmu* may be of considerable interest to economists, no matter what their personal attitude toward Marxism may be. They provide not only a sketch of the theoretical analysis of the economic consequences of socialism in Poland, but also they reveal the constant evolution of Marxist thought that is taking place in the light of a constantly accumulating wealth of experience derived from the steady growth of a specific national economy.

TADEUSZ LYCHOWSKI

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Wirtschaftsrechnung, Investitionen und Wachstum in einer Zentralverwaltungswirtschaft. By KLAUS DIEKMANN. Berlin: Duncker & Humblot, for Osteuropa-Institut an der freien Universitaet Berlin, 1960. Pp. 144.

This book consists of three related discussions: (1) a critical review of prior literature on the subject, including some comparisons with actual Russian experience, (2) an investigation into the problems connected with the application of linear programming and electronic data-processing to central planning in a fully controlled economy, and (3) an analysis of the relationship between growth and investment under certain assumptions.

Diekmann begins by discussing Marxian value theory; he presents arguments on both sides but shows in detail how selection of alternatives for pro-

duction processes brings unrealistic results if return on invested capital is ignored. Russian planners therefore have had to develop various disguises under which the concept of interest is applied to decisions, without offending the ideological requirement of its nonexistence.

The devices have taken many forms: Kukel-Kraevskij advocated selection of the alternative with the smallest total of investment and running costs for several years; A. L. Lure suggested opportunity costs as a guideline (he calls them relative effectiveness of capital); S. G. Strumilin constructed a system of indexes to serve the same purpose; F. S. Mstislavski went even further with a detailed scale of preference coefficients based primarily upon "underlying" and current investment requirements, and the French economist Ch. Bettelheim developed an evaluation of opportunity costs somewhat similar to Lure's. Most of these methods were rejected by official Soviet policy in favor of an approach based upon *Rueckflussfrist* (recouping period, during which the investment is amortized). However, any analysis would have to be based upon economic preferences only if there are no socially more important considerations favoring any one alternative.

The next chapter is devoted to a critique of Paul Hensel's *Einfuehrung in die Theorie der Zentralverwaltungs-wirtschaft* in which Hensel describes his concept of thorough planning: the division of goods into a series of classes depending upon their relationship to a primary list of goods scheduled directly by the planning authority. Diekmann's own model, which follows, shows many similarities to Hensel's, except that Diekmann places greater emphasis on production processes to be used. He shows that both demand and supply conditions can in fact be expressed with series of linear equations as long as the assumed conditions of full knowledge and control by the central authority are maintained. By grouping products and with the help of the simplex technique he reduces the task to a magnitude within the capabilities of a large electronic computer installation. If his expectations are correct, the job can be completed in less than one month, certainly quickly enough to make the information usable as the blueprint for a detailed plan.

In the last section of the book Diekmann discusses the apportionment of investments among consumer-goods and capital-goods industries as well as the influence of this proportion upon economic growth. He considers this exposition necessary for the completion of his argument since he has not previously distinguished between consumer goods and production goods. Again he builds mathematical models upon a series of assumptions similar to the previous ones. Unfortunately he decides to ignore the effect of investments necessitated by research and by new inventions on the ground that they would be too difficult to evaluate. As can be anticipated from this stipulation, the saturation point for investments (based on previously known technical achievements) is ultimately reached and any further investment must parallel population growth or be completely futile.

Diekmann is an excellent mathematician; in his earlier chapters, when analyzing the models of others, he is quick to point to inevitable results from the basis upon which some of them are built. It is hard to understand there-

fore why he himself falls victim to similar errors when building his own structure.

Despite this shortcoming the book is a valuable contribution to the rapidly growing body of information about the mechanics of planning for a modern economy. It points up the sharp dichotomy between the highly sophisticated ability of our society to solve those problems which we can define mathematically and the backwardness with which we approach social problems, such as a determination of utility and usefulness which, in Diekmann's discussions as well as in the actual Russian case, is left to the whim of a few individuals in authoritative positions.

ERWIN RAUSCH

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Money, Credit and Banking; Monetary Policy; Consumer Finance; Mortgage Credit

Money Metropolis. By SIDNEY M. ROBBINS and NESTOR E. TERLECKYJ, with the collaboration of IRA O. SCOTT, JR. Cambridge: Harvard University Press, 1960. Pp. xvi, 294. \$5.00.

The Money Side of "The Street." By CARL H. MADDEN. New York: Federal Reserve Bank of New York, 1959. Pp. 104. 70¢ (35¢ if purchased in quantity for classroom use).

The New York money market, the nexus of our financial system, has attracted little scholarly attention, so these two books will fill a large gap. Madden's contribution is essentially a revision and extension of *Money Market Essays* written by several members of the Bank's staff in 1952. The monograph by Robbins, Terleckyj and Scott is the fifth volume in the New York Metropolitan Regional Study undertaken by the Harvard Graduate School of Public Administration at the request of the Regional Plan Association. The over-all study examines the strategic economic and demographic features of the region (a 22-county area straddling the boundaries of New York, New Jersey and Connecticut) and projects them through 1965, 1975, and 1985.

In the first two-thirds of his book, Madden describes the market for short-term funds and the principal types of instruments traded; in the third section he concisely explains the Federal Reserve's role as guardian of the money supply. The main tasks of Robbins and his colleagues are to explain New York's hegemony and to appraise the forces which might weaken it. The key variables studied are employment in the various segments of the finance industry and changes in its distribution within the region and between the region and the rest of the country during the years 1947-56. Employment patterns indicate whether financial activities are labor- or market-oriented, and when employment is combined with output it provides a rough index of productivity trends. Robbins and his colleagues first examine the money market and the hinterland of financial institutions surrounding it and then take up the projections of financial employment through the ten-year intervals noted above. In an appendix on statistical methodology, they discuss the assumptions underlying their computation and extrapolation of labor-output ratios for each major

segment of the finance industry. While they use historical data on jobs and financial transactions to determine the ratios, the authors are careful to adjust the estimates for expected changes in the relation of manpower to output.

The heart of the market consists of a few hundred trading specialists and the managerial elite in the major banks and other financial firms—all bound together by a network of instantaneous communications. The money market core itself accounts for between 5 and 10 thousand of the 120,000 employees in financial activities in lower Manhattan (Robbins, p. 25), but it is the keystone of the entire structure. The main force attracting participants is the desire to reduce uncertainty and avoid the risk of trading on highly unfavorable terms (Madden, p. 13; Robbins, p. 34). External economies (legal firms, printing establishments, etc.) also pull functions to the center. Both books deal adequately with the money market's stock-in-trade (United States government and other securities, federal funds, commercial paper and bankers' acceptances—since the call money desk at the New York Stock Exchange was recently closed). However, Madden highlights the liquidity inherent in a viable money market which enables banks to adjust quickly their reserve positions and allows individuals and nonbank intermediaries to shift their portfolios without incurring insufferable capital losses (Madden, pp. 14-17). On the other hand, in Robbins' wider historical perspective one can trace the migration of the money market center from Philadelphia to New York during the first quarter of the nineteenth century; this was primarily a response to New York's faster rate of economic growth, the willingness of its financial institutions to specialize, and constructive legislation. Neither book, however, discusses the continuing weakness in market arrangements which allows speculators in government securities such favorable opportunities as they enjoyed in the spring and summer of 1958.

The large Wall Street banks link the money market with the rest of the financial community. Madden concentrates on the shift of most of the traditionally "wholesale" institutions to "retail" banking. A similar shift in the country at large, according to the Robbins team, is one of the main factors behind the increase of 16 per cent in commercial banks' loans and investments (in constant 1955 dollars) between 1947 and 1956. If taken alone, this expansion would have raised national employment by 16 per cent. But changes in bank portfolios (spearheaded by the enormous rise in consumer credit) stimulated a potential growth of 70 per cent in the demand for labor which was only partly offset by improvements in productivity. On the other hand, if productivity had improved while the other variables remained unchanged, employment would have shrunk by almost one-quarter. Within the New York region, no growth occurred in total loans and investments; this is partly attributable to legislation which until early 1960 generally kept city banks from tapping the large reservoir of deposits in the suburbs. Moreover, the city banks make about one-half of their loans to borrowers outside the New York region, and this too depresses deposit growth (Robbins, p. 85). The enlarged scope for branches will undoubtedly enable the New York banks to mobilize additional deposits, but they will probably never recover their former commanding position. This is not true of the banks' foreign business, however, because the

nation's expanding commitment in international trade will also enhance New York as a world money market.

Beyond the commercial banks lies the massive range of life and property insurance companies, securities firms, mutual savings banks, etc. Madden does not venture into this territory, but the Robbins group explores it in some detail. Employment in these industries in the New York region grew only moderately between 1947 and 1956, and the largest lag centered in life insurance companies, especially in home office employment. The hesitant performance of life insurance companies is principally due to the region's smaller increase in personal income compared with other sections of the country. Life insurance companies are concentrated in mid-town New York and Newark, but increased mechanization of routine home-office operations and the drift of agencies to the suburbs may weaken (but not sever) the industry's labor orientation. The necessity of property insurance companies to reinsure their highly volatile risks will maintain proximity among them, and their sizable holdings of common stocks will also pull them to the money market core. Despite the periodic threat of a New York city tax on stock transfers, the securities industry is likely to remain rooted around the two securities exchanges in New York which account for 90 per cent of the volume of trading on the 17 organized exchanges in the country. The spread of electronic data-processing equipment in securities firms will almost certainly reduce their demand for clerical employees and may centralize some of the record-keeping now done in the branches. The remaining segments of the financial community (primarily savings-type institutions) have always been population-oriented, and this pull will undoubtedly continue.

By 1985, employment in the country's financial institutions is expected to be more than twice the level in 1956, but the New York region's share will probably decline to 14 per cent compared with 19 per cent in the base year. Within the region, the older cities are expected to have two-thirds of the financial employment in 1985 in contrast to 88 per cent in 1956, but some absolute increase will occur—mostly in mid-town New York. Nevertheless, the money market core is likely to keep intact the present configuration of financial institutions in the Wall Street area.

ANDREW F. BRIMMER

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Studies in the Theory of Money, 1690-1776. By DOUGLAS VICKERS. Philadelphia: Chilton Co., 1959. Pp. ix, 313. \$6.50.

This book reviews the contributions made to monetary theory by nine writers: Locke, Barbon and North; Law, Berkeley and Vanderlint; Cantillon, Hume and Steuart. Vickers has organized them into these three groups of three, labeled respectively "Definitions," "The Inflationists," and "Process Analysis." A preliminary 40 pages and a concluding dozen complete his study. As the jacket states, the book deals with the "important but neglected hundred years" preceding the *Wealth of Nations*. As such, it is a welcome contribution towards our understanding of the basis of classical economics, as well as a thorough study of the individual work of the selected nine.

Vickers would have eased his readers' task if he had been able to construct a Procrustean bed of theoretical topics and to adapt the analysis of each of his nine subjects to fit it. This might well have required too surgical a simplification; but as it is, no two of these chapters are organized in the same way. The author is also rather prone to use some of our less helpful forms of jargon. In addition, a number of important points are made in the footnotes instead of in the text. And, doubtless because the chapters on individual writers are extended, the chapter summarizing the exposition is highly condensed. It must be said that the outcome is a very difficult book to digest.

This is a pity, because it makes a number of points well worth making. That, in the period covered, the existence of unemployment and the dangers of hoarding money were continual stimuli to economic thought, is now generally appreciated; but Vickers reminds us in detail how fundamental these stimuli were to the evolution of monetary thinking. By picking out a series of writers, spread over a century, he has also been able to show the relationship between contemporary reactions not only to these social problems, but to the very different historical conditions which were successively encountered. And his classification of his nine writers elucidates the progressive emergence of monetary theory from the comparative statics of Locke and his contemporaries. In Vickers' terms, this evolution was from an analysis "equational-definitional in the static sense" through the "macro-dynamic analysis of economic activity" achieved by the Inflationists, to the "dynamic analysis of the money-flow process" of Steuart.

Such discussions of individual writers encounter, of course, formidable obstacles. It is easy to appear to attribute to the individual under study a discovery which was in fact common ground amongst his contemporaries. An occasional clear-sounding statement of a now-familiar truth may lead the enquirer to assume a logical insight which the writer did not in fact possess. The recurrence of a similar statement in a later writer may lead to the equally incorrect conclusion that there it was derivative. On the other hand, one may miss the significance either of a single conclusion or of the connection between two writers' views because of the unfamiliarity, or the sheer looseness, of the terminology used. How far Vickers can be held to have avoided all these pitfalls depends upon the interpretation placed on many ambiguous passages. The present reviewer has noted a few quotations where the context does not appear wholly to support the author's conclusions; and a few others where the significance which he attributes to the phrases transcribed is at least open to doubt. But Vickers has quoted extensively and aptly, and to a large extent we can form our own conclusions on the evidence which he has given us.

Any more detailed comment on so closely reasoned a book as this can only be brief, and therefore unduly dogmatic. To one reader, however, neither the praise accorded to Barbon (p. 87), nor the inclusion of Vanderlint as a significant figure, seems to be quite justified. On the other hand, the reconstruction of Berkeley's thought from the welter of *The Querist*, the analysis of Cantillon's views (especially on the trade cycle), and the further rehabilitation of Steuart, are particularly well done. Vickers would put us further in his

debt if, looking back on his work with the advantage of a further four years' reflection, he could now develop more fully and systematically the synthesis to which his final chapter points the way.

J. K. HORSEFIELD

Washington, D.C.

Die Möglichkeiten der Schweizerischen Nationalbank zur Beeinflussung des Geld- und Kapitalmarktes. By DR. HEINZ LUTZ. Winterthur: P. G. Keller, 1958. Pp. 196.

Die Schweizer Banken im Dienst der staatlichen Konjunkturpolitik. By Dr. FRITZ MARKUS RUDOLF. Winterthur: P. G. Keller, 1958. Pp. 172.

The present monographs represent welcome and useful additions to the growing literature on central banking and monetary policy abroad. In combination with Amberg's *Die Politik der Schweizerischen Nationalbank* (Winterthur, 1954), discussed in the March 1956 issue of this *Review*, they provide a complete guided tour of the Swiss credit mechanism in general and the relations between the Swiss National Bank and the country's banks in particular. In a sense, the two works complement each other, since Lutz looks at Swiss monetary policy from the point of view of the National Bank and its role in the money and capital markets, while Rudolf analyzes the same policy as the banks have carried it out as agents of official economic policy. Along the way, the reader is given authoritative and detailed descriptions of the Swiss money and capital markets (Lutz, pp. 87-103), the history and evolution of the Swiss banking system (Rudolf, pp. 49-68), Swiss commercial banking legislation (*id.*, pp. 123-39), and Swiss mortgage financing (*id.*, pp. 140-45).

The broad picture that emerges from the two works is one that has been painted before, but to this reviewer's knowledge never with such clarity and precision. It is the picture of a highly developed and highly specialized banking system (comprising, at the end of 1958, 428 banks with a total of 3,833 offices) that, in the view of both authors, has been able to meet the credit needs of the domestic economy, while serving at the same time as an important source of capital to foreign countries. In these activities, the system has been guided by the Swiss National Bank in what both authors (as well as other observers) feel has been a highly successful and satisfactory manner, given the extremely limited statutory powers at the bank's disposal. These powers are discount operations, intervention in the gold and foreign-exchange markets, control over certain interest rates, and control over the credit institutions' foreign lending. Open-market operations, while available to the National Bank in a meaningful sense since 1954, have been given only limited scope and, moreover, could not be undertaken during the current phase of high-level economic activity because of the bank's slender portfolio of open-market paper. The existing commercial-bank liquidity requirements are designed exclusively for the protection of depositors.

The Swiss National Bank has made extensive use of moral suasion (which could be dubbed "monitory policy"), particularly through numerous gentlemen's agreements concluded with the banks as a whole or with specific groups

of credit institutions, and at times also with certain important financial intermediaries. Over the past thirty years, such agreements have covered at one time or another virtually every sector of the Swiss money and capital markets, their objects having included ceilings on mortgage loans, special cash reserve requirements, nonpayment of interest on foreign-owned deposits, and limitations on commercial-bank foreign exchange purchases. The crucial role of these agreements—which a leading Swiss banker early this year described as “a typically Swiss solution”—is recognized by both authors, who describe the background, characteristics, functioning, and results of each agreement in great detail (Lutz, pp. 164-76, and Rudolf, pp. 74-120). While stressing the advantages of these accords—such as the relative speed in concluding them and the possibility of applying them selectively—the authors recognize their disadvantages, in particular the lack of legal enforceability and the virtual impossibility of using such agreements to effect major shifts in monetary policy. The authors therefore conclude that gentlemen’s agreements (and moral suasion in general) cannot and should not be used as full-fledged substitutes for the more orthodox instruments, a view shared by the Swiss National Bank. Both authors consequently advocate at least a broadening of the National Bank’s open-market powers and the introduction of statutory variable reserve requirements. Rudolf, writing from the point of view of the banks, argues in addition for the introduction of deposit insurance and for the central registration of commercial bank loans.

Despite the clarity of exposition and wealth of detail of both monographs, however, the sophisticated reader will miss a unifying theme. Although Lutz introduces his study by reviewing the theory of central banking and Rudolf by expounding the role of the commercial banks in economic fluctuations, neither author stops to investigate the actual goals of the Swiss National Bank’s over-all policy. Such a discussion would have added immeasurably to both works by relating the instruments and policy measures described to the broader aims pursued by the National Bank within the framework of the Swiss economy.

JOHN HEIN

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Public Finance; Fiscal Policy

Ricardo on Taxation. By CARL S. SHOUP. New York: Columbia University Press, 1960. Pp. 285. \$6.00.

This is a thorough examination of Ricardo’s writings on taxation, primarily of the taxation chapters in the *Principles of Political Economy and Taxation*, but with full use of the pamphlets, speeches and correspondence. After two introductory chapters, the first dealing with the reception of his work on taxation by his contemporaries and later critics, the second giving an all too brief discussion of Ricardo’s “macroeconomic system,” Professor Shoup proceeds to a chapter-by-chapter analysis of Ricardo’s *Principles*. Each chapter of Shoup deals with a chapter of Ricardo, and with extensions of the reasoning of that chapter in his other writings. Thus Chapter 3, “Taxes Paid out of

Capital: Taxes Paid out of Income," examines Ricardo's Chapter 8 "On Taxes" in which this issue is discussed. Chapter 4, "Specific Tax per Unit of Agricultural Produce," examines Ricardo's Chapter 9, "Taxes on Raw Produce." And so it goes through taxes on land rent, on agricultural output (tithes), on land, on gold and houses (two chapters of Ricardo here to one in Shoup), on profits and on wages. Ricardo's Chapter 17, "Taxes on Other Commodities" is dealt with in Shoup's Chapter 11 as a discussion of "War Finance." Shoup's Chapter 12, "Taxes and Bounties on Foreign Trade," brings together material from several of Ricardo's chapters beginning with 22. His chapter on "Taxable Capacity" is chiefly concerned with Ricardo's Chapter 26 on "Gross and Net Revenue." There follows a chapter dealing with "miscellaneous" and unimportant tax issues.

An interesting chapter on "Ricardo and the British Tax System of His Day" emphasizes the feeling developed in the earlier chapters that Ricardo was very remote from reality. Shoup said at the beginning of his book: "In Adam Smith's long chapter on taxation the reader is treated to a rich slice of real life, against a background of somewhat undependable economic reasoning; with Ricardo we move on a high level of economic abstraction, supported by generally dependable reasoning, based however on dubious postulates of a most sweeping nature" (p. 14). A final chapter, "An Appraisal of Ricardian Analysis" recurs to this theme: "Given the economic world that he had created in his mind, Ricardo succeeds brilliantly in demonstrating the effects of taxation on the distribution of income among the three claimants" (p. 249). The trouble is that Shoup seems too fascinated by the intellectual problems of the analysis. "Puzzling out some of the Ricardian passages becomes almost the game of solving cryptograms" (p. 253). He is too little concerned with what Schumpeter called the "Ricardian Vice." His attitude is illustrated by his reference to Malthus: "Ricardo has a system-building mind; he is an intellectual architect of the first order. Malthus may have had more insight than Ricardo on particular economic problems, a more fruitful intuition, and a broader grasp of the social environment in which economic forces operate, but Malthus never took the trouble, or never was able, to link his observations into a closely articulated whole" (p. 252). But then of Ricardo he had said: "It must be confessed that for the most part the conclusions on taxation and capital are again inherent in Ricardo's system, and are correspondingly simplified."

As a student of the classical literature whose interest in it is not antiquarian but is in the contribution that the study of this literature can make to the better understanding of the world of today, I have found this analysis of Ricardo disappointing. Lest it were my lack of sympathy with Ricardo that led to this disappointment I reread the taxation chapters of the *Principles*. I found that they could have been examined for their value in suggesting problems and solutions rather than as providing a game of cryptograms. And I then wondered whether we do not now need to confess our inability to answer for the real world the questions that Ricardo answered for his simple model. The simple problems of particular equilibrium analysis we can handle fairly well; when it comes to the economy as a whole we are still pretty helpless.

Should not Shoup have considered more seriously the implication of his comment on the Ricardo-Smith-Buchanan controversy? "Thus Ricardo is able to agree completely with Smith and Buchanan when they lower their sights from grand generalizations about some widespread tax like one on raw produce to a common-sense analysis of a tax on a small item like malt" (p. 166).

V. W. BLADEN

University of Toronto

Federal Receipts and Expenditures during Business Cycles, 1879-1958. By JOHN M. FIRESTONE. Princeton: Princeton University Press, 1960. Pp. xvi, 176. \$4.00.

This book, which is one of the Studies in Business Cycles of the National Bureau of Economic Research, contains 75 pages of text and an appendix of 97 pages of description of the series and of tables. For the first time, a monthly record of federal revenue receipts and expenditures has been analyzed with respect to cyclical behavior 1879-1958. Cyclical patterns are traced: (1) 1879-1914, a period of peace; (2) the years of war and of transition from war—1914-21, 1938-54; (3) the interwar years, 1921-38; and (4) the years 1954-58. In (1), (3), and (4), "the cyclical behavior of the series was substantially similar," using as the test the method of analysis developed by Wesley C. Mitchell and Arthur F. Burns.

The beginning year, 1879, was selected because monthly figures were available only thereafter. As the appendix makes clear, the author has gone to great pains to make his figures homogeneous by taking account of changes in Treasury classification and accounting. Even so, the figures have limitations, most notably because they exclude receipts and expenditures in which the government acts as a trustee.

For peacetime cycles the author finds a remarkably persistent and countercyclical pattern of behavior of surpluses in prosperity and deficits in depression, with the influence of revenues the dominant factor. Expenditures typically rose during depression and recovery alike. In the period 1879-1914, the behavior was "wholly apart from any concept of modern fiscal policy"; it was automatic, growing out of changes in the size of the revenue base, rather than out of changes in tax rates. Changes in rates, however, did occur and no attempt is made to measure how much influence they had on receipts, because such a measurement would be very hypothetical. The author does indicate, in chronological sequence, the important changes in revenue laws.

Although the countercyclical behavior of federal surpluses and deficits 1879-1914 is marked, the variations were too small to have much effect, being only 2 per cent of the average change in GNP during expansions and 8 per cent during contractions. In the 1954-58 cycle the patterns were accentuated because of built-in expenditures, the dominance of income-tax receipts geared to earnings, and the willingness of government to accept deficits.

In war and transition periods (1914-21, 1938-54), the peacetime patterns are somewhat overlaid. In war, expenditure changes are overriding; in transition, the peacetime patterns re-emerge, but this is attributable chiefly to the decline in expenditures.

While the careful analysis by Firestone provides few surprises, it does refute exaggerations which have crept into economic literature, and it quantifies, to some extent, the cyclical effects of the federal budget. Budgetary balance has consistently been upset by the business cycle, and the upsets have been countercyclical in their effects. Now that federal receipts and expenditures have become a much more important component of GNP, that the amplitude of swings in receipts has grown, and that both Congress and the Executive appreciate the fiscal techniques of economic stabilization, the likelihood of severe depressions has diminished.

JAMES A. MAXWELL

Clark University

Postwar Market for State and Local Government Securities. By ROLAND I. ROBINSON. Princeton: Princeton University Press, for the National Bureau of Economic Research, 1960. Pp. xxiv, 227. \$5.00.

Treasury-Federal Reserve Study of the Government Securities Market. Part I, July 27, 1959, pp. vii, 108. Part II, February 1, 1960, pp. vii, 159. Part III, February 1, 1960, pp. vii, 112. Washington, D.C.: Secretary of the Treasury and the Board of Governors of Federal Reserve System.

Although billed as a dispassionate description and analysis of the operation of one sector of the capital market, Robinson's book is essentially a pitch for the removal of the tax-exemption privilege from the interest income of state and local government securities. His thesis is that taxation of interest income by the federal government will yield so much revenue that states and local governments could be offered a subsidy greater than that obtainable from tax-exemption of their securities and still net the U.S. Treasury an increase in revenue.

This is the latest definitive study of tax-exempt securities by an economist. Its thorough treatment of the subject places it in the same category with works by Charles O. Hardy for Brookings (1926), Harley L. Lutz (1939), Lucile Derrick (1946), Lyle C. Fitch (1950), and George E. Lent (1955).

An analysis of the demand for funds by state and local governments is followed by a description of who buys their bonds, how much, and why. The analysis of the supply of funds by investors is the more difficult task, and the author does not here acquit himself as well. Observations such as "State and local government obligations are the second choice of many investors, the first choice of very few" (p. 99) are entraneous. Robinson's descriptions of the marketing of new issues of state and local securities and their secondary market are succinct, colorful, and penetrating. It is here that his interviews with people close to the market have paid off: statistics alone could not have produced such clear exposition.

The work reaches its culmination when the author seeks to divide (mathematically) the "tax-exemption subsidy" between reduced borrowing costs and investors' surplus. First he determines what the income tax liability would have been if, instead of tax-exempts, banks and casualty insurance companies had purchased corporation bonds and individuals had purchased either corporate bonds, common stocks, or half of each. Second, the true differential be-

tween taxable and tax-exempt bonds is assumed to be the difference between Moody's Corporate Bond Yield Index, based on 30 bonds, and Moody's Municipal Bond Yield Index, based on 5 bonds, for each of the 4 top ratings, Aaa, Aa, A, and Baa. For lower-rated and unrated issues the author devises his own differential. In view of his explanations of the legal pitfalls surrounding municipal securities (p. 50), his insistence that the true differential can be no greater than the difference between the two indexes is incongruous. Third, his differentials are multiplied by the amounts of bonds sold each year in each category to determine the interest-cost reduction.

The interest-cost reduction each year is divided by the federal tax loss, i.e., the income tax liability if taxable rather than tax-exempt securities had been purchased, to determine the percentage of such loss retained by state and local governments. Table 29 shows the percentage dropping from 67 per cent in 1947 to 20 per cent in 1953 and increasing thereafter. Although figures for 1945 and 1946 are given in the appendix, the computation of the division of the "tax-exemption subsidy" was not carried back beyond 1947 when a net disinvestment in tax-exempts by individuals and casualty insurance companies resulted in an investors' deficit rather than surplus. Basically, the percentage of federal tax loss retained by state and local governments as reduced borrowing costs is a function of the differential.

In the foreword, Raymond W. Goldsmith says, "since [1956] the devaluation of the tax-exemption privilege has proceeded further." Since state and local governments retained only 20 per cent of the value of tax-exemption in 1953, according to Robinson, it would follow that they retain a much smaller percentage today; perhaps the tax-exemption privilege has lost its value to the issuing authorities. Robinson's prose overstated the case that his arithmetic merely illustrated.

For a book about the securities industry, where interest rates are often figured to a half-dozen decimals, the arithmetic calculations are not exemplary. There are too many tables where the numbers do not add up. References are handled carelessly: a consistent pattern of citing by exact titles, authors, and publishers is lacking; some works are cited without authors or publishers, making tracing to the original source impossible. The book is well written and informative, but its approach is not scholarly.

From its title the second book apparently complements the first. While Robinson's study is broad and covers a ten-year period, the government report is focused entirely on federal financing during 1958. One of the most elusive facts about the study is its authorship. The reports keep referring to the "committee" that listened to the remarks of the consultants and synthesized their discussions into a cohesive report. The "committee" seemingly consisted of economists and statisticians. If there was a political scientist or a sociologist among them, his influence never seeped through to the printed word.

Part I of the study is concerned solely with opinions of 119 consultants from the financial world on the functioning of the market for government securities, the causes of the extreme cyclical variation in prices of government securities in 1958, and the question of whether an "exchange" in government se-

curities should be organized. Suggestions for stimulating an active market in governments included the re-introduction of the tax-exemption feature (pp. 46, 80, 93), which now applies to securities of states and local governments.

Part II of the study is a statistical exposition of the situation in the government securities market, but basically it is a defense of the money managers for the slovenly market of June 1958. The Treasury-Federal Reserve group scrutinized the effect of countercyclical fiscal and monetary policies on government interest rates and cautioned against the possibilities for speculation during the recovery phase of the business cycle when governmental inflationary policies have not been completely halted.

Part III consists of working documents for Treasury and Federal Reserve officials. Citations to sources are nonexistent or given in an offhand manner. Four related subjects are explored: the adequacy of market statistics, margin requirements, the repurchase agreement, and the need for an association of dealers in government securities.

The two books are timely and have value for those interested in doing intensive research in public securities.

HENRY J. FRANK

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Public Finance in Theory and Practice. By A. R. PREST. Chicago: Quadrangle Books; London: Weidenfeld & Nicholson, Ltd., 1960. Pp. 408. \$7.50.

Taxation and civilization are correlative. At this stage of civilization books concerning taxation and public finance abound. Since there is no dearth of literature in the field all new works must be evaluated to determine their specific contribution. From Christ's College, Cambridge, England, A. R. Prest presents this analysis of theoretical public finance and a most welcome explanation of the taxing process of the United Kingdom. The book is written in a comprehensible style and is indeed a contribution to the further understanding of this increasingly complex subject.

Any American reader will find the comparative analysis of U.K. and U.S. taxing systems most enlightening. It includes tabulations showing the relative tax positions of families under the income tax laws of the respective countries. Noteworthy is the discussion of capital gains taxation which includes theoretical consideration of the issue and provides as well some analogies between U.K. and U.S. treatment of the problem. Prest states (p. 289): "Indeed, it could be argued that in some respects capital gains are treated more harshly in this country than in the U.S., where there is an explicit tax on them." This comment may come as a surprise to many who believe that capital gains escape taxation in the United Kingdom. With reference to capital gains taxation in the United States Prest says (p. 300): "... there seems no doubt whatever that the taxation of capital gains is the subject of more acrimony and more time-consumption on the part of officials and taxpayers than any other aspect of the U.S. Federal income tax."

Other discussions of particular interest to readers in this country include those of national debt, the national insurance funds, expense allowances, dis-

tinctions between earned and unearned income, and taxation of overseas incomes. After a brief general introduction, the book is subdivided into three sections covering analysis, description and policy. A short, selective bibliography is presented at the end of each of the three subdivisions. The bibliographies are adequate but they are not comprehensive.

This reviewer would take exception to the statement found on page 16: "We have discussed the historical origins and growth of the main present-day constituents of the subject-matter of public finance." The background discussion began on page 13 with reference to Adam Smith, Ricardo, and Mill. This can hardly be called a full discussion of historical origins. Similarly, at page 24, reference is made to the long history of direct and indirect taxation. The only historical reference is again J. S. Mill, with no comment about the Physiocrats.

On the positive side one must praise the author for a relatively concise treatment of subject; 408 pages including bibliography and index for a comprehensive and informative volume is an achievement. This volume is a creditable addition to the extensive list of articles and publications of A. R. Prest. At this particular time in history a more universal view of ideas and procedures is essential. The U.S. reader has much to gain by delving into this work on public finance. For text use, for supplementary reading, or for reference material, the book under review can be used with profit.

JANET K. MESSING

Hunter College

Gosudarstvennyi biudzhët SSSR. (The State Budget of the USSR.) By K. N. PLOTNIKOV. Moscow: Gosfinizdat, 1959. Pp. 422. Rbl. 9.20.

This is a supplementary text for use in institutes and university departments training students for careers in financial administration. Its 32 chapters, in 6 parts, cover: (I) the Soviet state budget's function in the economy, its internal structure, and its relation to the constituent budgets of Republics and regions; (II) taxes and other revenues; (III) income and outlay relations between the state budget and industry, agriculture, trade, and municipal services, on both current and capital account; (IV) outlays for social security, education, and national defense; (V) budget planning and review; and (VI) financial supervision, inspection, and control procedures.

The author, Professor Kirill Nikonorovich Plotnikov, has been a major writer in the field for over twenty years. His career was crowned recently by his appointment as director of the USSR Academy of Sciences Institute of Economics. In the fall of 1959 he was one of a delegation of outstanding Soviet economists which visited the United States under the US-USSR cultural exchange agreement.

Unlike the author's 1948 and 1955 treatises, *Biudzhët sotsialisticheskogo gosudarstva* (The Budget of the Socialist State) and *Ocherki istorii biudzheta sovet'skogo gosudarstva* (Essays on the History of the Soviet State Budget), this book contains few figures. Its emphasis is on the procedures employed in making up and carrying out annual state budgets under post-1957 institutional arrangements. Plotnikov notes in his preface that the history and evolution of

Soviet budgets are not treated, "in order that students' attention should not be deflected from the main questions of budget work," and that the data used in examples and calculations are hypothetical. Taken by itself, therefore, this descriptive survey is mainly valuable for its discussion of questions concerning proper functioning of the present day budget system. Taken together with the author's earlier work, plus the figures for recent years available from statistical handbooks and technical journals, this volume supplies an informed and judicious commentary that rounds out a comprehensive analysis of the budget's role in Soviet economic life.

HOLLAND HUNTER

Haverford College

International Economics

Europe's Coal and Steel Community: An Experiment in Economic Union. By LOUIS LISTER. New York: Twentieth Century Fund, 1960. Pp. 495. \$8.00.

An unusual experiment in the social control of business has been under way since 1952 in Western Europe. At that time a group of contiguous countries agreed to place the heart of their industrial life under the direct regulation of a common supranational organization.

Many individual countries have undertaken to regulate domestic industry, and occasionally a group of nations have entered into treaty arrangements to cooperate in the regulation of industries operating in more than one country or on the high seas. But such treaties have generally relied upon national governments to give effect to the agreed regulatory principles, and at most the industries in question have been of peripheral concern to the participating countries. Coal and steel, however, are the foundation of Western Europe's industrial life, and under the European Coal and Steel Community these industries of the six member countries are subject to direct regulation by the administrative structure created by the treaty setting up the Community.

There is another respect, too, in which the ECSC is unique. Countries before have formed customs unions, but it is unusual for a group of countries to merge just a part of their national economies. The supranational character and partial approach to integration of the ECSC experiment hold a fascination for students of international economic affairs and government-business relations.

Americans, particularly, can compare the arrangement and experience of the Community with efforts of the thirteen original American states to cooperate from 1781-1789 under the Articles of Confederation and the subsequent strengthening of powers of the central authority under the federal Constitution. The Coal and Steel Community has gone beyond the articles of Confederation in making the supranational agency financially independent of the member governments (permitting it to tax the industry directly), in providing for direct relations between the supranational agency and enterprises in the industries concerned (permitting the agency to levy fines and providing the enterprises recourse to supranational judicial review), in denying to mem-

ber governments powers to regulate trade in coal and steel between the states, in requiring concerted policies toward trade with outside nations, and also in relaxing the rule of unanimity. However, it has fallen well short of creating a federal power. Thus the ECSC stands midway between the Articles of Confederation and the federal Constitution; it was designed to secure some of the benefits of economic union without incurring the full subordination of economic life of member states which has been the result of the U.S. federal union.

How has the more limited union worked out? Are gains from it discernible? Have the objectives of union been frustrated by conflicting national interests left outside the common undertaking? To what extent will such conflicts be eliminated by the creation of the broader European Common Market? On these questions Lister's analysis throws considerable light. His own assessment is that "the need is for more, not less, integration."

The prime economic objective of the Community, of course, was to secure the advantages of specialization and trade in coal and steel. Greater productivity, lower costs, lower prices, greater output, higher wages—in short, economic efficiency and progress—were the major economic goals, subject however to a parallel concern that high employment should be maintained and that disturbances in economies of member countries should be minimized.

In a common market with competitive conditions equalized, competition was to provide the driving force for realizing the main objectives. But this move in the direction of economic liberalism was taken by six countries each with a long history of accepting private restrictions as the natural and inevitable end of the exercise of competitive freedom, each with strong private organizations of business associations in those parts of the coal and steel industries which were not nationalized several with a high degree of public ownership and management of parts of the industries, and most with governments actively influencing the private conduct of business in these fields. How far has the move to "let a little of the air of competition" into Western Europe's economy succeeded? Are resources coming to be used more efficiently? What are the evidences for economic progress? The author marshals the evidence one needs to answer these questions.

After an initial chapter on background and objectives, he takes us on tour of the Community explaining where production of coal and steel takes place, where the markets are within the Community, and where the industries draw upon outside sources and supply outside markets.

After these two informative introductory chapters, Lister considers costs of production and investments. He exhibits great industry and considerable ingenuity in developing evidence on the average real and money costs of finished steel in the main steel-producing countries of the Community as compared with the United Kingdom and the United States. His interpretation of the available data, which brings in locational factors, consideration of the significance of jointly supplied products, governmental influence on prices of raw materials in different countries, differential productivity, hourly payroll costs, and labor costs per ton, is both cautionary and instructive. Of considerable interest are his findings that average labor and material costs vary little among European producers (Italy excepted) and that lower U.S. costs of raw materials offset

higher U.S. labor costs so that taken together they are not greatly different from, for example, German costs. However, from the higher domestic U.S. price for merchant bars and higher costs per ton for new plant in the U.S. which the author gives, it would appear that American capital costs per ton are higher. The author does not give us a complete resolution of final sales value of steel into all its cost components including overheads nor is he able to trace all cost components over time, but the information he has assembled is employed skillfully in exposing important differences between national industries.

To the discussion of costs Lister links an analysis of the record of expansion and the role of the High Authority in contributing to the development of the industries along lines of comparative efficiency. By pointing up the relationship between national governmental price fixing, the availability of internal business savings, and the extent of self-financing of new investment, he contrasts the limited powers of the supranational authority and the importance of powers retained by the individual states over their national industries.

This theme is reinforced by his conclusions on costs and investments of the coal industry, in which national price policies and investment programs are found to be of greater significance than the common market arrangements in shaping the direction and pace of investment. Loans by the High Authority have been a minor source of financing; and its special subsidies, financed by a levy on low-cost producers in the Ruhr and Netherlands, have served mainly to keep alive the high-cost mines of south Belgium rather than to aid consolidations or shutdowns required to increase the competitive position of that region. (However, more recent events—notably the new readaptation program approved by the Council of Ministers in December 1959—hold out the prospect that this function of the agency may yet prove to be more effective.)

The continued prominence of national governmental policy is further stressed in the author's exposition of the industries' organization in chapters on concentration, concerted practices, prices, and competition. These chapters, and the related appendixes on the concentration movement in the French steel industry and the reconcentration movement in German coal and steel since the second world war, will be read with interest by those who have been looking for evidence on the metamorphosis of European market structures under the postwar European "new look" at the problem of private monopolies and cartels. In seeking to describe the ownership and control relationships in the two industries, Lister finds it meaningful to work with "groups" of firms spanning both coal and steel industries and having common interests through interlocking directorates or officers, equity participations, or long-term contracts. Beyond spotting and delimiting such groups, he finds numerous links between them, so that one should think of an industrial structure which hangs together. Groupings are interrelated rather than independent.

Lister marks an increase in concentration of control, particularly in the steel industry (coal having been nationalized outside the Ruhr), with the advent of the new Community and attributes it to the increased uncertainty resulting from the wider market area and the desire of firms to strengthen their position. He gives evidence on the extent of concentration of the Ger-

many steel industry in 1959 compared with the situation in the 1950's and the late 1920's and finds the structure, while not so heavily dominated by a single firm, still oligopolistic. The advent of the Community has not resulted in any great new interpenetration of capital across national frontiers. The substantial German, French, and Belgian holdings across national lines developed before the second world war. The Community contrasts with the U.S. market in that no firm or group controls plants in nearly every part of the union as U.S. Steel does here. Thus, while the national steel industries are oligopolistic and organized in national associations, they do not enjoy the unifying principle of a dominant firm for the whole market, nor is there yet any indication that such a dominant firm is emerging.

Competition in coal is importantly conditioned by nationalization of coal mining and importing, notably in France, and by governmentally sanctioned private sales syndicates, notably in Belgium and the Ruhr. The private sales syndicates controlling supply to southern Germany, Lister is able to report, have been reconstructed to enlarge the range of alternatives to wholesalers and larger buyers.

Lister finds little prospect for local competition among coal producers; more for interregional competition where regions may draw upon different sources; but mostly he looks to competition from U.S. coal and from oil as providing the main pressure on coal prices in the Community. The measures governments in the Community have followed to protect their domestic coal mining industries against the disruptive swings in the landed cost of U.S. coal (fluctuating with freights rates) and pressure of increasing supplies of cheap petroleum come in for discussion in a chapter on the energy market.

Costs of transportation affect competition in coal and petroleum marketing, and the chapter on transport rate problems of the Community illuminates many of the problems of competition discussed earlier. The author evidently reserved the discussion of transportation until late in the book because of the fine illustration it provides for his central theme that the Community is too limited in scope and reserves too much power to member nations. This theme is also reinforced by the consideration in later chapters given to external commercial policy and trade and to social policies.

Lister has given us a workmanlike book that reflects a deep understanding of the industries in question and the forces pulling in different directions on this infant Community. The volume greatly illumines the realities of European integration and will be appreciated by all who seek to understand Europe's experience to date with its new institutions.

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Preispolitik im teilintegrierten Markt. By GOTTFRIED ERB and PETER ROGGE. Veröffentlichungen der List Gesellschaft No. 9. Tübingen: J. C. B. Mohr (Paul Siebeck), 1958. Pp. xii; 228. DM 17; paper, DM 13.50.

In its most recent annual report—for 1959—the High Authority of the European Coal and Steel Community complains that the arrangement “for

coordinating the work of the High Authority with the general economic policies of the Governments has proved inadequate to resolve some of the practical difficulties encountered, more particularly in the field of price policy." The complaint is not new; the High Authority has been worried about this problem for a number of years. What has been happening, specifically, is that the governments of the countries in the Community, especially France and Germany, have been exercising substantial and often decisive influence on coal and steel prices even though they gave up the right to fix those prices when they signed the Schuman Plan treaty.

The source of the difficulty is partial integration which makes a conflict of this sort almost inevitable. The coal and steel industries of the six countries form part of an integrated market under the treaty but they are also parts of national economies that are not as closely linked with one another as the coal and steel industries are supposed to be. The High Authority looks at the matter from one point of view, the governments from another. Without reneging on their treaty commitments, the governments not only have to take account of more immediate pressures than the High Authority but want to fit coal and steel prices into their broader economic policies. As a result the Community's price structure is not what one would expect it to be if only the High Authority's policies were effective. The discrepancies are also made possible by the limited degree of interpenetration of national markets that exists in spite of the removal of trade barriers and by a whole range of factors that limit flexibility in coal and steel prices. High demand sharpens the discrepancies; slack markets make for higher interpenetration and a certain leveling of prices.

Partial integration has been much discussed in the light of recent European experience but the subject lacks a good analytical literature. The main virtue of this highly competent book by Erb and Rogge—two members of the List Gesellschaft's staff—is that it thoroughly and carefully surveys and documents one part of the problem. The book is principally an account of the price policies of the High Authority and of the national governments during the transitional period that ended in February 1958. The actual movement of prices is also examined and the book contains 40 pages of useful tables and charts. The authors miss nothing of importance; their systematic exposition clarifies many of the issues. There are good and interesting analyses of a number of points. Erb and Rogge are realistic about the limited applicability of simple models to the price experience of the industries they are examining. They call attention to the factors outside their prescribed subject which influence the price patterns.

Within the limits they have set themselves Erb and Rogge have done a good job and drawn modest conclusions. The subject warrants going beyond these limits. The problems of partial integration involve more than the differences in national and international policies. What we need now is some close study of the direct effects on an industry of being imbedded in a national economy while becoming in some degree integrated with the industries of other countries; and also of the economic consequences of this kind of partial integration

as contrasted with what we think we know about complete integration on the one hand and traditional trade liberalization on the other. Is there, indeed, a separate subject?

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L'Europe unie, route de la prospérité. By MAURICE ALLAIS. Paris: Callmann-Lévy, 1960. Pp. 373. 22 NF.

This entry into the torrent of books on European integration by the distinguished French theorist and econometrician starts out as an analytical study but fairly quickly shifts to pamphleteering. Before he is done Professor Allais has made proposals for ending the war in Algiers, for German unification, for nuclear disarmament, for challenging the Soviet Union to quinquennial comparisons of real national product per capital *à la* Gilbert and Kravis (an "Open Cupboards" proposal?), along with a take-it-or-leave-it scheme for integrating Commonwealth preference into the European Economic Community. Winner of the "Grand Prize of the Atlantic Community," the book presents a powerful, even passionate, plea for the Common Market and ultimately an Atlantic Economic Community, based on liberal principles of free enterprise and free markets. It is addressed, however, to a broad public and not to the economist.

Allais' enthusiasm is not contagious in this quarter. One can agree with many of his conclusions, whether for freer trade, or against agricultural restriction, against integration through planning, or the uneconomic behavior of the European Coal and Steel Community. One can be thoroughly for the Six and for allocation through the market process. But he writes as a true believer, rather than as a scientist, even a partisan scientist. Much of his case rests on the general argument for free trade, rather than for a discriminatory reduction of tariffs. The defense of the Six seems to rest on the need for harmonization in the political sphere, but this receives only two pages (pp. 133-34). And his treatment of trade diversion (pp. 210-212) consists largely of dismissing it out of hand.

Much the most interesting portion of the book for economists will be the opening chapters which seek to explain the difference in real income per head between France and the United States. This is calculated at 2.34 times in 1957 (Gilbert and associates, whose work Allais cites elsewhere but not in this connection, give 2.4 for 1955 at United States weights but 1.8 at European weights). It is correctly asserted that this difference may be due to differences in natural resources, capital, labor, scale (yielding either decreasing costs or more intense competition), or to noneconomic factors, such as social mobility and love of output (what the Germans call "*Produktionswut*"). But I believe that the conclusion ascribing major significance to the competitive effect rests on shaky foundations.

Resources and capital are eliminated by *a priori* reasoning. Resource industries contribute only 25 per cent of French income. If resource products were available free of charge, French national income could be increased only

33 per cent (25/75). Therefore the importance of resources is limited. But this is reasoning by means of the labor theory of value. Allais uses a labor-theory-of-value explanation of the gains from trade and excuses it in a footnote (p. 102). But it is inexcusable here. If France were to receive its resource products free, there is no way of knowing, without more information, how much income could be increased. It might be zero per cent, if the resources engaged in the industry were entirely specific. It might be 500 per cent if the resources engaged in the industry were working at sharply diminishing returns and could be put to work at constant or increasing returns outside. It is significant that the resource industries of France (p. 25) produce only 20 per cent of income and employ 29 per cent of the active working force (these figures were averaged to reach 25 per cent above). Further, Charbonnages de France and Electricité de France have been prodigal in their use of capital.

For capital, the Allais argument runs that the capital/output ratio is the same for the two countries. (This requires overlooking some French estimates, but this does not upset me: I am prepared to agree that the capital/output ratio is the same all over the world.) Therefore capital does not count. But of course it is the capital/labor ratios which should have been compared. With linear homogeneous production functions and constant capital/output ratios, differences in output per capita are to be explained entirely by differences in capital/labor ratios. Sauvy has somewhere estimated the French capital/labor ratio at 1,000,000 old francs (\$2,000) per man. I don't know what the United States figure may be, but it is closer to \$10,000. Here is room for some considerable portion of the difference, but Allais ascribes to it none.

Nor in his estimation does quality of labor count. He recognizes that workers and staff in the United States have on the average more education than their counterparts in France, but a man is a man, and that is that. (Compare Leontief who claims that one U.S. worker is equal to 3 foreign.)

Finally on scale, Allais asserts that the Verdoorn effect of increasing efficiency with scale is really a result of competition. American plants are bigger because competition made them more efficient, rather than more efficient because bigger. Here is the familiar identification problem.

Allais may be right: the competition of the European Economic Community may double French real income per head, as he thinks. I doubt that it will do so without raising capital per unit of labor or increasing scale. And if it does, as I too hope, it would seem to reflect more honor on economic intuition than on econometric calculation.

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The Tendency Towards Regionalization in International Trade, 1928-1956.
By ERIK THORBECKE. The Hague: Martinus Nijhoff, 1960. Pp. xiii, 223.
f 25.00.

Professor Thorbecke's study appears at a time when economic regionalism is much in the news. While it appears that economic blocs are here to stay, their composition and the nature of the regional arrangements are changing. The non-Soviet world finds itself in a period of bloc formation and regrouping

whose eventual outcome cannot yet be foreseen. The division of the trading world into the three regions considered by Thorbecke, the continental OEEC, the sterling area, and the dollar area, owes its existence mainly to international payments problems, although intrasterling trade relations had also been fortified by the empire preference system. In the absence of new forces of regional cohesion one should have expected the return to convertibility and the gradual reduction of discrimination for balance-of-payments reasons to lead to a decline in the regionalization of trade. However, new bloc configurations have emerged, or are in the process of formation, based on considerations of market size and resource allocation, and perhaps also on political elements. Continental Europe is split between the Six and the Seven, a new Central American common market and a South American free trade area promise a similar division of Latin America, while discussions of an Asian bloc, an African free trade area, and some kind of North-Atlantic arrangement, leave the future of the sterling area in doubt. Thorbecke's study of the effects on trade flows of the regional arrangements of the 1930's and the first decade following the second world war can thus not be taken as the description of a process which is still continuing, though it is, of course, of historical, theoretical, and methodological interest.

In Part I, the author describes the changes in the system of multilateral trade which have occurred between the interwar period (1928 and 1938) and the decade of the 1950's (1953 and 1956). He divides the non-Soviet world into 10 regions, of which 8 can be fitted, in the manner of Folke Hilgerdt's League of Nations study, into a pattern of offsetting regional trade balances. Thorbecke tries to arrange a circular configuration in which each region has merchandise surpluses with those regions which are closer to it in the clockwise direction and deficits with those which are closer to it in the counter-clockwise direction. Such a representation is instructive, but the author makes too much of it when he implies (p. 91) that a functioning world trade system depends on the establishment of such a neatly symmetrical pattern of trade balances. In the first place, trade can be optimally multilateral without a "great circle" arrangement of the trade balances. Secondly, service balances and regular long-term capital flows are an integral part of the system. As Condliffe points out in his foreword to the book, what may be described as multilateral or bilateral is really the payments system rather than trade as such.

In Part II, Thorbecke shows that in the three regions mentioned earlier the ratio of intraregional trade to total trade has increased between the 1930's and the 1950's (selected years 1928-1956). For all three regions taken together this ratio rose from one-third in 1928 to about 45 per cent in 1954. In 1955 this indicator of the degree of regionalization declined sharply for the sterling area, and in 1956 also for the dollar bloc and the continental OEEC. Since the computations do not go beyond 1956 it is impossible to say whether the return of convertibility has brought a reversal in the trend as far as these three regions are concerned or whether 1956 was merely an atypical year.

After a discussion of the changes in the intraregional trade balances, the author analyzes for each of the three regions the determinants of the apparent

tendency toward regionalization under the headings of geographical, political, economic, and monetary causes. The first category is rather curious, since the geography has, of course, not changed. Monetary causes predominate, but are supplemented by "economic" ones, i.e., by changes in comparative advantage.

A wealth of quantitative information is supplied in 61 tables as well as in the text, where fewer facts and figures and a shorter exposition might have made for easier reading. The book is, nevertheless, a valuable empirical contribution towards the development of the economics of blocs, which appears to be a field of rising importance.

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*The opinions expressed in this review are not necessarily those of the International Monetary Fund.

Foreign Aid Theory and Practice in Southern Asia. By CHARLES WOLF, JR. Princeton: Princeton University Press, 1960. Pp. xix, 442. \$7.50.

This is a very informative and interesting book. It is divided into four parts. The first two deal with U.S. aid in Southern Asia and are mostly factual. Parts III and IV are concerned with the analysis and allocation of foreign aid. The central analytical question which the study raises is how to improve the allocation of foreign aid in a region, once the total amount to be allocated has been determined.

The book starts by discussing the first years of U.S. economic aid to China, Korea and the Philippines. Next the effects of the emerging cold war on foreign aid are considered. The Truman doctrine, proclaimed in 1947, confirmed the use of military and economic aid as key instruments of U.S. policy in the context of the cold war. Economic development was not conceived as a goal for any of the early Asian programs. The author then discusses the gradual development of U.S. aid in Southern Asia for the years 1951/57 and deals in detail with the aid allocation to various countries, explaining the principles of policy and describing the various agencies which were responsible for U.S. aid.

The aid was divided in subsequent years into military aid and defense support, and economic aid. It was only in later years that the requirements of economic development began to assume greater importance as can be seen from the figures of U.S. aid to Southern Asia during 1951/57. Total economic and technical aid obligations were \$1,376 million as compared with \$1,740 million of funds obligated for U.S. military (defense) support in the area. Furthermore, appropriations for U.S. military end-item assistance to Asia and the Pacific amounted to \$4,405 million. With the start of the Korean war, building up military forces became a highly valued objective of U.S. foreign policy in Asia as well as in Europe.

Reviewing the yearly allocations of the foreign aid, the author refers to a possibility of lowering "the rate of exchange" as between economic and military assistance in 1955 when no major local war existed. About the same time, growing emphasis was given to Asia and the objective of political stability

ranked high. Congress continued, however, to assign the lesser value to the U.S. aid of stimulating development in "neutralist" (India) as contrasted with friendly nations.

While the reader obtains a mass of information on year-to-year decisions, not much is said on the effect of U.S. aid on the economy of recipient countries or on the achievements of the aid objectives. It is not practicable to deal here in detail with the very interesting discussion of the objectives of the U.S. aid. It should not be difficult to state the objectives of military aid. One interesting point is made by the author. Although strengthening of internal security forces is one of the most important objectives of military aid, it cannot be evaluated in military terms.

Dealing with the objectives of the economic aid, the author rightly discounts the importance of the two frequently mentioned objectives, namely extending the gains from trade and increasing supplies of strategic materials available to the United States. The important objectives of economic aid are to increase political stability, to reduce political vulnerability by making possible a higher rate of economic growth (development) in the recipient countries. The hypothesis of political vulnerability suggests a relationship between economic variables and political behavior, but as the author warns, both concepts are extremely vague. It would be helpful to know whether and how political stability is related to measurable indicators which can be affected by economic aid.

The author has worked out a model for measuring the danger of penetration (vulnerability). Under the heading "Economic Change and Political Behavior" Wolf presents a model relating the notion of political vulnerability to the three socio-economic variables: economic aspirations, standard of living, and economic expectations. Each of the components is defined in terms of measurable indicators. A rudimentary test of the model's predictive power was made using Indian data based on the 1951-52 elections. The author concludes that vulnerability indices derived from the model are of value in explaining the regional distribution of communist voting and that the test results tended to support the basic hypothesis.

It is very surprising to find that the index of political vulnerability of East India which includes West Bengal (Calcutta) was much lower than that of Central Indian and not very different from the all-India index. It would be interesting to compare this index with one based on the 1956 Indian elections at a time when more dependable data were available.

With regard to the economic aid allocation, optimal allocation in a region requires, according to the author, country allocations proportional to the product of the vulnerability parameter based on the model and the productivity parameter. The objectives of the aid would call for maximizing the political gains which can be obtained from it. A higher policy "value" would be accorded to a "large" than to a "small" country and also to a country with a "high" than a "low" initial vulnerability level in the case of countries of equal population.

One could go on examining what other criteria should be used for allocating

U.S. aid in accordance with U.S. foreign policy. The author is well aware and points out that the allocation "results" yielded by the models would have to be adjusted in accordance with judgment for which models are no substitute. This is more true of the military aid allocation. The author makes an interesting point concerning substitutability of military aid; it may be sometimes advantageous to substitute a seemingly higher-cost mobile U.S. tactical capability for apparently cheaper labor internal forces (local capability).

Wolf is on shaky ground dealing with the allocation of aid between economic and military programs, which involves the comparison of incommensurables. One can agree with him that judgment should be focused on the desirable balance between economic and military aid. Any such judgment would hardly be based on technical grounds, on an imputed "rate of exchange" between the returns from these two forms of aid. This reviewer believes that only case studies could supply a good basis for providing an answer as to whether in some countries concentration on economic aid would not have strengthened political stability more than large military aid.

The question might be asked whether the author has successfully carried out his assignment to work out an analytical approach for the improved allocation of the U.S. foreign aid. Wolf's model which might be used to support judgment or decisions on foreign aid is rather original and interesting. It would have been helpful if the author had prepared real case studies using his analytical approach to the allocation of U.S. aid in Southern Asia.

Throughout the book U.S. aid is regarded as an instrument of U.S. foreign policy. The scene is now changing and the main emphasis is being placed on promoting economic growth, on improving the level of living. The growing volume of the Soviet bloc economic assistance must also be taken into account. The author mentions on the last pages of the book that instruments available to U.S. foreign policy for meeting the "peaceful" dimension of the coexistence challenge are limited.

Several factors must be considered. Other developed nations of the Free World are requested to provide economic aid. Such aid is being extended for example to India to help in financing the second five-year plan. The recipient countries might have a penetration or vulnerability index different from that suggested in this book, and they will not yield their right to decide where and how to develop their economy. The author, aware of the new developments, points out that the question of what is the appropriate commitment of U.S. national resources to foreign aid is the one which will become more important than the problems discussed in the book.

This development should not take away any merit from Wolf's inspiring study. Quoting the author, "It will be a better answer—if intuition is informed by careful consideration of peaceful coexistence" and the book should be a good source for supplying information and guidance. However, so long as foreign aid remains basically an instrument of foreign policy, the role of economic theory and analysis in deciding aid allocation must remain limited.

ANTONIN BASCH

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The End of Empire. By JOHN STRACHEY. New York: Random House, 1959. Pp. 351. \$5.00.

The first line of this book declares that its primary purpose is to consider Britain's relationship to the world now that her empire is being dissolved. Fortunately this concern for Britain does not interfere with a more general treatment for those of us not fortunate enough to be Britishers. Much restrained since his *Coming Struggle for Power*, Strachey examines the general problem of modern imperialism with bold vivid strokes in a book which is worthwhile reading for anyone interested in the problem of imperialism.

First there is a section on how an empire is built, taking British India as a case study. While he never lets the reader forget he is a British apologist for the work of his national and personal ancestors, the story is told objectively and always with an eye on the modern world. Blending daring, corruption and violence, a lowly company of merchant "Ambassadors of Complaint" wrested control of Bengal almost by accident from an incompetent satrap of the decaying Moguls and gradually developed an ideal colonial administration with the wits to get out gracefully when the time came. What were the consequences for India and for Britain? For India, initially famine, plunder and devastation; later, Vera Anstey's "arrested development." Marx (quoted from *New York Tribune* articles) pinpointed the British role as both destructive and regenerative; but according to Strachey, the real criticism was that the British did not "effectively break up the stagnation of Asiatic society." Law, order, railways and health measures without rapid industrialization resulted in actually worsening economic conditions. For England, the "drain of unrequited value" extorted from India helped the former to industrialize at the critical time. Now it is India's turn to get some of that unrequited value back. For Britain and the West, assisting India's take-off is repaying a debt.

After describing the new Imperialism of the 1870's, Strachey sets forth the Hobson-Lenin explanation of imperialism which holds that investment of surplus capital abroad is a necessity for a major capitalist country and becomes even more so as concentration of industry proceeds. The trouble with this theory, says Strachey, is that the countervailing power of labor and "farmer cartels" was underestimated by these early analysts. With this kind of bargaining power, the masses got more of the share of the product than was anticipated. He quotes (p. 110) a passage from Lenin, which indeed acknowledges that with a rising standard of living in the developed countries, the drive of imperialism would peter out.

At this point, Strachey notes that non-Marxist economists do not recognize this domestic impetus to imperialism, and he seeks to demonstrate, with considerable help from Gunnar Myrdal, what he calls the theoretical crux of his thought, namely that uncorrected market forces are not harmonious and equalizing, as orthodox thought holds, but result in basic inequalities. Hence Lenin's theory of imperialism is essentially correct, but he didn't allow for political forces building up behind Keynesian economic prescriptions. Here Strachey's thought is clearly related to his *Contemporary Capitalism* which, taken with the book under review, is in a series "intended to illustrate the democratic

socialist approach to the world's problems." The thesis of both books is that modern capitalism is basically a different animal from that of the 19th century variety because the political and social conditions of modern democratic states have modified the terms of exchange between labor and employers. The polarization of society between subsistence-bound proletarians and savings-accumulating monopolists has not occurred. With mass markets, outlets for investment at home appeared, and the pressure to expand overseas was relieved. Taken together the two books form an interesting appraisal of the modern world from the standpoint of this modified Marxism. Much of its analysis will be unacceptable to many orthodox economists who believe the explanation of imperialism to be more complex than this unilinear sequence (though Strachey does pay lip service to other motives for imperialists).

The second and third parts of the book contain a potpourri of things, often full of good insights, the most interesting of which concern whether empires pay. They have not paid, says Strachey, except for the unrequited value which helped in the original take-off. This value was an important contribution; and hence today, in shaping a post-colonial policy in place of imperialism, Britain and other rich countries have the moral responsibility of helping, in their turn, the take-off in the former colonial areas. Given the widening gap in the levels between the developed and underdeveloped world, Strachey following Myrdal holds redistribution is vital, and he comes up with a program of "assisted takeoff" in which the rich countries provide a small per cent of their growing GNP to developing countries to overcome initial inertia. Rich countries must assume this moral mission, because it is right and necessary to do so. In Strachey's words "If we offer the stone of indifference in place of the sword of empire we shall ourselves perish." Again the orthodox will not agree.

However this may be, one may raise the question of why, instead of an hortatory appeal, Strachey didn't explore more of the character of this proposal. The really important question is not whether there should be such a postimperial program, but how it should be carried through. As this reviewer has maintained in his own book on mixed enterprise in India, Western business must accept more social responsibility for international investment and experiment with all kinds of new channels, public and private, for growth and profit. Wooing foreign investment is not the sole responsibility of the recipient countries, but socially responsible private enterprise in the West must be prepared to adjust its demands radically in the modern postimperial world. Moreover, a social sense of long-term strategies must replace the simplistic, short-run, quick-gain and high-return notions, and public investment closely associated with foreign interest must supplant much of the current military-oriented foreign aid. The case rests not only on Strachey's moral grounds; the West's own economic welfare is bound up in the developing countries in the face of alien alternatives. The problem is for Western, especially American, business to change its ancient stiff-necked ethic and accept more long-run social responsibility for its own market system if it really wants its own survival.

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The Latin American Common Market. By the Secretariat of the Economic Commission for Latin America, United Nations, Department of Economic and Social Affairs. New York: Columbia Univ. Press, 1959. Pp. 146. \$1.25.

During the first ten years following the second world war, Latin American average per capita product increased at the relatively high rate of 2.7 per cent per year. This rate of growth appears to have resulted from exceptional factors which brought to Latin American international commerce unusually favorable terms of trade. Since then the fall in prices of primary commodities has resulted in a sharp decrease in the value of Latin American exports, a major consequence of which has been the decline in the growth rate in the region. Nor is this lower growth rate a temporary phenomenon under present arrangements, according to projections by the Economic Commission for Latin America.

The problem that ECLA addresses, then, is how to achieve a similar growth rate to that of the postwar years so that while the total Latin American population grows from about 180 million in 1955 to a projected 300 million in 1975, average per capita income will have increased by about 70 per cent during the same twenty-year period.

What seems to stand in the way of achieving this worthy goal? The necessary very large rise in agricultural and industrial output doesn't appear feasible under the present institutional framework even with a large increase in foreign investment. Unless foreign investment increases at an increasing rate, depending on the rate of interest, servicing this investment would all too soon require repayments abroad equal to the incoming investment.

The requisite growth rate is predicated upon the increased availability of machinery and equipment; but here the problem becomes manifest. The recent decline in terms of Latin America's traditional trade results in a diminution of imports of machinery, equipment and intermediate goods vital to the growth of the area. Furthermore, owing to the low income-elasticity of demand for traditional Latin American exports, no sufficient rise in the volume of exports, is envisaged to offset the decline in value of exports, so that the imports of capital goods that can be financed will not suffice to maintain the requisite growth rate. Too, the effect of import substitution has been carried to such a point, in many of the more developed states in Latin America, that any further decline in exports would affect the importation of vital equipment and intermediate goods and thus the level of output in those states, since they now manufacture much of the consumer goods demanded presently within their own borders.

No one Latin American state has at present a sufficiently wide internal market to exploit the advantages of scale economies which obtain in the manufacture of much machinery, equipment, intermediate goods and vehicles, so that in the present international trade framework, import substitution in these vital lines would only be accomplished at great expense to consumption and, in fact, would lead to an actual decline in the real growth rate. Nor could any Latin American state, in the early stages of development of a capital goods in-

dustry hope effectively to compete in the world market with the great industrial centers of the world in the present international trade framework.

Thus ECLA concludes that a regional payments union which would substitute multilateral payments for the present bilateral arrangements, and some degree of preferential regional market system, to be accomplished gradually and flexibly over time, would provide an internal market large enough to exploit economies of scale in those industries deemed vital to the further growth of the region. Inclusion of reciprocity principles would tend to diminish the advantages of the more developed segments within the region over the less developed, and would, by reducing fears and suspicions, encourage further multilateral trade development in the region.

Although the report by the secretariat of ECLA did not go into the international welfare implications, it is interesting to note that total imports from nonparticipants will not be decreased in their projections, but rather will increase. Indeed, to extend the economic logic of J. Meade, the removal of underemployed labor, with particular regard to underdeveloped regions, from employments where their marginal productivity is virtually zero to employments offering superior productivity, without trade diversion affecting employment elsewhere in the opposite direction to the same extent, can only increase the total of international economic welfare.

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Giant Among Nations: Problems in United States Foreign Economic Policy.

By PETER B. KENEN. New York: Harcourt, Brace and Co., 1960. Pp. xv, 232. \$5.00.

It has become increasingly apparent that neither the success of European reconstruction nor the easing of the global dollar shortage—two of the severest postwar challenges to the stability of the world economy—have perceptibly diminished the degree of involvement of the United States with international economic developments. Today, relatively new problems posed by, for example, Western European economic integration, the capital and other growth requirements of underdeveloped countries, and still unresolved older issues, such as the means of effectively stabilizing prices of internationally traded primary products, seem to warrant as much attention in the formulation of our foreign policy in the economic sphere as reconstruction and the dollar gap did previously. That we must face these and other matters of importance with greater resourcefulness, imagination and intelligent planning than we have shown in the past conduct of our foreign economic affairs is the principal and recurring theme of Professor Kenen's timely book.

Written primarily for the nonspecialists but not lacking in scholarship, this compact volume explores the major aspects of current U.S. economic diplomacy in the fields of foreign trade, investment and aid. The commercial policies, negotiations, and administrative operations carried out under our Reciprocal Trade Agreement Acts, the functions of and our relations with the leading international agencies conducting, promoting and regulating global trade, pay-

ments and lending (e.g., GATT, the IMF, the World Bank), our official policies and attitudes toward our private investment and our numerous foreign-aid programs, are all fairly thoroughly reviewed and in the main trenchantly criticized. Throughout, the prose is free of equivocation or jargon. The potentially widespread appeal of this book is substantially enhanced by its extensive use of data for analytical as well as illustrative purposes. In this respect it is markedly superior to the other (meager) nontechnical current literature in this field, including the widely publicized report of the Rockefeller Brothers Fund (*Foreign Economic Policy for the Twentieth Century*, New York 1958).

While avoiding invective, Kenen's strongest criticism is directed at deficiencies which others have claimed to be characteristic of our foreign relations in general—the growth of bureaucracy in administration, inconsistencies in professed goals and between objectives and actions, and narrowness in viewpoint when broad-mindedness is so urgently needed. And a case can be made for such views of our foreign policy in the purely economic area. Agencies responsible for the administration of our postwar foreign-aid programs have tended to multiply, increasing the difficulties of coordination and slowing the pace of policy implementation. We openly preach liberalism in matters of trade policy, but insist on reciprocity and, after much hard-headed bargaining, grant inconsequential tariff concessions which are rendered even more meaningless by the maze of "escape clauses" and "peril point provisions" imbedded in our commercial treaties. We pride ourselves on principles of noninterference and claim openhanded generosity toward peoples of underdeveloped countries, yet seldom remove our grants from the context of cold-war diplomacy or divorce our public lending from the oftentimes overly confining criteria of "bankability" and "debt-servicing capacity."

This reviewer has no quarrel with what Kenen considers to be the defects of our foreign economic policy. However, the remedial proposals advanced have been voiced before and have as yet failed to gain acceptance with policy-makers, whether because existing institutions lack sufficient flexibility for the proposals to be implemented, or because they do not satisfy other desiderata, or because they are simply unsuited to the temper of the times. If progress is to be made, new insight and imagination must be combined with a sense of what is achievable today—admittedly no easy task.

The author is unduly pessimistic over the prospects of future significant tariff reductions under our Trade Agreements Acts, apparently not seeing that the emerging new trading blocs may help simplify and widen the scope of effective bilateral bargaining. And his negative attitude toward use of selective fiscal incentives to promote our private foreign investment underscores a lack of complete realism when viewed against the background of the immediately pressing capital needs of the underdeveloped countries, the present preponderance of our private over our public lending, and the ever-uncertain level of the latter. But such shortcomings as these do not seriously detract from an otherwise enlightening little book.

SEYMOUR S. GOODMAN

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Soviet Economic Warfare. By ROBERT L. ALLEN. Washington, D.C.: Public Affairs Press, 1960. Pp. x, 293. \$5.00.

A considerable stream of articles and pamphlets has emerged from the research work of the Soviet Bloc Foreign Economic Relations Project of the University of Virginia. Now Professor Allen, the Project director, has written a book-length summary of the findings. The book is intended for a popular audience; it contains no documentation or source references and the index is meagre, but the bibliography is excellent. Because of the absence of documentation, the usefulness of the book for scholarly purposes is severely limited. The professional user of the Project's research will find it more satisfactory to consult the scholarly articles and pamphlets, many of which contain the documentation on which the book is based.

As the well-chosen title indicates, the book is about warfare, and the author approaches the subject of Soviet foreign economic policy primarily from that point of view. As one of the outstanding conclusions, the author presents the proposition that the Soviet Union uses its foreign economic policies "consistently and exclusively to promote the interests of the Soviet state," with the corollary that the purpose of Soviet economic warfare is "undermining the efforts of the underdeveloped countries to establish equitable economic, political and social systems" (p. 3). The conclusion is based on an examination of Soviet foreign trade policies and practices, credits for capital goods and arms, participation in UN technical assistance, Soviet economic and political goals, and economic capabilities. Following these topical chapters is a series of area studies dealing with Soviet policy in various groups of underdeveloped countries. The book contains a rich compendium of detail about the subject, but again the absence of documentation makes evaluation difficult. One reads that "there is some evidence that Eastern Europe and particularly mainland China have become jealous of Soviet agreements to extend assistance to some neutral countries" (p. 15), but since no evidence is presented one does not know quite how to deal with this tantalizing statement.

The book gives indications of having been hastily written, and on a number of issues the author had not quite made up his mind. State trading, for example, is defined in one place as "the device of a faithless country whose announced goal is the conversion and subversion of the world" (p. 25), and in another place it is stated that "a nation which has adopted state trading and which did not use it in the total interests of the state would be seriously remiss in its responsibility" (p. 36). Whole paragraphs are repeated verbatim, such as one referring to Soviet sales of wheat during the '30's "largely at the expense of literally starving Ukrainians" (p. 110 and p. 114). One is referred to Appendix A for data supporting a generalization about Soviet Bloc trade since 1938 and 1948, only to find that Appendix A contains no systematic data for those years (p. 118). More careful editing would have spared the reader these irritations.

The author has had to thread his way gingerly through what is undoubtedly difficult and contradictory material. He shows, for example, that there is no consistent pattern in Soviet foreign trade pricing policy. One does have

the feeling, however, that the Soviets are "damned if they do and damned if they don't." When they overprice their exports they are charged with being "predatory" (p. 169) and when they underprice them (as in the case of aluminum and tin) they are engaged in "questionable practices" (p. 171). There is an apparent paradox between the author's findings that "much of Soviet performance so far has not been good" and that "there can be no doubt that so far Soviet foreign economic activities have been successful" (pp. 246-47). But he faces the issue squarely and argues that the success is based on reasons other than actual performance, such as the novelty of Soviet aid, Soviet support of underdeveloped countries' aspirations for industrialization, their desire to be neutral in the cold war, and other political factors.

Soviet Economic Warfare contains an impressive quantity of material on a subject of great political and economic interest. Despite its technical weaknesses, it should be of interest to a popular audience.

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Industrial Organization; Government and Business; Industry Studies

Antitrust Policy, an Economic and Legal Analysis. By CARL KAYSEN and DONALD F. TURNER. Cambridge: Harvard University Press, 1959. Pp. xxiii, 345. \$7.50.

The central purpose of this impressive and provocative book is to propose that antitrust policy be revised so that its principal focus becomes the prevention of undue market power. The argument runs as follows: The chief aim of antitrust policy should be the maintenance of competitive processes, subject however to the condition that this is not incompatible with the desirable economic objectives of efficiency and progressiveness. The pervasive presence of oligopoly is the principal source of jeopardy to competition; and the non-competitive parallel action of oligopolies cannot be adequately curbed under the present law, even if the provisions against restrictive agreements are broadly applied and full use is made of the provisions concerning monopolization. Moreover, the existing antitrust law, though increasingly concerned with market power, is deflected by its continuing emphasis upon undesirable conduct, which makes it unreliable when it is applied to impairments of competition by decorous means. The proper goal should be "the reduction of undue market power, whether individually or jointly possessed." To this end there is need for a broad program of dissolution, divorcement, and divestiture designed to eliminate market power where possible unless those possessing the power can show that they derive it from economies of scale, valid patents, or the introduction of new processes, products, or marketing methods.

The heart of the book consists of a statistical summary of the extent of oligopoly, a statement of the argument for the proposed revision of policy, discussion of the relevant economic and legal concepts, analysis of the operational problems that are likely to arise, and suggestions as to appropriate

changes in law and administration. These appear in Chapters 1 to 4 and the last part of Chapter 8.

The book has a second purpose, less fully worked out: to appraise antitrust policy generally and to make suggestions for improvement. Apparently the discussion of market power was supplemented (a) because the parts of the law that prescribe rules of conduct should be altered in some respects if they are to reflect a central purpose to condemn what contributes to undue market power; and (b) because if undue market power could be prevented the problems to which some other parts of the law are addressed would be simpler and the law could be correspondingly changed. Considerations such as these underlie the discussion of mergers, of price discrimination, and of some aspects of the patent system.

However, once the field of analysis has been broadened, it is not limited to matters thus related to the central theme. The book includes: general treatments of the scope of "per se" rules and antitrust aspects of patent and price discrimination law; a discussion of exceptions to competitive policy, with special attention to surface and air transportation, crude oil production, and organized labor, and with a brief condemnation of the exemption applied to arrangements for resale price maintenance; a chapter concerned with the competitive impact of tax policies, government procurement, direct aids to small business, and commercial policy; and half a chapter concerned with general problems of administration and enforcement. What is said about nearly all of these topics is fresh, stimulating, and wise; but some of the discussions are so brief that they amount to little more than casual remarks. Labor is treated, for example, in four pages, transportation by rail, truck and air in five, tax policies in six.

Moreover, the predominance of the book's first purpose has distorted the volume in its balance and emphasis as a general analysis of antitrust policy. The clearest instance is the summary treatment given to collusive agreements. These receive sustained attention only as one topic in a general consideration of per se rules. Yet proceedings against restrictive agreements have long been and are now the most numerous, and probably are the most important, activities under the antitrust laws. When they are given only casual attention, their role is easily misconceived.

Thus the statement is made (p. 101) that in a market with many firms, none of which is dominant, it is difficult for a cartel to operate successfully without aid in controlling entry, either from the government or from a labor union. Yet in U.S. proceedings against restrictive agreements and in the practices of cartels in those parts of Europe where the government neither curbs nor fosters cartels, there are many instances of other effective ways to cartelize small business—for example, by mutually exclusive arrangements for cooperation between a producers' cartel and a distributors' cartel. The antitrust laws have been effective in destroying the opportunity for such mutual support among cartels in this country. For this reason, the problem area in American antitrust policy is not cartelization but the power of large enterprises. A book concerned with this problem area cannot easily become, by supplementation, an adequate analysis of antitrust policy as a whole. Conceived as a general

treatment of the antitrust laws, this book, though thought provoking and valuable, lacks the authority and balance that it possesses as an analysis of the relation between competition and business power.

In developing the latter theme, Kaysen and Turner have made a distinguished contribution to thought about the most important and difficult of American antitrust problems. Their argument that power is the proper focus for attention is the most persuasive statement of the view that has come to this reviewer's attention. Their discussion of the relation between power and performance and of the limits to an attack upon power that might be set by a concern for efficiency is likely to become a classic statement. In blending economic and legal analysis of the conceptual and operational problems that are raised by their proposal, they have enriched the contributions of economics and law alike. They have had the courage to face practical problems, even to the point of outlining the provisions of their suggested laws; yet they have clearly distinguished the fundamental principles of their proposal from the variant methods by which it could be given effect. Their book should become one of the landmarks in the development of antitrust policy.

Nevertheless, there are two important gaps in the exposition. The first has to do with the concepts of oligopolistic concentration that are used in analyzing the extent of oligopoly (Ch. 2) and in suggesting a rule of thumb that might be employed to simplify the attack upon market power (p. 98). An attempt is made to estimate statistically the extent of oligopoly in manufacturing and in mineral extraction. This undertaking necessarily involves two tasks—determining the degree of concentration to be regarded as significant and marshalling the evidence as to the prevalence of the relevant concentration.

The second task has been undertaken ambitiously on the basis of census data supplemented from other sources. In an effort to make census industries more nearly expressive of competitive markets, industrial classifications have been consolidated; and the standards and procedures used have been set forth in a methodological appendix. But the first task, which governed all the statistical work done for the second, has been performed summarily, without explanation of the choices available and of the reasons for the decisions reached. Concentration is analyzed with reference to two types of oligopoly—one type in which the first 8 firms have at least 50 per cent of the market and the first 20 firms have at least 80 per cent, and another type in which the first 8 firms have a market share of at least 33 per cent, with the rest of the market relatively unconcentrated. We are told that, though these types are defined arbitrarily, there is support for the definitions in empirical studies because such degrees of concentration appear to be significant for behavior in "the majority of markets with which we are familiar" (p. 27). This reviewer is tempted to say that most markets with which he is familiar suggest to him that anticompetitive oligopoly behavior is likely to arise only at levels of concentration decidedly higher. But such dicta, pro or con, are not useful. There is need for a reasoned discussion of the considerations that support the definitions chosen and of the nature and extent of the empirical evidence that

appears to reinforce the choice. This comment is made in the hope that it will evoke, by way of reply, an article supplying conceptual and factual support for these concepts of significant concentration.

Similarly, a rule of thumb is suggested (p. 98) to simplify the application of a law against market power: that market power might be conclusively presumed where, for 5 years or more, one company has accounted for 50 per cent or more of annual sales or 4 or fewer companies have accounted for 80 per cent. The arbitrary nature of such a rule is recognized, and presumably the exact percentages are not considered important. But since the proposed presumptions are intended to be conclusive, one must suppose that the percentages selected are believed to be high enough to preclude reasonable doubt, even when entry into the market is relatively easy. In the light of the complexity of the structural components of market power, described by the authors in previous pages, there is need for a discussion of the reasonableness of any conclusive presumption based upon market shares alone.

The importance of the failure to support the chosen concept of oligopoly lies in the fact that this concept necessarily conditions what is said about the prevalence of the problem of oligopoly power, the probable relation of this problem to economies of scale, and the feasibility of legal action. Nevertheless, the weakness is not critical. One might think oligopoly much less prevalent without denying that the problems it raises are of major importance and without rejecting the proposed policy for coping with it.

The second gap is more serious than the first. It is the gap between the concept of *market* power, upon which the analysis is based, and the facts of power and activity extending across numerous markets, which will often need to be considered in appraising the power of our larger enterprises and in devising programs of dissolution, divorcement, and divestiture. The concept of market power that is used is that familiar to economic theory, though developed with unusual subtlety. It involves determining the relevant market by grouping products in the light of cross-elasticities of demand and supply and of the geographic limits within which buyers and sellers have contact. It then involves estimating power in the relevant market on the basis of market shares, ease of entry, and the like.

Though the discussion recognizes that absolute size, as well as market share, may affect power, this point remains unanalyzed because "most absolutely big firms are also big in relation to at least some of the markets in which they operate" (p. 101). The exposition takes for granted that "small firms may have large market power because they are large relative to their rivals in the market" (pp. 100-1). There is no consideration of the question whether an absolutely large enterprise may have significant power even in the markets in which its share is relatively small; nor of the question whether a small concern with a larger market share than its rivals may lack power when the rivals, in their total activities in all markets, dispose of much greater resources in absolute amount. Such questions are often important; for large enterprises like General Electric or Du Pont are large primarily because of diversification that outruns the widest possible definition of a relevant market;

and in some of their markets their rivals are smaller and more specialized concerns that possess larger shares of the particular markets in which these concerns operate.

The problems that arise as to power based on diversification are numerous, and most of them have received little serious discussion. For example: (a) How far are small specialized firms with large market shares intimidated by the possibility that competitors (which may or may not have smaller shares in the specialized market but possess greater resources through greater diversification) might discipline or destroy them by price reductions or other forms of aggressive selling focused upon the specialized market? (b) When large diversified producers share several markets, each of them having a dominant position in one market and subordinate positions in the others, how far is their rivalry blunted, not merely by oligopolistic thinking about the single markets, but also by fear that retaliation might spread from market to market until the dominance of each enterprise was jeopardized in the market most important to that enterprise? (c) How far is oligopolistic forbearance weakened within any one oligopoly market if the oligopolists are diversified in other markets to different degrees and in different directions, so that they do not elsewhere encounter one another as rivals, and so that they have different ideas about the place of the particular oligopoly market in their strategy as whole enterprises? (d) Does a large concern that is a part of a dozen different oligopolies have power greater or less in each oligopoly than it would have if it was a part of that oligopoly only?

The first question arises about such relationships as that between General Motors and specialized producers of electric refrigerators or between General Electric and specialized producers of wiring devices. The second question is pertinent to the relations among chemical companies that are diversified but emphasize different classes of chemical products. The third question is pertinent to such oligopolies as that for automobile tires sold to the replacement market. The fourth question recently played an important part in consideration, by the European Coal and Steel Community, of a proposed merger among steel companies.

Consideration of such questions has been slight in the application of the antitrust laws, because the few proceedings striking directly at power have pertained to matters for which the questions were least pertinent. In a general program like that proposed in this book, similar questions could not be avoided. Indeed, a simple single-market analysis of power might produce bizarre results in such a program. For concentration ratios tend to be highest in medium and small industries; and since the principal activities of our largest enterprises lie in our largest industries, one might find that the broad attack upon market power tended to by-pass many of the largest concerns for proceedings against smaller ones whose power was more readily detected by an oversimplified analysis.

This second gap is as important in the discussion of remedies as in the diagnosis of power. The proposal is to reduce excessive power by dissolution and divestiture, dividing big enterprises into small replicas and, where necessary, eliminating or reducing vertical integration. Such surgery is discussed

on the unstated assumption that it can be applied neatly to the activities of the relevant market without affecting the structure of other markets. This is true for enterprises that operate only in the relevant market or limit themselves, outside that market, to vertically related antecedent and subsequent operations. It is also true, more widely, for diversified enterprises whose operations in the relevant market and in the related vertical line are neatly segregated (if not in separate corporate subsidiaries, at least in separate plants).

There remain, however, two classes of situations in which the structure of enterprise cannot be so simply altered. One is that in which plants are diversified and in which, therefore, it is impossible to modify the scale of a company's operation in the relevant market without affecting the scale of its operation elsewhere. The importance of this class of cases could be roughly estimated by comparing, in census industries, the output of products primary to the industry with the output of the industry (which includes the total output of all establishments primarily engaged therein).¹ The difference between the two figures would consist of products of other industries made in the industry's establishments. Where the difference is large, a dissolution program would be likely to have substantial effects unrelated to the industry's primary products.

In the other class of situations, vertically integrated concerns are not neatly restricted to a single succession of activities. A single raw material is fabricated by different processes into a variety of products. A single fabricated product is made from a diversity of antecedent materials, some of which are used to make other products as well. At successive stages of the sequence of operations, different percentages of the concern's total output are consumed internally and different percentages sold on the market. A coordinated program of research leads to the production of various commodities, each of which, though technologically related to the others, is produced by specialized equipment and sold in a special market.

Dissolution of a complex vertically integrated enterprise based upon analysis of the power problems of a single one of its interconnected activities would inevitably affect both its power and its efficiency in other activities. One could not dissolve a vertically integrated steel company because of its power in the market for sheet steel without making decisions about its capacity for pig iron and basic steel and thus about its other fabricated products as well. Neither could one dissolve a steel company because of its power as a producer of basic steel without affecting the degree of vertical integration and the market position of each of the company's fabricated products. To dissolve an electronics company because of its place in radio or television would not only raise the question whether any of the newly created firms should be connected with the central research establishment but also raise the question whether research pertinent to other types of electronic equipment would be affected. In such cases, the practicable forms of dissolution are likely to reach beyond the relevant market and to force consideration of the broader aspects of power and efficiency.

One could, of course, reduce the apparent size of the discrepancy by defin-

¹ Even such an estimate would be an understatement, since in some industries the census definition of an establishment is such that one plant may contain several establishments.

ing the relevant market broadly—by giving great weight to cross-elasticities of supply where they do not correspond to cross-elasticities of demand and by lumping together goods that can scarcely be considered substitutes. One could place all steel products in the relevant market, ignoring the differences between rails, merchant bars, sheet, strip, drawn wire, and structural shapes, because all these products are made from similar materials and often by the same great companies. The concept of the relevant market would thus be more vague, but the market as defined would be nearer to the facts of the power structure. To proceed in this way would be to attempt to remedy insufficiency of analysis by looseness of definition; and it is not at all certain that the errors would compensate one another.

Though economic analysis has proceeded market by market, and economists are naturally reluctant to depart from their tradition and thus depreciate their intellectual capital, the stubborn fact is that large enterprises are no longer merely market entities. Their efficiency or inefficiency has many intermarket and some nonmarket aspects. Their power is not merely power in a market nor merely the arithmetic total of their separate strengths in their various markets. If their power is to be effectively reduced by practicable modifications of their structure, they must first be seen as complete organizations and analyzed as such.

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A & P: A Study in Price-Cost Behavior and Public Policy. By M. A. ADEL-MAN. Cambridge: Harvard University Press, 1959. Pp. xiv, 537. \$10.00.

With the appearance of this book economists have much less reason to complain about the paucity of analytical literature on the economics of the distributive trades. Most studies of price determination for consumer goods pass over the distribution phase lightly and proceed as if manufacturers sold to final users. Increasingly that assumption is questioned; and one is reminded of the agriculturists' traditional contention that "what happens in the dark" between producer and consumer has much to do with economic welfare. Reference is not only to the efficiency with which and the price at which the distributive function is performed but also to the effect of these trades on the supplying industries. Recently, but based largely on casual observation, views as to how well mass distributors play one or both of these roles have ranged from the contention that they countervail oligopoly in supplying markets to identification of them as one side of a series of bilateral oligopolies; or to the claim that the consequences of that structural situation are tempered by the superior position of the mass distributors as potential entrants into the supplying industries and by the competitive milieu in which they resell. One author asserts "The formula for competition is quite simple; add one part of Sears Roebuck to twenty parts of oligopoly [in manufacturing]"¹ Change the name to A & P and one has the major conclusion of the book under review. But it

¹ Clair Wilcox, "On the Alleged Ubiquity of Oligopoly," this *Review, Proc.*, May 1950, 40, 71.

emerges from an intensive, theory-oriented study of one company that serves as a stereotype of the mass distributors of food.

In contrast to *Catalogs and Counters*² where all facets of Sears Roebuck and Company's history are covered, this study of A & P deals only with the issues of major concern in industrial organization and price analysis and with the related policy problems. Only with respect to adaptation of retailing facilities to changing conditions do the controlling Hartfords appear as bumbling. In contrast they understood fully the import of a consistent policy of a very low store-wide gross margin (or "price of retailing services," an identification made by the author in other writings) that would attract so much volume that operating costs would fall sufficiently to make this the long-run profit-maximizing policy. Divisional managements, with their eyes on the shorter run, had to be persuaded and at times disciplined. Equally well visualized by top management were the procurement methods that would cut to the lowest possible level cost of goods laid down at the retail outlets.

While reasons for savings from integrating wholesaling with retailing are not brought out by the author, the other sources of low landed-cost of goods to resell are specified: skill in buying, savings from quantity purchases in depressed markets, discriminatory prices obtained, and the gains from the company becoming its own supplier. The last course was followed where the supplying industry was deemed inefficient or had discriminated against A & P (a frequent observation in the book) because, in earlier years, of the outmoded practice of not giving A & P the equivalent of the wholesaler discount and, in later years, under the requirements of the Robinson Patman Act. Self-supplying proved to be very profitable, measured by transferring goods to retail divisions at market prices, because of manufacturing and transfer economies made possible by vertical integration and because of escape from manufacturers' prices for well-known brands. Acceptance then had to be earned for A & P's private brands. Here one misses an exploration of the extent to which that move was facilitated by dynamic influences weakening the hold that high-priced brands had on consumer preferences once the packaged commodities became of generally accepted quality. The equally successful integration back to shipping point or terminal market *office* purchase of fresh produce stemmed from the superiority of information and of direction of flow of perishables in an integrated organization and from by-passing the notoriously inefficient physical handling of perishables in large-city terminal markets. For the store-wide business, the combination of low landed-cost and a lower gross margin for retailing than even chain store rivals could match—advantages that lessened as A & P was imitated—all worked to the benefit of the consumer.

But the federal government and some economists have not viewed all of this as good. Indeed, most of the primary material used by the author came from the display of the company's affairs (through 1941) in a series of antitrust proceedings. Drawing on data in that source, the author concludes that discriminatory buying prices in favor of A & P at most accounted for 15 per cent of its retail price differential below independent stores, in contrast to the Fed-

² Boris Emmet and J. E. Jeuck, *Catalogs and Counters*, Chicago 1950.

eral Trade Commission's conclusion in its 1934 *Final Report on the Chain Store Investigation* that "the growth of the chains was 'based largely on special price concessions from manufacturers' . . ." (p. 241). Rather than merely thwarting such concessions, the legislation stimulated by that *Report*, the Robinson Patman Act, is characterized as having "a single unifying principle; to enforce discrimination against the lower-cost buyer or lower-cost method of distribution" (p. 160). In contrast, the author argues strongly that price differences in favor of the large buyer (often even when discriminatory) aid competition in imperfect markets; and so do price reductions in resale that enable economies of scale and rapid turnover to be obtained. The government's brief in the 1946 Sherman Act case acknowledges the latter point, but still accuses the company of intending to create a partial monopoly by "selling below [current] cost" in some localities (rather than selling on a systematic cost-plus basis) to squeeze out rivals, a practice made possible by "recoupment" from profits from other operations; but no substantial evidence was offered that competition was injured thereby. Compared with an interpretation oriented to price theory, this is naive economics; and yet it was adopted in large part by the court in its decision, because, as a consequence of the ineptness of the defense, the contentions were not offset during the adversary process in the court proceedings. The latter deficiency has now been remedied by the author; but not all will be convinced.

Journal articles containing previews of the author's criticisms of this case have elicited strong reactions that will not be quieted entirely by the book's full presentation of the evidence used and by the analysis of it that proceeds along orthodox lines. Profit maximizing may lead to price reductions to reduce costs, as A & P has done in retailing, or cost reductions in acquisition of goods, as it has done so that resale prices can be reduced, all to gain a competitive advantage that will pay off. The conclusion is that competitors who do not match these moves fall but competition is enhanced. Against claims that A & P was tending to create monopoly (the fewness of outlets in a shopping area is considered to be inherent in retailing) the final appeal is to the ease of entry into retailing—this is assumed—and to the test of the market, which in this case is the effective imitation of A & P's methods by other chains and by independents with cost-saving relations with wholesalers. All of this will not convince some, but to disagree with the author they will have to use other evidence than he has—a difficult task—or question the adequacy of his use of the orthodox framework in his elaborately developed analysis, or argue from another framework. Some will not be satisfied with the ease-of-entry assumption in light of evidence that A & P fought loss of volume by price cutting, or will be concerned about competitive behavior of shopping-area oligopolies made up largely of outlets of the same small group of companies that meet each other as sellers, and to a lesser degree as buyers, in hundreds of such localities.

These and other major topics in the book would be seen in better perspective had the author placed A & P more pointedly in the setting of the revolution in food distribution. Then A & P's behavior (the period covered ends with 1941) would have been seen as part of a massive transition with inevitable severe

consequences for some and with the leader in low-cost distribution both bringing about, and being able to take advantage of, the eroding position of those that were out of step. The emerging and more stable structure could then have been identified and appraised, although doing so definitely would have called for examining the food trades in the 1950's.

Even without these additions this book is by all odds the premier study of a distributive industry, not merely of a company. The mass of evidence examined is impressive and the guide lines for its analysis clear. The investigation of the relevant policy problems is unusually extensive and challenging and to a large degree is convincing. And not least, the style is lucid and often spritely; the book is pleasant reading.

R. B. HEFLEBOWER

• *Northwestern University*

The Theory of the Growth of the Firm. By EDITH TILTON PENROSE. New York: John Wiley & Sons, 1959. Pp. viii, 272. \$6.00.

Mrs. Penrose seeks to develop a theory of the *growth* of the firm, as distinct from the traditional theory of its equilibrium size, factor combination, output and pricing. The latter approach, she holds, is ill-suited to the modern business enterprise and to modern problems of real importance. She applies her theory of growth to the explanation of business diversification, integration and merger, and to the social problems of small-business survival, industrial concentration, and over-all economic growth.

In a number of respects Mrs. Penrose builds on quite orthodox foundations. Competition, though imperfect, is vigorous, except where special "artificial" barriers exist. Informed, though far from perfect, knowledge and expectations are assumed. The classical postulate that the firm (i.e., its managers) seeks vigorously to expand its profits is retained, and she rightly sees that in the actual world profit and growth are mutually compatible and requisite rather than antagonistic goals.

Departures from orthodoxy are nevertheless considerable. The firm's managers are interested in increasing the absolute level of profits rather than maximizing its rate. Most important, the notion that management is a "fixed" factor limiting size or growth is rejected. On the contrary, she contends, the successful firm continuously generates underused management resources, and this "receding managerial limit" is the key factor explaining business expansion. Because of it "there is no 'optimum,' or even most profitable, size of firm," and a firm's size is "but a by-product of the process of growth." Firms are limited only in their *rate* of growth through time.

Mrs. Penrose distinguishes between the economies of size and of operation on the one hand and "economies of expansion" on the other. The latter refer only to the cost of effecting expansion, not the average costs of producing once expansion has occurred. They are not uniquely related to the size of the firm, and can be measured meaningfully only by comparing the costs of expansion by one firm with those of another. "Economies of growth" may be economies of size or merely economies of expansion, or both.

While most firms have some impetus to expand, some will enjoy advantages over others, and the economies of expansion of different firms may lie in different directions. While all successful firms tend to grow, the direction and rate of growth of any firm depends on the particular pattern of its underused resources and opportunities.

Mrs. Penrose introduces one other key concept, the *interstices* of the economy. These are new opportunities for expansion that develop out of macro-economic growth, change and innovation. The interstices are especially important to small and new firms, since they permit them to expand despite the disadvantages of small size. Larger firms either cannot currently exploit them or, on the principles of comparative advantage, choose not to do so.

Armed with these concepts, Mrs. Penrose attacks her problems. Diversification reflects the fact that underused management is normally more profitably employed in related than in old lines, and that interstices appear largely in new lines. Merger is analyzed at length, largely in terms of the differential economies of expansion and diversification and the relative costs and profits of expanding internally and by acquisition. Mergers permit more rapid growth but do not necessarily lead to increased concentration. New and small businesses persist largely because the changing interstices of the economy throw up more opportunities than large firms can seize.

Regarding the outlook for concentration, Mrs. Penrose is measuredly optimistic. Despite the continuous generation of economies of expansion, there are special brakes upon the speed of growth of giant firms. On the one side "oligopolistic competition" leaves less management free for expansion. On the other, the amount of management required per dollar of expansion rises as firms and expansion programs increase in size. Hence the rate of expansion of very large firms tends to decline. If economic growth and innovation develop ample interstices, the over-all growth of smaller and medium-sized firms will exceed that of the very large. She concludes: "... in a steadily growing economy, or in an economy where expansion is more prevalent than stagnation, the process of concentration will come to an end and eventually reverse itself" (p. 258).

But, she warns, this requires steady economic growth and the absence of serious "artificial" barriers to expansion by new, small and medium-sized firms. Conversely, the stifling of new and smaller firms will reduce economic growth, and strengthen the hold of dominant concerns. Given a fair field for competition, economic growth will continue and concentration will not increase.

Mrs. Penrose has made a significant contribution to our thinking about the growth of business firms and the outlook for capitalist industrial structure. She has, in my judgment, properly stressed the key importance of management, innovation, and economic growth in these problems. One could wish she had sharpened her concepts somewhat, and in places, that the discussion were less discursive. But as a pioneer effort on the theoretical-applied level in a very important field, her book deserves wide attention.

JAMES S. EARLEY

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Die Investitionspolitik in nationalisierten Industrieunternehmen Frankreichs seit ihrer Nationalisierung. By WALTER STOCK. Köln: Westdeutscher Verlag, 1960. Pp. 220. DM 22.00.

In this country, much has been done to reduce the discrepancy between the hypothetical models of the investment process and actual investment decisions. In Germany, the gap has been less recognized, either because less factual information on actual decisions has been available or because a closer rapport exists between specialists in theory and management. Especially the books of Erich Schneider on *Wirtschaftlichkeitsrechnung* and of Erich Gutenberg on *Betriebswirtschaftslehre* have developed the criteria and methods of calculation which firms ought to employ if they seek to arrive at fully rational investment decisions. The author of the present book has not only relied upon these guides for the making of rational decisions but in part (pp. 33-63) has used theoretical formulae as if they were factual evidence on investment planning whenever the French nationalized industries enjoying financial autonomy did not furnish the necessary information.

A somewhat different method was employed for analyzing public investments financed through the budget. A global growth model for an open economy was utilized as the standard for evaluating French investment plans. As one would expect, the result of the comparison was negative: the French planning agency was not guided by the functional criteria suggested by an equilibrium model of growth. In the first phase, the agency concentrated upon sectoral growth so as to overcome the losses of capacity during the second world war and to promote production in the less developed regions. However the public investments were financed, the author did not seem to employ a method of analysis that gave full recognition to the actual goals underlying French investment planning.

It so happens that the Monnet Plan (1947-52) and the Hirsch Plan (1954-57) differed in their goals and scope as well as in their techniques of investment planning. The Monnet Plan was limited to coal, electricity, steel, cement, transportation, and agricultural machinery. The Hirsch Plan concentrated on investment for housing, consumer-goods industries, as well as agriculture and overseas areas. In their methods, the first plan arrived at the desired investment estimates by projecting the expected demand in the various industries. The second plan was placed within the national income account of 1951-52 and utilized more modern procedures of estimating desired investment. Yet at no place in the book is there a clear statistical comparison of the two plans. Since the book was organized in terms of a global growth model, there seemed to be no reason for contrasting the two plans. Nor is there a critical examination of the different procedures of estimating desired investment, partly because the particular version of the input-output tables used had not been published (p. 108). The interesting question as to whether in France modern techniques have resulted in superior planning estimates remains unanswered.

In examining the effects of investment planning, the author largely limits himself to the Monnet Plan. There was a significant increase in capacity, production and productivity of the six industries promoted. Most successful was the case of hydroelectric power, in which capacity rose by 73 and output by 66

per cent from 1947 to 1954. Investment plans were fulfilled by 95 per cent for coal and electricity. In comparison with other European countries, French coal production per work-day underground in 1954 was 23 per cent above the level of 1938, whereas Great Britain had only gained 11 per cent and West Germany was still one-third below its output of 1938. For some of the basic industries, the coupling of nationalization with detailed investment planning seems to have been a success.

The Monnet Plan collided in two respects with the over-all development of the French economy. Although the volume of gross saving came close to the 25 per cent of national income stipulated in the plan, there was no ready transfer of these savings to the basic industries. A compulsory loan provided the investable funds for the earlier years. Anticipating a sufficient supply of foreign currency in the plan, the French economy accumulated a considerable deficit in its balance of payments. Payments under the Marshall Plan eliminated this deficit. The Marshall Plan thus removed the two major hurdles that hindered the realization of the Monnet Plan. The price stability from September 1949 to June 1950 indicated that the two plans together had also removed the inflationary pressure. In the absence of the Korean war, a coordinated internal and external increase in supply would have shown that inflation can be overcome by effective planning.

ARTHUR SCHWEITZER

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Capital in Transportation, Communications, and Public Utilities; Its Formation and Financing. By MELVILLE J. ULMER. National Bureau of Economic Research Study No. 4. Princeton: Princeton University Press, 1960. Pp. xxxviii, 548. \$12.00.

Professor Ulmer's book is the fourth in a series of National Bureau monographs on the principal capital-using sectors of the economy. It provides material for a wide range of studies of the regulated sector of our economy, and is a substantial contribution. The previous National Bureau monographs on capital formation dealt with residential real estate, agriculture, and financial intermediaries. Additional studies embodied in occasional papers have treated manufacturing and mining.

The individual studies have borne the imprint of the talents and interests of the various authors as well as the peculiarities of the sectors examined. Ulmer's work is no exception. While he follows the path trod by Creamer, Borenstein, and Tostlebe, in preparing estimates of the capital-output ratio, he also strikes out on his own in a number of important directions. Principally these are an examination of long-cycles in investment and a study of methods of financing capital formation.

The regulated industries examined in detail are the steam railways, electric light and power, telephones, street and electric railways, and local bus lines. The period examined generally runs between 1870 and 1950. As a consequence, there could be little analysis of the air transportation sector.

Over one-half of the book is taken up with appendixes concerning sources

of data, methods of deriving various statistical constructs from the data, and notes on the accuracy of the estimates. These sections will provide a mine of information for future investigators of these industries.

The raw materials of the study consist of various government and industrial reports on individual parts of the regulated sector. From these reports the author has distilled estimates of time series of output and capital formation. It appears that greater attention was paid to the problems of measuring capital formation and capital consumption because of the availability of earlier estimates of output in the works of Harold Barger and J. M. Gould.

The raw material is organized into chapters in which the following issues are examined: secular patterns of output and capital formation; trends in capital coefficients; factors underlying long-term trends; long cycles; the evolution of financial structures.

The author examines in great detail secular changes in output and in the stock of capital. He uses the capital-output ratio to summarize these trends. The coefficient is used both in the average sense as the ratio of the stock of capital to the output flow at a point in time, and in the marginal sense as the ratio of the change in the stock of capital to the change in the output flow during an interval of time.

Ulmer finds marked declines in the average and marginal capital-output coefficient of steam railroads, electric light and power, telephones, and street and electric railway. The smallest declines appear in telephones. In local bus lines, there appears to be no secular decline in the coefficient, and the author is quick to take advantage of this information in a later chapter interpreting these results. He recognizes correctly that these secular movements must be interpreted in terms of three major sets of economic forces: (1) Construction of facilities in anticipation of demand will yield a high ratio of capital to output in the early years of any industry, and a subsequent decline. (2) The indivisibility of capital units is an important feature of any regulated industry which owns its own right of way or power transmission system. The existence of increasing returns in these industries when they are first built will yield a gradual decline in the capital-output coefficient. (3) Improvements in technology have been capital-saving.

The author is not successful in isolating any changes in the capital coefficient which might be due to changes in the relative prices of labor and capital. He uses an index of wage rates in constant dollars, and an index of long-term interest rates to carry out this analysis. This is not an appropriate procedure. The cost of using a capital good is also dependent upon its initial purchase price and expected life. This is an area where much more work will have to be done before satisfactory conclusions are unearthed. The data which Ulmer has made available should encourage this effort.

Ulmer devotes a chapter to the long cycles evidenced by his capital-formation data. He shows that these data conform fairly well in cyclical dating to the long cycles found by other investigators. The main interest of this chapter lies in the author's attempts to explain the conformity of the series as well as in his ideas concerning the economic significance of the long cycle.

The final chapter of content concerns itself with the evolution of financial

structures. Here Ulmer breaks new ground. Through the construction of tables showing the sources and uses of funds, he examines methods of financing gross investment used by these sectors. His conclusion bears quoting because of the implications for public regulation.

There was a . . . pronounced and progressive shift over time toward internal financing. In the railroads this trend is in evidence in its most extreme form. In the 1880's, railroads secured 98 per cent of capital needs from outside. As in the case of all regulated industries, very little use then or at any other time—with one brief exception—was made of short-term credit. Ninety per cent of all capital requirements were met through the issue and sale of stocks and bonds. Gradually, in the subsequent periods the relationship between internal and external sources shifted. By the decade just preceding the entrance of the United States into World War I, internal sources had already mounted to the level of more than 40 per cent of all financial requirements. About two-thirds of this came from retained profits, the rest from depreciation charges. . . .

The next three decades witnessed the climax of this trend. Between 1921 and 1940, 95 per cent of the industry's funds were generated internally, either through retained profits or depreciation charges. . . . In the final decade of the 1940's, all capital needs were satisfied from internal sources. . . . And it should be borne in mind that this was a decade of very substantial investment. . . .

The same trend, but in modified form appears in the other components. In electric light and power, . . . between 1938 and 1950, more than 50 per cent of all capital requirements were obtained internally. . . .

In telephones, however, . . . the trend toward internal financing, while evident over the 1877-1950 span, was less distinct than in the other regulated industries (pp. 155-56).

These findings with regard to the financing of capital formation raise a complex issue for the regulatory authorities. To a large extent the above patterns may be ascribed to the declining profitability of the rail sector relative to the other two sectors. This in turn is partly due to declining demand for rail service and partly to regulatory restrictions on the adjustment of rail resources and services. Sooner or later an adjustment of rail facilities to reduced demand will be carried out; and it is safe to predict that what we still call the steam railroads will not expand their stock of capital at a rate comparable to the past.

In view of the possibility that future capital requirements for rail service can be met out of retained earnings and depreciation, the rate of return on rail capital required for continued service may not be as high as that set by the market on new capital issues. It will be recalled that one of the chief arguments against the use of marginal cost pricing for rail service was the failure to cover total costs including the cost of capital. An industry so regulated would not be able to attract additional capital. But it now seems reasonable to explore the possibility that the railways might charge according to marginal cost, might meet their interest charges, and expand as necessary through re-

tained earnings and depreciation charges. This would require the long-term debt to be permanent. The appropriate earnings on equity capital would then be a residual. In truth the equity holder would pay the tax necessary to satisfy the Lerner-Hotelling condition. As long as fixed bond interest charges are met, this does not require any elaborate bankruptcy procedure, for the equity market will evaluate the possibility of future rail dividends. In view of the dim prospects for rail earnings at present and the treatment of rail equities in the securities markets, this proposal may leave the equity holder no worse off than he is now. This last suggestion was stimulated by a reading of the Ulmer volume, but it in no sense reflects Ulmer's thinking or work.

GEORGE H. BORTS

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• *The Electric Interurban Railways in America.* By G. W. HILTON and J. F. DUE. Stanford: Stanford University Press, 1960. Pp. 463. \$9.50.

George W. Hilton and John F. Due have collected a storehouse of information concerning a little-known but important sector of American transportation history—the rise and later disbandment of the electric interurban railway.

The authors divide the book into two parts. The second part deals with the individual histories of interurban lines throughout the country and contains some of the best writing in the book. It is not for casual perusal, even though the thumbnail sketches of each line would be interesting for any reader. Although the order in which these histories are listed is not particularly helpful, the voluminous research on a diverse set of local enterprises has been admirably compressed into short pertinent paragraphs.

In the first part of the book, where the history is more general, the authors give their definition of an electric interurban railway and then examine selected aspects of the industry. They describe in detail the location, construction and financing of small disconnected lines between communities where promoters found favorable conditions, and the struggle to form economically profitable systems from bits and pieces.

In the chapter on the technology of the industry, detailed listing of the interurbans that utilized some particular type of equipment somewhat obscures the fact that during the short life cycle of the electric interurban railway, there was no final decision on an optimum type of equipment. Each promoter preferred his own combination of choices.

This reviewer wishes that the activities and problems of the industry in the basic areas of labor, accidents, advertising, land development, franchise provisions, and community relations had been as adequately subjected to research as were the areas included. There is need also to explore the reasons why major sources of capital were willing to invest so heavily in the electric interurban industry (for instance, an examination of the joint-cost allocations of the early electric companies might shed some interesting light on the contribution of the electric interurban to the solvency of a major modern industry). It would be worth while to have information concerning the losses to the overall economy from the drainage of capital into a venture that turned out to be such an unfortunate one. Lastly, the activities of the New York Central

and the Pennsylvania Railroad executives and the others who joined them in supporting the Section 19 rule might have been examined.

In the chapters dealing with the suspension of operations in a declining industry, the authors undertook some very worth-while research. The early demise of the electric interurban is partly due to operating primarily to haul passengers. This least-profitable type of rural local service necessitated too great an expense of time and manpower for too little return. The decision to raise rates decreased total revenue as the demand for service shifted away more than proportionately. Hence, an organized effort to encourage freight haulage was made. But freight traffic did not prove adequate to provide satisfactory profits and the need for additional revenue continued. The failure to make any adequate analysis of demand plagued the industry throughout its short life. The interurban companies could and did raise the funds necessary to finance the construction and operation of an extensive network; but they neglected to probe customer identity and needs, so that the authors found information as to these matters scanty. The book also lacks a detailed discussion of the positive contributions of the industry to the economy.

Research on a declining industry is rare and highly valuable. More detailed analysis of the data might have given it deeper import. Nonetheless, the perceptive reader can draw significant conclusions from the factual reporting of the short life history of one specific form of transportation, since modern railroads are themselves presently confronted with some of the same problems that the electric interurban railway faced and failed so dismally to solve during its brief existence in the early years of the twentieth century.

CLINTON WARNE

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Electric Power Regulation in Latin America. By DAVID F. CAVERS and JAMES R. NELSON. Baltimore: The Johns Hopkins Press, 1959. Pp. xx, 279. \$6.00.

The economic and regulatory problems of the electric power industry in Latin America are the subject matter of this study. It is based primarily on materials gathered and analyzed by Marvin S. Fink and his associates in a first-hand study conducted for the Harvard Law School and financed by the International Bank for Reconstruction and Development. Although the research is confined to the five countries of Brazil, Chile, Colombia, Costa Rica, and Mexico, the report reaches conclusions and recommendations for corrective action which are believed to be pertinent to conditions in other Latin-American countries as well as all less-developed countries in the world.

Electric power is indeed a problem industry in the countries studied. While handicapped by a variety of political, social, and economic policies, the focal point on which all the economic problems of the industry converge is that of rates. The rates charged are the final result of regulatory policy and are the prime cause of the ability or inability to finance expansion from internally available funds or from voluntary savings. Rates also influence the levels of electrical consumption. The actual impact of regulation has been to impose

"startlingly low" rates upon most of the suppliers of electric power in comparison with the rates prevailing in the United States. Comparative data in real terms show that in 1929 rates in the Latin American countries studied were higher than in the United States; in 1939 they were about the same, and by 1954 they had declined to a level approximately 60 per cent of rates in the United States. Cost differences might account for this disparity in rates, but a careful analysis of such factors as relative cost of capital, efficiency in generation, and installation and distribution costs yields the conclusion that the United States enjoys a net cost advantage. No technological or economic justification is found for the precipitous downward trend in "real" electrical rates.

The results of maintaining "cheap" electricity are similar in every country. Inadequate earnings by suppliers, both private and public, have produced insufficient investment and an electric service which has been short in supply, irregular, and inconvenient, hence actually damaging to industrial operations and development. Industrial users have been forced to install their own costly and less efficient diesel plants. Suppliers have been forced in some instances to introduce antipromotional rates to discourage consumption by residential customers. Moreover, government ownership has grown rapidly since 1945 owing in part to a preference for public over private ownership but in large measure owing to the power shortages occasioned by the poor climate for private investment. In the case of public suppliers the effective economic cost of chronic underpricing is also being paid in the form of an accentuation of inflationary pressures owing to the creation of new purchasing power by central banks to finance operating deficits and electrical expansion. Moreover, funds and credit devoted to subsidizing electric supply could be better utilized in promoting needed public services like education and sanitation having little or no earnings potential.

Electric utility regulation in the countries studied, patterned on the concepts and procedures prevailing in the United States, is designed chiefly to control the level of rates in accordance with general standards specified by central governments and administered by regulatory commissions. Unlike the situation in most of the United States, government-owned utilities are subjected to the same rate control as private companies. Other differences are that regulatory agencies are usually politically controlled by the executive branch of the government; they have less discretion in the administration of statutory rate formulas; and their decisions are subject to little judicial review. These institutional differences explain in part the failure of regulation to render economic justice to electrical suppliers.

But the basic difficulty, in the opinion of the authors, is the attempt to apply conventional regulatory theory and practice in an economic environment characterized by persistent and rapid rises in operating costs owing to inflation, rising capital costs, depreciation in the foreign exchange value of national currencies, and the inability to achieve compensating economies through improved load factors and new, low-cost plant installations. In these acutely inflationary circumstances, the time-consuming regulatory process can rarely keep pace in adjusting rates to cover rising operating costs, including depreciation, and the capital costs measured by the traditional mode of rate-

base times rate-of-return. Since a considerable share of the capital invested in the electrical utility industry has been acquired from foreign sources (the World Bank and the Export-Import Bank of Washington alone had loaned by the end of 1959, \$536 million and \$242 million, respectively, for private and public electric power development in Latin America), deterioration of foreign exchange rates further complicates the problem of assuring adequate returns to investors. Regulatory "lag" and "attrition" of returns have also been problems in the United States, but they have not been overly serious owing to the lower rate of inflation, continued technological improvement, and the abundance of capital seeking investment.

Much attention is given to suggestions, some of which have been debated and others actually adopted in a few instances in the United States, for remodeling the regulation and financing of the electric industry. The prime objective here is to make the industry self-sustaining and thus able to attract domestic capital. As the authors put it, "What a developing nation needs more than cheap electricity is plentiful electricity. And the best way to get it is to pay what the service costs" (p. 257).

Three major lines of attack are proposed. First, in order to eliminate the adverse effects of regulatory lag upon rates and earnings, automatic compensatory adjustments in rates should be authorized for changes in fuel prices, wage rates, taxes, and foreign exchange rates. Patterned on the fuel clauses commonly found in rate schedules in the United States and also in Brazil and Colombia, the amount and timing of rate changes would be governed by formulas approved in advance. Second, in order to equalize opportunities with unregulated business in attracting capital, periodic adjustments in the capital accounts of private and public electric enterprises should be allowed to reflect significant changes in domestic price levels and foreign exchange rates. This novel proposal would mean the abandonment of the traditional property rate-base, measured either in terms of the inflexible historical cost or the unrealistic reproduction cost. For it would be substituted the capital accounts, revalued each year on the basis of a cost-of-living index. The rate of return applied to the "investment base" would be differentiated in accordance with the debt and equity components of the capital structure and would be sufficiently high to cover adequate dividends and earnings retained for investment. It would not reach the astronomical level required with an original cost rate-base. Adequate depreciation charges, reflecting replacement cost of plant, would be introduced and the reserves created appropriately handled for rate-making purposes. Third, in recognition of the fact that regulatory reforms will not alone suffice to attract capital, it is recommended that new forms of securities be issued containing protections against inflation, that institutional investors be encouraged to acquire electric securities, and that industrial and even residential users be compelled to invest in the industry.

The sorry plight of a basic industry, competently reviewed and analyzed in this volume, calls unquestionably for drastic modifications in public policy. Perceptive of the economic and accounting considerations involved, the authors have designed reforms which should ameliorate the serious situation confronting the Latin American economies as well as foreign investors. As-

suming a willingness to adopt them their workability will depend on the availability of reliable data to guide regulatory authorities, adequate administrative machinery, and the exercise of informed and expert judgment. Should economic conditions in the United States ever require the adoption of such controversial measures, their administration would certainly tax the ingenuity, patience, and capabilities of regulatory agencies and courts.

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The American Petroleum Industry: The Age of Illumination, 1859-1899.

By HAROLD F. WILLIAMSON and ARNOLD R. DAUM. Evanston: Northwestern University Press, 1959. Pp. xvi, 864. \$7.50.

For a generation historians of business have been working on the assumption that the best procedure for writing meaningful histories of industries was first to produce the building blocks and then to use them for erecting the edifice. This method involved writing biographies of key men and firms, along with important special studies. The first volume of *The American Petroleum Industry*, soon to be followed by a second bringing the story up to 1959, proves that the idea was a good one, whether or not other paths to the same goal might have been effective. Williamson and Daum, with the aid of several assistants, have combined the results of previous research on men, companies, topics with new findings in newspapers, trade periodicals, engineering and mining journals, and early encyclopedias into a comprehensive history of the industry prior to the twentieth century. The product is a noteworthy addition to the history of economic and business activity in the United States.

No reader can cavil at the comprehensiveness of the coverage in *The Age of Illumination*. In eight parts, embracing from one chapter each for the prologue and epilogue to eight for Book IV dealing with the formative years 1862-1873, the authors range over the early history of petroleum and illuminants as well as every function of the industry. Problems of transportation and relations with governmental agencies receive their share of attention. While thus casting their nets widely, Williamson and Daum maintain unity in the book as a whole.

Prior to 1900 the major producing areas in the United States were the Appalachian and Lima-Indiana fields. Shortly after that date the output of those two regions was dwarfed by crude oil production in numerous, widely scattered parts of the country. As a consequence, the pattern of competition was to change rapidly in the twentieth century, even before the dismemberment of the Standard Oil Company (New Jersey) in 1911. The struggle for world markets, begun before 1900, was to assume new forms and take on a new significance in the next generation.

The book contains significant new information and interpretations on the development of illuminants prior to 1859 and on the history of technological change in the petroleum industry up to 1899. On both topics the authors have expended considerable effort and have woven their findings meaningfully into the book. Unfortunately, much of the new data on technology, having been extracted from sources not indicating the time and place of first commercial

application, could not be incorporated as effectively into the pattern of competitive practices which form the core of the book as, say, the organization of the Standard Oil Trust and innovations in marketing.

One of the most impressive attributes of this history is its objective and impartial tone. There are no attacks on or defense of either unhampered monopoly or unrestrained competition. In fact, some readers, particularly those interested in the "romance" of the industry, may well be disappointed in the coolness and calmness of the prose. Views of the authors stand in sharp contrast to the extreme statements made by contemporaries in the heat of hectic competition or in condemnation of the Standard Oil combination. The evaluations of Williamson and Daum are based on a wide range of data and are judicious. Even though I disagree with their emphasis on some points, such as the heavy weight they give to transportation in the growth of Standard Oil in contrast to the importance attached to efficient administration in *Pioneering in Big Business*, their position is so well and so calmly supported that I am willing to leave the resolution of the issue to later students of the industry.

In fact, from the point of view of the economic historian the book leaves little to be desired. In lucid prose the authors have effectively synthesized new with old research and writing. They have made efficient use of a large volume of statistics, presenting them in the text as well as in appendixes. Line drawings and pictures are placed strategically near the ideas in the text they are intended to illustrate. The only parts of the book that could have been improved, as far as this reader has perceived to date, is the proofreading of the index and occasional heavy use of passive verbs. The American Petroleum Institute, the sponsor of the study, has every right to be proud of the book.

In closing, it should be pointed out that this history of an industry is the only one of its kind now available. There is no other study of an industry which compares with it either in comprehensiveness or in portrayal of the dynamic process of development. To have no other like it is a reflection on the fraternity of historians and economists interested in economic history and economic growth. Needless to say, the time has long since passed when writers should have produced enough company histories and topical studies on such a basic industry as iron and steel to permit an accurate, all embracing history to be published. It is hoped that other scholars, noting the high value of this synthesis of the history of the petroleum industry, will respond to the challenge.

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Land Economics; Agricultural Economics; Economic Geography; Housing

Energie und Verkehr. By HELLMUTH S. SEIDENFUS. Veröffentlichungen der List Gesellschaft 18. Tübingen: J. C. B. Mohr (Paul Siebeck); Basel: Kyklos-Verlag, 1960. Pp. xv, 225. DM 19.80; paper, DM 16.50.

The contribution to energy economics made in Seidenfus's book is especially valuable in a period of both dramatic changes in the world's supply of energy

and remarkable economic developments in the Common Market area. The author discusses the outlook for energy consumption in the six Common Market countries plus Switzerland and Austria. This area which he calls the *Untersuchungsraum* comprises the core of Central Europe outside the Iron Curtain.

The focal point of Seidenfus's study is the analysis of the impact of changes in energy supply and demand for special forms of energy (*Edelenergie*) on the transportation system. The important discoveries of oil and natural gas in Africa and in new fields of the Near East in combination with rising per-capita incomes in Western Europe—which induce an increased demand for special forms of energy, e.g., electricity, fuel oil and gasoline—necessitate radical innovations in the existing transportation system of Western Europe.

- The estimates of future energy requirements of the eight countries suffer from the weakness of all long-term projections; they are based on the supply situation, consumer preferences and the political ideas of past decades, data subject to radical change during the next fifteen years. Therefore, actual developments between 1959 and 1975 are bound to deviate greatly from projections. Important parts of the author's conclusions, however, seem convincing: his expectation of a decline in shipments of coal and crude oil by railroads and ships, especially along the Rhine; his assumption that price policies protecting the coal industries may only delay but not prevent the rapid growth of pipe-line systems in the area under investigation. Seidenfus seems equally justified in expecting a rapid expansion of refinery construction and in anticipating intensified highway construction around refinery centers to facilitate the transportation of petroleum products.

The author's conclusions with respect to individual countries are less convincing. In the present state of world tension it should not be assumed that Austria's crude oil supply will be supplemented by shipments from Rumania or the USSR. The alternative lies not only in shipments from refineries around Adriatic ports, but also in shipments of crude oil from Western European ports down the Danube and by pipe lines. The location of the new Austrian refinery in Schwechat, near Vienna, is suitable for both domestic crude oil and crude oils imported from East or West.

The last chapter contains a thorough discussion of policy questions. In his analysis of the goals of economic policy Seidenfus accepts the German neoliberal belief in the responsibility of government to promote workable competition. A good application of these principles for the field of energy economics can be found in Wessels' paper "Die Stellung der Energiewirtschaft in der Wettbewerbswirtschaft." Seidenfus is aware of possible conflicts between the goals of national security and maximization of economic efficiency. He also discusses legal problems which have to be solved before pipeline construction in different countries can be co-ordinated in a satisfactory way. A lack of co-ordination of transportation policies in the eight countries would create serious obstacles to economic integration and economic growth.

The style of Seidenfus's book does not make for easy reading. At many points the reader would prefer to pursue economic discussion rather than being overwhelmed by statistical details. A number of maps are useful tools

for the understanding of the location problems; they would be more helpful if they gave a few important place names. The effectiveness of the book could have been greatly increased by a more streamlined presentation. But this statement is not meant to deprecate the stimulating approach to energy economics offered in this study.

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Labor Economics

The Growth of British Industrial Relations. By E. H. PHELPS BROWN. New York: St. Martin's Press; London: Macmillan & Co., 1959. Pp. xxxvii, 414. \$9.50.

This is a beautiful book; so much so that the reviewer is moved to complain about how little opportunity the author gives for dissent or even suggestion for improvement. Not the least of its many virtues are the concise 24-page synopsis at the beginning, and the detailed bibliography at the end.

The subject matter is much broader than the title suggests. Phelps Brown uses the theme of industrial relations as a frame for a picture of the condition of the British working class as it was in 1906-14. Even more, he shows vividly how this condition resulted from the economic evolution of the 19th century. This background material is contained in the first two chapters. Chapter 1 (The Condition of the People) is concerned with the economic condition of the working-class household and Chapter 2 (The Condition of Work) with the utilities and disutilities encountered in earning a living. In the reviewer's judgment, these two chapters are as good an example of socio-economic description, with the necessary flavoring of economic analysis, as can be found anywhere; they must be read to be appreciated.

Though they deserve study on their own account, in the scheme of the book the first two chapters serve merely as background for what follows. Chapter 3 (The Development of Industrial Relations) describes the more important types of employer and worker organizations that arose during the 19th century, and how they interacted. Different types of unions (craft unions, operatives' unions and general unions) are described in historical context. Each of these union types is associated by the author with a particular method of collective bargaining. The chapter goes on to describe the growth of arbitration and conciliation and the sliding scale, and how these instruments of adjusting industrial conflict were dulled by the growth of working-class militancy in the 1880's and '90's.

Chapter 4 (The Development of Public Policy) is a very lucid account of how Britain came to avoid establishing governmental machinery for settling labor disputes and, until 1908, to avoid setting minimum wages except for public employment. Chapter 5 (Institutions and Procedures in 1906) is for its first 8 sections, a description of union and employer organizations as constituted in 1906; the last three sections describe and, to some extent analyze, how collective bargaining sets wages and other terms of employment. They

also point out how little collective bargaining affected working conditions; i.e., only slight attention was paid to grievance procedure.

Chapter 6 is a history of 1906-14 covering both the legislation and the industrial strife of this period. The author's view is that the series of big strikes in the years 1911-14 was not the major threat to the social order that many contemporary observers believed. This is because the unrest, though intense, was confined to only a limited segment of the economy. Also the political difficulties of the period (the Irish Home Rule issue and the status of the House of Lords) and the growing prospect of war made labor intransigence seem more threatening than it now appears in retrospect.

In the final chapter (Sequel and Survey), Phelps Brown interprets and criticizes existing British institutional arrangements as they bear upon industrial relations. He explains the decline in social protest after the first world war, as compared with 1911-14, as due to an improvement in the economic condition of the working class. The improvement in industrial relations since 1914, and especially since 1926, is held to reflect this improvement and also improvement in "on the job" relations between workers and employers. This is true as far as it goes, but the failure of labor militancy to revive despite the mass unemployment of the late 1920's and 1930's requires explanation.

Failure to provide such an explanation is not of itself proper ground for criticism since this later period is beyond the purview of the book. However, since a correct explanation must involve the changing ideas and socio-economic characteristics of British union officials and the internal balance of political forces within Britain, as well as the factors that are discussed, failure to take account of ideological factors and union politics in the earlier period may constitute an error of omission. This is not to deny that Phelps Brown is correct in stressing changes in economic variables as the prime movers of industrial relations; but it may be that abstraction from the effect of the ideology and political situation of the leaders of the labor movement is carried a bit far.

However, since Phelps Brown covers so much in the space of 367 pages of text, it is ungrateful to complain that he does not do still more. In fact, some readers will probably complain about the manner in which this volume compresses socio-economic history. Its extreme economy of presentation is achieved by the author's careful discussion of what he considers important and inattention to the rest. Whether one approves of the result depends upon how wisely he thinks the author has chosen. This reviewer approves thoroughly; he feels that Phelps Brown has taken hold of the principal threads of the history of industrial relations in Great Britain and woven a really magnificent tapestry. Even those readers who will not share this enthusiasm fully cannot help but find the book a provocative essay in socio-economic history.

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TITLES OF NEW BOOKS

General Economics; Methodology

- ALBERTINI, J. M. AND OTHERS. *Les rouages de l'économie nationale*. Paris: Edit. Ouvrières, 1960. Pp. 212. NF 7.50.
- ARROW, K. J., KARLIN, S. AND SUPPES, P. *Mathematical methods in the social sciences*, 1959. Proceedings of the first Stanford Symposium. Stanford math. stud. in soc. sci., no. 4. Stanford: Stanford Univ. Press, 1960. Pp. viii, 365. \$8.50.
- "The objective of this symposium was to present in an effective way some areas of current research in which mathematical methods are being used to attack problems in social and management sciences." There are nine papers on economics, "Efficiency of resource allocation, stability, capital accumulation and technological change, and consumer's behavior are some of the topics covered." (From the preface.)
- CLARK, F. G. AND RIMANOCZY, R. S. *How we live: a simple dissection of the economic body*. 2nd ed. Princeton: D. Van Nostrand, 1960. Pp. 89. \$2.85.
- GALBRAITH, J. K. *The liberal hour*. Boston: Houghton Mifflin, 1960. Pp. xii, 197. \$3.50.
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- A collection of essays by Carl Menger, F. F. von Wieser, Ludwig Pohle, Ludwig von Mises, Paul Painlevé, Jacques Rueff, and Luigi Einaudi.
- STAMMER, O. AND THALHEIM, K. C., ed. *Festgabe für Friedrich Bülow zum 70. Geburtstag*. Berlin: Duncker & Humblot, 1960. DM 44.80.
- Proceedings of the Business and Economic Statistics Section, American Statistical Association, 1959. Washington: Am. Stat. Assoc., 1960. Pp. 319.
- Seconds mélanges d'économie politique et sociale offerts à Edgard Milhaud. Thème: l'économie collective. Liege, Belgium: C.I.R.I.E.C., 1960. Pp. 300. German edition also available from same publisher. Either edition separately, Bfr. 150. Both editions together, Bfr. 250.

Price and Allocation Theory; Income and Employment Theory; Related Empirical Studies; History of Economic Thought

- AMBIRAJAN, S. *Malthus and classical economics*. Bombay: Popular Book Depot, 1960. Pp. 212. Rs 12.50.
- COLSON, CL. *Textes choisis*. Paris: Dalloz, 1960. Pp. 354. NF 15.
- DAVIDSON, P. *Theories of aggregate income distribution*. New Brunswick, N.J.: Rutgers Univ. Press, 1960. Pp. x, 151. \$5.

A review of "the major theoretical attempts to explain the distribution of the total product of an economy into proportionate shares which accrue to the various economic classes." Starting with the classical and neoclassical theories (including Marx), the survey goes on to examine the Cobb-Douglas production function, theories based on monopoly (Kalecki, Mitra), aggregate demand theories (Keynes, Boulding, Robinson, Kaldor), and aggregate supply (Weintraub).

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EITEMAN, W. J. *Price determination in oligopolistic and monopolistic situations*. Michigan Bus. rept., no. 33. Ann Arbor: Bur. Bus. Research, School Bus. Admin. Univ. of Michigan, 1960. Pp. v, 45.

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- ARTLE, R. Studies in the structure of the Stockholm economy: towards a framework for projecting metropolitan community development. FFI report no. 57. Stockholm: Business Research Inst., Stockholm School of Economics, 1959. Pp. 207. Sw. kr. 20.
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The book is concerned with two groups of countries—the poor and the rich—and with the relations between them. "This book is designed to explore the relationships of events, to demonstrate that the assumptions on which policies are at present based are often inappropriate, and that the steps taken on the basis of false theories worsen rather than improve the situation. We will see that the attainment of economic growth cannot usually be ensured by 'economic actions' but requires the existence of certain social attitudes to work and leisure, consumption and saving, investment and innovation." (Pp. 10-11.) The book is primarily addressed to the general reader.

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Related Disciplines

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NOTES

Members who wish to make suggestions for officers of the American Economic Association for 1961-62 are invited to place names with James Washington Bell, secretary of the Association, for transmission to the Nominating Committee, which will be appointed by the incoming president, Paul Samuelson.

Announcements

The Survey Research Center of The University of Michigan announces the second summer workshop on the use of consumer survey data for teachers of economics and business administration to be held from June 19th to July 15th to coincide with the first four weeks of the 14th Annual Institute in Survey Research Techniques. Mature scholars and applicants with Ph.D. degrees will receive preference although some selections may be made from advanced graduate students. Some fellowship funds will be available.

For additional information write to Mrs. Judith Agard, Survey Research Center, The University of Michigan, Ann Arbor, Michigan, or see the printed program of the Christmas meetings in St. Louis for an announcement of an informational meeting where individual questions about the archival resources of the Survey Research Center may be answered together with inquiries concerning the nature of the workshop.

A graduate program in Latin-American studies has been instituted at Louisiana State University. The program offers the Master of Arts degree with the following areas of concentration: anthropology, economics, geography, government, history, sociology and Latin-American literature. Inquiries should be addressed to Graduate Program in Latin American Studies, 150 Himes Hall, Louisiana State University, Baton Rouge 3, Louisiana.

The Bureau of Public Health Economics of the School of Public Health, University of Michigan, has established a program of graduate training for research in medical care. The program is offered in conjunction with the bureau's research program on social factors associated with the provision of health services for the aged which has been initiated under a five-year grant from the Ford Foundation. Academic appointments as research assistant are available to a limited number of students in economics and several other social sciences. Those interested in applying should write to Dr. S. J. Axelrod, Director, Bureau of Public Health Economics, School of Public Health, University of Michigan, Ann Arbor, Michigan. Applications must be submitted before February 1, 1961 to be acted on for the fall semester.

The International Conference on Input-Output Techniques will be held at the Palais des Nations, Geneva, from September 11 through 15, 1961, rather than during the week of August 28 as previously announced. Economists and statisticians interested in attending should write Dr. Elizabeth W. Gilboy, Secretary General, at the Harvard Economic Research Project, 1583 Massachusetts Avenue, Cambridge 38, Massachusetts.

Deaths

Clare W. Barker, Indiana University, July 6, 1960.

Alexander M. Bing, November 1959.

Howard R. Delancy, Ohio Oil Company, October 1959.

Bernard W. Dempsey, Marquette University, July 23, 1960.

Lloyd E. Dewey, New York University, March 24, 1960.

Yehuda Grunfeld, Hebrew University, Jerusalem, July 16, 1960.

B. B. Holder, Washington and Lee University.

George Kleiner, University of Illinois, July 12, 1960.

Sanford A. Mosk, University of California, Berkeley.

Cyrus K. Oakley, April 1960.

John J. Walsh, Catholic University of America, December 16, 1959.

W. S. Woytinsky, Twentieth Century Fund, June 1960.

Retirements

David H. Colin, Washington Square College, New York University, September 1960.

A. Anton Friedrich, Washington Square College, New York University, September 1960.

Bruce W. Knight, Dartmouth College, June 1960.

Earl E. Muntz, New York University, September 1960.

Alva L. Prickett, Indiana University.

Earl R. Sikes, Dartmouth College, June 1960.

Shirley D. Southworth, College of William and Mary.

Visiting Foreign Scholars

Daphne Y. Bell, University of Otago, New Zealand: lecturer in economics, Boston College.

Douglas G. M. Dossier, University of Edinburgh: visiting associate professor, University of Washington, 1960-1961.

Francesco Forte, University of Urbino, Italy: visiting associate professor of economics, University of Virginia, fall term, 1960.

Herbert Geyer, J. W. Goethe Universitaet, Frankfurt: visiting lecturer, department of economics, Southern Methodist University.

Gideon Rosenbluth, Queen's University, Kingston: visiting associate professor, University of Washington, spring term, 1961.

S. Tsuru, Tokyo University: visiting professor of economics, University of Rochester, spring semester, 1961.

Promotions

Mark A. Alexander: assistant professor of industry, School of Business Administration, University of Pittsburgh.

Vance Q. Alvis: professor of economics, West Virginia University.

George R. Anderson: associate professor of economics, University of Michigan.

A. Harvey Belitsky: assistant professor, department of economics, Rutgers, The State University.

Monroe Berkowitz: professor of economics, Rutgers, The State University.

Edwin Bishop: assistant professor of economics, Georgetown University.

Rudolph C. Blitz: associate professor of economics, Vanderbilt University.

Harvey E. Brazer: professor of economics, University of Michigan.

Dwight S. Brothers: associate professor of economics, Rice University.

John A. Buttrick: professor of economics, University of Minnesota.

John S. Chipman: professor of economics, University of Minnesota.

Eaton H. Conant: assistant professor of management, School of Business, Indiana University.

Lev Dobriansky: professor of economics, Georgetown University.

William L. Doremus: professor of marketing, School of Commerce, New York University.

Wallace I. Edwards: associate professor of economics, Miami University, Oxford, Ohio.

Charles E. Ferguson: associate professor, department of economics and business administration, Duke University.

Seymour Fiekowsky: associate professor of economics, Los Angeles State College.

James M. Folsom: assistant professor, department of economics and business administration, Duke University.

Samuel Frumer: assistant professor of accounting, School of Business, Indiana University.

Leonard J. Garrett: lecturer in industry, Wharton School, University of Pennsylvania.

Francis W. Gathof, Jr.: assistant professor of economics, The American University.

Gerald J. Glasser: associate professor of economics, New York University.

Zvi Griliches: associate professor of economics, University of Chicago.

Walter W. Haines: professor of economics, University College, New York University.

Charles M. Hewitt: professor of business law, School of Business, Indiana University.

Eugene C. Holshouser: assistant professor of economics, Bureau of Business Research, University of Kentucky.

Masaharu Inaba: assistant professor of economics, Hofstra College.

Thomas S. Isaack: professor of management, West Virginia University.

Arcadius Kahan: assistant professor of economics, University of Chicago.

Laura L. Karadibil: assistant professor of accounting, School of Business Administration, The American University.

Edward J. Kuntz: associate professor of business administration, School of Business, Indiana University.

Kenneth K. Kurihara: professor of economics, Rutgers, The State University.

Ernest Kurnow: professor of economics, New York University.

Harry H. Landreth: assistant professor of economics, Miami University, Oxford, Ohio.

Robert W. Lentilhon: associate professor of accounting, School of Business Administration, University of Massachusetts.

Albert Levenson: assistant professor of economics, Hofstra College.

Solomon Levine: professor of labor and industrial relations, Institute of Labor and Industrial Relations, University of Illinois.

Robert E. Lewis: assistant vice president, The First National City Bank of New York.

James B. Ludtke: professor of finance, School of Business Administration, University of Massachusetts.

Ludwig H. Mai: professor of economics, St. Mary's University, San Antonio, Texas.

E. Scott Maynes: associate professor, University of Minnesota.

William G. McDonald, Colonel, U.S.A.F.: assigned to Director of Management Analysis, Hq. U.S.A.F.

James W. McKie: professor of economics and business administration, Vanderbilt University.

William J. McKinstry: associate professor of economics, Miami University, Oxford, Ohio.

Paul Medow: assistant professor, department of economics, Rutgers, The State University.

Eva L. Mueller: associate professor of economics, University of Michigan.

Douglass C. North: professor of economics, University of Washington.

William B. Palmer: professor of economics, University of Michigan.

John H. Porter: associate professor of business administration, School of Business, Indiana University.

Elizabeth R. Post: lecturer in business administration, Vanderbilt University.

Harry Rosenthal: associate professor of accounting, School of Business Administration, The American University.

Gunther H. Ruff: associate professor of economics, Georgetown University.

I. Richard Savage: professor of economics and statistics, University of Minnesota.

Sayre Schatz: associate professor of economics, Hofstra College.

Mark B. Schupack: assistant professor of economics, Brown University.

Lloyd V. Seawell: associate professor of accounting, School of Business, Indiana University.

Barbara A. Simpson: assistant professor of economics, College of William and Mary.

Edward K. Smith: associate professor of economics, Boston College.

Babette Solon: assistant professor of economics, Hofstra College.

Anthony M. Tang: associate professor of economics and business administration, Vanderbilt University.

James N. Tattersall: assistant professor of economics, University of Oregon.

Lynn Turgeon: assistant professor of economics, Hofstra College.

C. Edward Weber: associate professor of industry, School of Business Administration, University of Pittsburgh.

Richard C. Wilcock: professor of labor and industrial relations, Institute of Labor and Industrial Relations, University of Illinois.

Edgar G. Williams, professor of management, School of Business, Indiana University.

Gustavus G. Williamson, Jr.: associate professor, department of economics, University of South Carolina.

Wallace O. Yoder: professor of marketing, School of Business, Indiana University.

Administrative Appointments

Robert L. Allen: assistant director, Institute of International Studies and Overseas Administration, University of Oregon.

Monroe Berkowitz: chairman, department of economics, Rutgers, The State University.

Walter Buckingham: director, School of Industrial Management, Georgia Institute of Technology.

Thomas H. Carroll, Ford Foundation: president, George Washington University; also professor of economic development and administration.

Philip W. Cartwright: associate dean of arts and sciences, University of Washington.

J. E. Chace, University of Florida: head, department of finance, insurance and real estate, University of Arizona.

Hibbert D. Corey: acting chairman, department of economics, College of William and Mary.

Henry M. Cunningham: assistant dean and associate professor, School of Business Administration, The American University.

Leon A. Dale, University of Wisconsin: chairman and associate professor, department of industrial relations, University of Bridgeport.

Edward E. Edwards: acting chairman, department of finance, School of Business, Indiana University, 1960-61.

John P. Gill: chairman and professor, department of business statistics, School of Commerce and Business Administration, University of Alabama.

J. Benton Gillingham: acting executive officer and associate professor of economics, University of Washington.

Paul V. Grambsch, Tulane University: dean and professor, School of Business Administration, University of Minnesota.

William L. Haerberle: director, Indiana Management Institutes, School of Business, Indiana University.

Bernard Hall: director, Bureau of Business Research and Service, and associate professor, College of Business Administration, Kent State University.

Lloyd F. Hayn: undergraduate dean, Keene Teachers College, New Hampshire.

E. Gordon Keith: associate dean, Wharton School, University of Pennsylvania.

• Irving O. Linger: chairman and professor, department of economics, Texas College of Arts and Industries, Kingsville, Texas.

Carl H. Madden: dean, College of Business Administration, Lehigh University.

W. David Maxwell: chairman, department of economics, School of Business Administration, Tulane University.

John B. Minick: chairman and associate professor, department of economics, Marshall College.

James A. Morris: director, Bureau of Business and Economic Research, University of South Carolina.

Schuyler F. Otteson: chairman, department of marketing, School of Business, Indiana University.

David H. Pollock: chief, U.N. Economic Commission for Latin America, Washington group.

Marshall A. Robinson: dean, School of Business Administration, University of Pittsburgh.

Gerhard N. Rostvold: chairman, department of economics, Pomona College.

Howard G. Schaller: dean, School of Business Administration, Tulane University.

Charles J. Stokes: chairman, department of economics, University of Bridgeport.

W. E. Stone, University of Pennsylvania: head, department of accounting, University of Florida.

Ben B. Sutton, University of Minnesota: vice president, Apache Oil Company.

James A. S. Ternent: assistant director, Center of Studies for Economic Development, University of the Andes, Bogotá, Columbia.

Pinkney C. Walker: chairman, department of economics, University of Missouri.

Weldon Welfling, Simmons College: chairman and professor, banking and finance department and department of economics, Western Reserve University.

Samuel G. Wennberg: chairman, department of business management, University of Missouri.

Jack R. Wentworth: acting director, Bureau of Business Research, Indiana University.

Charles R. Whittelsey: chairman, finance department, Wharton School, University of Pennsylvania.

W. Donald Wood: director, industrial relations centre, and associate professor of economics, Queen's University, Canada.

Herbert B. Woolley, New York University: manager, economics department, California-Texas Oil Corporation.

Appointments

Carol S. Adams: instructor in economics, University of Michigan.

Leon Agranat: associate professor of finance, School of Business Administration, The American University.

Leonall C. Andersen: instructor, School of Business Administration, University of Minnesota.

Leslie P. Anderson: assistant professor of finance, University of Arizona.

Albert K. Ando, Massachusetts Institute of Technology; special visiting lecturer in econometrics, Boston College.

Louis E. Andrade: instructor, School of Business Administration, University of Minnesota.

Henry B. Arthur, Swift & Co.: Moffett professor, School of Business Administration, Harvard University.

Ludwig Auer: research associate, department of economics, Iowa State University (Ames).

Eliezer B. Ayal, Cornell University: assistant professor of economics, University of Michigan.

Vladimir N. Bandera, University of California: assistant professor of economics, Boston College.

Hubert E. Bice: visiting professor of marketing, Lehigh University.

George H. Bickle: instructor in marketing, Lehigh University.

Lloyd C. Billings: lecturer in economics and business administration, University of Arizona.

Eugene A. Brady: instructor in economics, Iowa State University (Ames).

John W. Brand: instructor, School of Business, University of Kansas.

Gerard Brown: lecturer in economics, Georgetown University.

George J. Burak: assistant professor of business, School of Business Administration, University of Massachusetts.

V. E. Cangelosi: assistant professor of business administration, University of Arkansas.

Lon C. Cesal: research associate, department of economics, Iowa State University (Ames).

Jung Sic Chai: instructor, School of Business Administration, University of Minnesota.

Richard B. Chalecki: instructor, department of economics, Washington and Jefferson College.

Kenneth E. Cook: research associate, Bureau of Business Research, University of Kentucky.

Edwin B. Cox, University of Pennsylvania: assistant professor of business administration, Boston University.

Boyd C. Culley: instructor, department of economics, Washington and Jefferson College.

John G. Cummins, Johns Hopkins University: assistant professor of economics, School of Business Administration, Tulane University.

George Dalton: assistant professor of economics, Bard College.

Earl F. Davis: instructor, business law department, School of Commerce and Business Administration, University of Alabama.

James L. Davis: instructor, department of marketing, School of Commerce and Business Administration, University of Alabama.

Jim E. Davis: instructor, department of economics, University of Illinois.

Otto Davis: assistant professor of economics, Carnegie Institute of Technology.

Nicholas J. Demerath: Ford Foundation visiting professor of business administration, School of Business, Indiana University.

Jeanne E. Dost: instructor in economics, Washington State University.

Martin W. Duffy: lecturer, Industrial Relations Center, University of Minnesota.

J. W. Dunlap: instructor in economics, College of Business Administration, University of Arkansas.

Richard M. Duvall: instructor, department of economics and business administration, Duke University.

Fred Dziadek: lecturer, department of economics, Georgetown University.

Arakkal T. Eapen: instructor in economics, University of Michigan.

James D. Emery, University of Chicago: instructor in economics, West Virginia University.

• Stephen Enke: visiting professor of economics, first semester 1960-61, Southern Methodist University; Cape Town University, following semester.

Lou Esplund: instructor, School of Business, University of Kansas.

Walter D. Fackler: associate professor of business economics, Graduate School of Business, University of Chicago.

Noel J. J. Farley: instructor in economics, Boston College.

Martin A. Faulkner: instructor in business administration, Washington State University.

Allen Ferguson, Northwestern University: research economist, The RAND Corporation.

Max E. Fieser: instructor in economics, University of Oregon.

Edward M. Foster, Massachusetts Institute of Technology: special visiting lecturer in mathematical economics, Boston College.

John M. Frikart: assistant professor of economics, University of Arizona.

Tom Geraghty: assistant professor, department of economics and business administration, University of Chattanooga.

A. J. Giunta, Western Maryland College: associate professor, department of business administration, University of Scranton.

Frederick J. Glover: Fulbright visiting associate professor of economics, College of William and Mary.

Jon E. Goldstein: instructor, School of Business Administration, University of Minnesota.

Larry E. Greiner: instructor, School of Business, University of Kansas.

Edward Gross: professor of industrial relations, University of Minnesota.

Judith Grouse: assistant professor, department of economics, Syracuse University.

Ernest W. Grove: agricultural economist, Commodity Stabilization Service.

Dale O. Gustafson: instructor, School of Business Administration, University of Minnesota.

Harold Guthrie: associate professor of economics, University of Kentucky.

Burton C. Hallowell, Wesleyan University: staff of Commission on Money and Credit.

Arthur A. Halper: assistant professor, department of economics, Brooklyn College.

Paul W. Hamelman, University of Pittsburgh: assistant professor of management, West Virginia University.

Richard W. Hansen: instructor, School of Business Administration, University of Minnesota.

William L. Henderson, Ohio State University: assistant professor, department of economics, Denison University.

Harvey S. Hendrickson: instructor, School of Business Administration, University of Minnesota.

John Henning: lecturer in economics, College of Arts and Sciences, Rutgers, The State University.

Irwin L. Herrstadt: assistant professor of economics, Northeastern University.

James G. Hilton: assistant professor of economics, University of South Carolina.

Abraham Hirsch, William and Mary College: associate professor of economics, Brooklyn College.

Jack Hirschleifer: associate professor, department of economics, University of California, Los Angeles.

Ira Horowitz: assistant professor of business administration, School of Business, Indiana University.

J. K. Horsefield: staff of International Monetary Fund.

Clarence J. Huizenga: acting assistant professor of business economics, Graduate School of Business Administration, University of California, Los Angeles.

Charles W. Hultman: assistant professor of economics, School of Diplomacy and International Commerce, University of Kentucky.

Sol Jacobson: associate professor, department of economics, Brooklyn College.

Harry L. Johnson: associate professor of finance, School of Commerce and Business Administration, University of Alabama.

Ole S. Johnson: professor of marketing, School of Business Administration, The American University.

George M. Jones: economic analyst, Western Pacific Railroad Company.

Edward J. Kane: assistant professor, department of economics, Iowa State University (Ames).

Jerzy F. Karcz: assistant professor of economics, University of California, Santa Barbara.

Donald W. Katzner: instructor, School of Business Administration, University of Minnesota.

Seymour Kaufman: associate professor of accounting, School of Business Administration, The American University.

Martin L. King, University of Kentucky: associate professor of marketing, School of Business Administration, The American University.

Ramon Knauerhase: instructor in economics, Lehigh University.

Anthony T. Krzystofik: assistant professor of accounting, School of Business Administration, University of Massachusetts.

Ching-wen Kwang: associate professor of economics and accounting, Northeastern University.

John LaTourette: instructor in economics, College of Arts and Sciences, Rutgers, The State University.

Joseph Lazar: professor of industrial relations and business administration, University of Minnesota.

Marvin Lee: instructor, department of economics, Hofstra College.

Herbert S. Levine: lecturer in economics, University of Pennsylvania.

Irwin M. Levine: instructor in business administration, Emory University.

Orlando Lobo, Georgetown University: staff of the International Monetary Fund.

Millard F. Long: assistant professor of economics, Vanderbilt University.

Michael C. Lovell: visiting lecturer in economics, Wesleyan University.

Ira Lowry: assistant professor of economics, Carnegie Institute of Technology.

Fang-hwa Mah: assistant professor of economics, Los Angeles State College.

Maurice Mann: senior economist, Federal Reserve Bank of Cleveland.

Jacob Marschak: professor of business economics, Graduate School of Business Administration, University of California, Los Angeles.

Fernand Martin: visiting assistant professor of economics, department of economics and political science, University of Saskatchewan.

Mildred Massey: assistant professor of economics, Los Angeles State College.

Thomas R. Masterson, De Paul University: associate professor of business administration, Emory University.

William A. Mauer, Duke University: assistant professor of economics, School of Business Administration, Tulane University.

Morris L. Mayer: assistant professor of marketing, School of Commerce and Business Administration, University of Alabama.

Donald McClurg: assistant professor, department of economics, University of Colorado.

Robert M. McInnis: instructor in political economy, department of economics and political science, University of Saskatchewan.

Erskine W. McKinley, Rockefeller Foundation: associate professor of economics, School of Business Administration, Tulane University.

John N. McKinney, University of California, Berkeley: instructor, department of economics, Washington Square College.

Ronald I. McKinnon: lecturer, department of economics, Syracuse University.

Richard L. Meyer: instructor, School of Business Administration, University of Minnesota.

Charles L. Mills: instructor in business administration, Washington State University.

David N. Milstein, Resources for the Future: lecturer in economics, University of Michigan.

Jora Minasian: research economist, The RAND Corporation.

Robert A. Minick, University of Texas: assistant professor, Southeast Missouri State College.

Michael Mischaikow, Indiana University: assistant professor of economics, West Virginia University.

Richard W. Molten, University of North Carolina: assistant professor of economics, University of South Carolina.

Richard E. Neel: assistant professor of economics, College of William and Mary.

Duane C. Nichols: instructor, School of Business, University of Kansas.

Thomas O. Nitsch, Ohio State University: assistant professor of economics, Creighton University.

Marion M. Nobel: instructor in economics, University of Michigan.

Gerald L. Nordquist: assistant professor of economics, State University of Iowa.

Richard L. Norgaard: instructor, School of Business Administration, University of Minnesota.

Jack N. X. Oanh, Trinity College: International Bank for Reconstruction and Development.

Alvan J. Obelsky: lecturer in economics, University of Michigan.

Arthur J. O'Neal, Jr.: instructor in economics, Lehigh University.

Frank Palalay, Defiance College: assistant professor of economics, Wisconsin State College.

Harry M. Palmer: industrial program specialist, Army Rocket and Guided Missile Agency, Redstone Arsenal, Alabama.

James Parthemos, Tulane University: research department, Federal Reserve Bank, Richmond, Virginia.

James M. Patterson: assistant professor of marketing, School of Business, Indiana University.

Joseph A. Pechman, Committee for Economic Development: executive director of a special research project, Brookings Institution.

Daniel S. Pearl: instructor, School of Business Administration, University of Minnesota.

Benedict S. Pedrotti: instructor in economics, Lehigh University.

David R. Pender: assistant professor of economics and research economist, School of Business Administration, University of South Carolina.

Harold A. Peterson, College of Wooster: instructor in economics, Boston College.

Doris G. Phillips: assistant professor of economics, Washington State University.

Richard W. Poole: assistant professor, department of economics, Oklahoma State University.

Robert Rice: instructor, department of economics, Hofstra College.

M. Luella Richey: visiting professor of accounting, School of Business Administration, University of Miami.

Jennie Richmond: instructor, department of economics, Boston University.

Robert L. Rizek, USDA Regional Marketing Coordinator: department of economics, Iowa State University (Ames).

Stefan H. Robock: associate professor, School of Business, Indiana University.

Charles E. Rockwood: assistant professor of economics, Florida State University.

William F. Saalbach: assistant professor, department of economics, Washington and Jefferson College.

Thomas R. Saving: visiting assistant professor, department of economics, University of Washington.

David Schwartzman, New York University: associate professor of economics, New School of Social Research.

Kap-kyung Seo: instructor in economics, University of Hawaii.

George D. Shelby: associate professor of economics, Northeastern University.

John P. Shelton: visiting associate professor of finance, Graduate School of Business Administration, University of California, Los Angeles.

Albert J. Simone: instructor in economics, Northeastern University.

Irving A. Sirken: chief economist, Argentine Transport Study, International Bank for Reconstruction and Development.

Larry A. Sjaastad, University of Chicago: lecturer in economics, University of Minnesota.

Melvin D. Skold: research associate, department of economics, Iowa State University (Ames).

J. Graham Smith, Ohio State University: instructor, department of economics, University of Florida.

Karl U. Smith: Ford Foundation visiting professor of business administration, Indiana University, spring term, 1961.

Leon Smolinski: assistant professor of economics, Boston College.

Milton Sobel: assistant professor of statistics and economics, University of Minnesota.
Arnold M. Soloway, Harvard University: visiting professor of public finance, Boston College.

Duane L. Sorensen: research associate, department of economics, Iowa State University (Ames).

Dufferin S. Spafford: instructor, department of economics and political science, University of Saskatchewan.

W. Allen Spivey: lecturer in economics, University of Michigan.

Byron E. Springer: instructor, School of Business, University of Kansas.

George Staller: assistant professor of economics, Cornell University.

Joseph L. Steele: assistant professor of economics, Memphis State College.

Kenneth Strand, Washington State University: assistant professor of economics, Oberlin College.

Adolf F. Sturmthal, Roosevelt University: professor of labor and industrial relations, University of Illinois Institute of Labor and Industrial Relations.

Robert B. Sweeney: associate professor of accounting, School of Commerce and Business Administration, University of Alabama.

Paul M. Sweezy: visiting professor of economics, Stanford University, winter and spring quarters, 1960-61.

Lawrence X. Tarpey: assistant professor of marketing, University of Kentucky.

Amos E. Taylor, Organization of American States: professor of economics, The American University.

Peter R. Toscano: assistant professor of economics, Lake Forest College.

C. P. Tseng: associate professor of economics, Emory University.

Heinz Vergin: instructor, School of Business Administration, University of Minnesota.

George J. Viksnins: instructor in economics, Hood College, Maryland.

Donald A. Walker, Boston University: assistant professor of economics, Boston College.

Larkin B. Warner: assistant professor, department of economics, Oklahoma State University.

Stanislaw Wasowski: assistant professor of economics, Georgetown University.

Eleanor Weber: instructor in economics, University Heights, New York University.

Murray L. Weidenbaum: lecturer in economics, University of Washington, spring quarter.

William H. Wesson, Jr., Louisiana State University: professor of economics, School of Business Administration, University of South Carolina.

David A. West: associate professor of economics, Carson-Newman College.

Donald L. Westerfield: instructor in statistics, Wharton School, University of Pennsylvania.

Walter Williams: assistant professor of insurance, School of Business, Indiana University.

Edward L. Winn, Jr., Indiana University: assistant professor of finance, School of Business Administration, University of South Carolina.

Pan A. Yotopoulos, University of California, Los Angeles: instructor in economics, University of Wisconsin-Milwaukee.

Laszlo Zsoltos: assistant professor of economics, School of Diplomacy and International Commerce, University of Kentucky.

Leaves for Special Appointments and Assignments

Michael Albery, Boston College: assignment to Guatemala, lecturing in organization and management.

Francis Bator, Massachusetts Institute of Technology: The RAND Corporation, 1960-61.

J. Homer Blackstone, Auburn University: agricultural economics consultant, U.S. Study Commission, Southeast River Basins, Atlanta, Georgia.

Clark C. Bloom, State University of Iowa: Ford Foundation program, Jordan.

Alpha C. Chiang, Denison University: visiting professor, New Asia College, Hong Kong.

John S. Chipman, University of Minnesota: International Cooperation Administration, Chile, 1960-61.

John R. Coleman, Carnegie Institute of Technology: consultant, Ford Foundation, India, 1960-61.

Seymour Fiekowsky, Los Angeles State College: economic analyst, U.S. Outdoor Recreation Resources Review Commission.

Victor R. Fuchs, New York University: Ford Foundation economic development program, 1960-61.

Robert H. Johnson, University of Iowa: senior specialist, Brookings Institution Economic Specialists Group, Saigon, under grant from the Ford Foundation.

A. D. H. Kaplan: special research appointment, Brookings Institution.

Richard L. Kozelka, University of Minnesota: consultant, International Cooperation Administration, Nicaragua, fall 1960; visiting professor, University of Florida, spring term 1961.

D. Philip Locklin, University of Illinois: mission to study transportation needs of Argentina, 1960-61.

C. Ward Macy, University of Oregon: consultant, Fiscal Commission of Jordan, summer 1960.

Carl C. Malone, Iowa State University (Ames): Ford Foundation food production program, India, 1960-61.

Richard Nelson, The RAND Corporation: Carnegie Institute of Technology, 1960-61.

Lawrence Pasel, Franklin College, Indiana: visiting professor of economics, Muhlenberg College.

Richard Phillips, Iowa State University (Ames): International Cooperation Administration, Nigeria, fall 1960.

Harry Rowen, The RAND Corporation: Center for International Affairs, Harvard University, 1960-61.

Walter S. Salant, Brookings Institution: consultant, social sciences program, Rockefeller Foundation, 1960-61.

I. Richard Savage, University of Minnesota: Harvard Graduate School of Business, 1960-61.

Harold M. Somers: visiting professor, department of economics, University of California, Los Angeles, 1960-61.

George W. Stocking, Vanderbilt University: American University of Beirut, Lebanon, 1960-61.

S. C. Tsiang, International Monetary Fund: visiting professor of economics, University of Rochester.

Festus J. Viser, New York University: Econometric Institute, University of the Netherlands, 1960-61.

Andrew M. Watson, University of Toronto: Economic Advisory Committee, Jordan, 1960-61.

Willis D. Weatherford, Swarthmore College: advisor on community development, Government of Malaya.

Leonard W. Weiss, San Jose State College: visiting associate professor of economics, University of Minnesota, 1960-61.

Richard C. Wilcock, University of Illinois Institute of Labor and Industrial Relations: visiting professor, University of California, Los Angeles, fall semester 1960.

Clair Wilcox, Swarthmore College: American Seminar, Salzburg; Harvard Advisory Group, Teheran, 1960.

Resignations

Joseph B. Black, Jr.: Indiana University.

G. Diran Bodenhorn: Graduate School of Business, University of Chicago.

Paul Davidson: Rutgers, The State University.

Richard W. Graves: Indiana University.

Samuel Laimon: Graduate School of Business, University of Chicago.

Miscellaneous

Vincent W. Bladen, University of Toronto: appointed a one-man Royal Commission to inquire into the automobile industry.

Harry G. Johnson, University of Chicago: appointed editor, *Journal of Political Economy*.

VACANCIES AND APPLICATIONS

The Association is glad to render service to applicants who wish to make known their availability for positions in the field of economics and to administrative officers of colleges and universities and to others who are seeking to fill vacancies.

The officers of the Association take no responsibility for making a selection among the applicants or following up the results. The Secretary's Office will merely afford a central point for clearing inquiries; and the *Review* will publish in this section brief description of vacancies announced and of applications submitted (with necessary editorial changes). Since the Association has no other way of knowing whether or not this section is performing a real service, the Secretary would appreciate receiving notification of appointments made as a result of these announcements. It is optional with those submitting such announcements to publish name and address or to use a key number. Deadlines for the four issues of the *Review* are February 1, May 1, August 1, and November 1.

Communications should be addressed to: The Secretary, American Economic Association, Northwestern University, Evanston, Illinois.

Vacancies

International Cooperation Administration: This Administration has several openings at the moment and anticipates additional vacancies from time to time for well-qualified economists interested in positions overseas. The positions are of two types. Some are for advisors in special areas, such as finance, taxation, and industry. Others are for generalists to make continuing analyses of a country's economy to help guide the planning and execution of the foreign aid program for the country. Both types of positions require economists with sound theoretical background and experience in research or applied practice. The minimum tour is for two years at the foreign post, but employees who make good are encouraged to make a career of the service. Candidates must have been citizens of the United States for at least five years. The candidate and all dependents who will reside at the foreign post must pass complete physical examinations. Preferred ages are between 32 and 55. Base salaries for the vacancies that most frequently arise range from about \$8,000 to \$14,000. There are also a number of fringe benefits, including housing, medical care, educational allowances for children, differentials ranging to 25 per cent for some hardship posts, and home leave between tours. If interested—whether or not immediately available—send a résumé or preferably the standard U. S. Government Employment Application Form (Form 57) to: Office of Personnel, International Cooperation Administration, Box ER-2, Washington 25, D.C. Applications will be held confidential.

Economist: A leading financial institution offers opportunity for an economist with graduate degree, doctorate preferred, and a strong background in money and banking, forecasting, and national income analysis. This position requires the ability to conduct independent research on a wide variety of problems bearing on Company operations. Starting salary \$7,300-\$8,500, depending on education and experience. Please send résumé giving full account of professional background. All replies will remain strictly confidential. P230

Economist: Opening June 1 or September 1, 1961, for a rapidly growing state college located in beautiful southern West Virginia. Doctor's degree required. Division of Business staff consists of eleven young faculty members and an enrollment of more than 300 majors. Concord College is accredited by the North Central Association of Colleges and Secondary Schools and the National Council for Accreditation of Teacher Education. Apply to Dr. Cloyd P. Armbrister, Chairman and Professor, Division of Business, Concord College, Athens, West Virginia.

Principles, economic theory and/or geography, statistics: Pennsylvania co-educational college desires M.A. or Ph.D. to begin January 30 or September 6, 1961. Attractive salary and rapid advancement for the right man. P231